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PART FIRST.

Analytical and Critical Reviews.

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1. *Arctic Explorations in the Years* 1853, '54, '55. By Elisha Kent Kane, M.D., U.S.N. Two Vols.—*Philadelphia*, 1856. pp. 464, 467.
2. *Voyages of Discovery and Research within the Arctic Regions, from the Year* 1818 *to the Present Time.* By Sir John Barrow, Bart., F.R.S.—*London*, 1846. pp. 530.
3. *The Voyage of the* Fox *in the Arctic Seas: a Narrative of the Discovery of the Fate of Sir John Franklin and his Companions.* By Captain M'Clintock, R.N. With Maps and Illustrations.—*London*, 1860.
4. *The Voyage of Her Majesty's Discovery Ship* Resolute *to the Arctic Regions in Search of Sir John Franklin and the missing Crews of H.M. Ships* Erebus *and* Terror, 1852, 1853, 1854. By George F. M'Dougall, Master.—*London*, 1857. pp. 530.
5. *A Personal Narrative of the Discovery of the North-West Passage; with numerous Incidents of Travel and Adventure during nearly Five Years' Continuous Service in the Arctic Regions while in search of the Expedition under Sir John Franklin.* By Alexander Armstrong, M.D., R.N., late Surgeon and Naturalist of H.M.S. *Investigator.*—*London*, 1857. pp. 616.
6. *Northern Travel. Summer and Winter Pictures of Sweden, Lapland, and Norway.* By Bayard Taylor.—*London*, 1858. pp. 389.
7. *Voyage d'une Femme au Spitzberg.* Par Madame Leonie D'Aunet. Deuxième Édition.—*Paris*, 1855. pp. 363.

A Woman's Voyage to Spitzbergen. By Madame Leonie D'Aunet. Second Edition.—*Paris*, 1855.

THAT extravagant but once popular fiction, 'Le Juif Errant,' of M. Sue opens, it may be recollected, with a prologue representing the meeting of Herodias and the Jew. They meet where the Arctic Sea surrounds with a girdle of eternal ice the deserts of Siberia and of North America—the outer limits of two worlds, separated by Behring's Straits. Towards this desolate region of fogs and tempests, of famine and death, where the extreme cold cleaves the stones, and the earth brings forth spangled icicles, these beings have advanced to meet after coming from opposite extremities of the world, and traversing regions of burning cloudless sun. The intention of the author in this opening has clearly been to intimate that a supernatural element is admitted *de primo* into his work. Yet the *supernatural* attending this union exists, it must be confessed, rather in the secondary circumstances of the latter, than in the fact of two human beings, who may have sojourned respectively in the heats of Guinea and Guyana, indifferently existing within the arctic zone. So far as that circumstance goes, there are northern voyagers now living who have passed—and that rather abruptly, too—from one extreme of temperature to another. But it is not the less a wonderful endowment of the human organism, that it can not only support but enjoy life under such extremes as would soon be fatal to very many animals. Whether amidst deserts of burning sand, or upon seas of never-thawing ice, there is man to be found existing. We are struck with still more astonishment when we remember that a human being can be bred under one extreme of temperature, afterwards transported to another extreme, and then re-transplanted. Like Herodias and the Jew, they shall pass from Saharas of sun to Saharas of snow, and back again, with impunity. There is a "Fort William" and a "Fort Enterprise." The one is at Calcutta, the other is near Winter Lake. At the former, during April, May, and June, the thermometer is seldom under 86° at any hour, and in the afternoon it rises to 90° and 100° in-doors, and to 130° in the sun's rays. It is sometimes so intensely hot that soldiers upon parade will drop down dead, as if struck by lightning. On the other hand, at Fort Enterprise, the thermometer falls to 57° below zero; and here Franklin's instrument, when hanging in his sleeping place sixteen feet from the fire, and exposed to its direct radiation, stood more than once, even in the daytime, at 72° below the freezing point of water. Dr. Coulter observed the thermometer stand at 140° on the banks of the Rio Colorado (32° 30′ N.); whilst the heat rose to such a height in Abyssinia, when Major Harris travelled there as envoy, that "fifty pounds of well-packed spermaceti candles were so completely melted out of the box as to be reduced to a mere bundle of wicks."* As we now write, (March, 1860) letters, which have been received from H.M. ship (21) *Pearl* state, that on her voyage out to Singapore the thermometer rose to 120° Fah. when passing through the Straits of Banca. Yet under both such extremes of temperatures Englishmen can live, however uncertain may be the tenure of their lives. The races of men that Nature herself plants there have by their birthright a stronger

* Highlands of Æthiopia.

hold. But exist and enjoy existence as they do both at the Equator and the Pole, there is a wide difference as to the extent and intensity in which this is done at the two spots, not only in respect to man, but to the organized world generally. In the South, life is abundant and luxuriant on the grandest scale; animals or vegetables, it matters not which, are developed in their utmost glory. From sky to earth, from ocean to air, all is passion, life, brilliancy, and colour beneath the vivifying influences of tropic light and sun. At the North there is life, it is true, but after all it appears to us rather as a *struggle* than an enjoyment to live.

Though Nature seems to have intended certain races to inhabit certain regions, rather than to have made one race cosmopolite and capable of assimilation to all latitudes, yet she has granted a great flexibility of constitution to some favoured tribes of men. These latter are the races which are indigenous to latitudes intermediate between the Equator and the Poles—the members of the temperate zone. But although endurancy for a time under extreme degrees of heat or cold by those not natives of the climate in which they occur, is easily accomplished, a limit to the capability exists. Permanent acclimatization is impossible, either to the individual or to the tribe. The life of the former is of less value than it would be in his native country, and he cannot propagate his race. The assertion that any one race ever has been, or ever can be, assimilated to all climates, involves but an hypothesis not sustained by a single historical fact, and opposed to the teachings of natural history. There are fashionable theories afloat which oppose our doctrine. Theories which maintain that aliens may thrive in any clime as well as the autochthones, if they choose to conform to the modes of life of the latter. We appeal, however, to the history of the past and of the present, and ask if it does not show that the races of men can permanently maintain themselves and thrive only in those localities to which they appear originally to belong? Is the Spaniard thriving in South America, the Celt or the Saxon in the northern half? Is there true colonization in India? Does the Englishman flourish in the islands of the Gulf of Mexico? Could the Negro inhabit Lapland, or could the Northmen long flourish on the Senegal or Gambia? Is the Red man fitted for a large portion of the eastern hemisphere, and does the White man wax strong amidst the forests of the Far West? Is the standard of health as high among the natives of the Union as it is amongst their progenitors in the British Isles? To point to quarters of the globe at present peopled by races foreign to the land, and apparently flourishing commercially, as facts opposed to such doctrine, is to be open to the reply, that annually into these countries have been, and still are, imported thousands upon thousands of emigrants, representing some of the best blood of the colonizing stock. To be able to form a satisfactory conclusion, this constant replenishment must be arrested, and a sufficient length of time allowed to elapse, to enable us to see how the foreign race could then propagate and maintain itself in its adopted clime. We believe it would fail and gradually die out, and that the period would come, however distant, when the Saxon would no longer be found in Australia, in Kentucky, in Tennessee.

But although no permanent acclimatization of a race foreign to the soil can ensue, a temporary duration of the individual in most opposite climates is not only possible, but comparatively easy and enjoyable, if the person alien to the soil will be prudent and adopt the customs of the autochthones. Nevertheless, there are regions marked by such extreme characters of climate as would appear to utterly forbid any lengthened continuance there of alien people. What, e.g., is the chance of life to the Saxon and the Celt who settle in tropical Africa for 15° north and south of the equator? What is the prospect even to the hardy Norwegian who migrates to the northernmost settlements upon the shores of the Arctic Sea? Negroes upon the one hand, and Eskimos upon the other, are alone adapted to the spots; let other races try them, and at no very distant period death will be their reward. If it be asked in what manner certain races are specially adapted to particular climates, we can only make answer that they are born with just those particular constitutions which best resist the destroying environments; constitutions fittest to endure the particular intensities of solar heat and light prevailing there, and to be well able to subsist upon the kind of food common to the place. How *structurally* such men are specially formed for these particular adaptations we know not. Certain broad differential characters exist between race and race, but not any organic distinctions can be pointed out which would, *à priori*, show why one man can only flourish beneath a tropical sun, and another only amidst hyperborean snow. Yet there must surely be some great and fundamental vital differences, or else long before now the white man would have colonized India, and the Kaffirs of the Cape have been acclimatized in Ceylon. The necessity of an hereditary adaptation to climate is well seen in the fact of the impossibility of raising a hybrid race from an union of the alien with the autochthonic tribes. For a limited period a mixed breed is producible, which may appear to answer the purpose of colonization; but it cannot continue, perish it must in the certain, though it may be slow, reversion to one or other of its typical stocks, according to circumstances unnecessary now to mention.

Leaving out of view, for the present, some of the secondary elements of "climate," we may draw attention to such of its primary ones which appear to necessitate a particular organic adaptivity. These are the intensities of solar heat and light, and the nature of the food proper to the locality. With respect to the former, it is important to remember, also, that high intensities of heat not only require a special constitution to bear their direct influence, but one also congenitally adapted to resist the effects which follow their action upon dead organic matter when such action is combined with a certain degree of moisture. Hence do we find the Negro both enjoying the rays of an equatorial sun, and continuing proof against the emanations of the swamps of Benin. Speaking of a certain locality in Africa, Lieutenant Burton remarks,

"How these 'Kurrahjog,' or 'Sun-dwellers,' as the Bedouins are called by the Burgher Somal, can exist here in summer is a mystery. My arms were peeled, even in the month of December; and my companions, panting with the heat like the Atlantes of Herodotus, poured forth reproaches upon the rising sun. The town's people, when forced to hurry across it in the hotter season, cover themselves during the day with tobes wetted every half-hour in sea water."*

Upon the other hand, to live in snow huts, and to subsist upon walrus and seal flesh, where mercury is frozen for months together, and the sun is invisible for a quarter of a year, are feats to be accomplished only by a few tribes for a constancy. They alone can continue to endure under such peculiar hygienic environments as the negation of heat and light brings in its train. We have remarked, however, that it is possible for some favoured races to live for a *limited time* alternately in such extremes of climate; and there are, of course, gradations between them, which may be occupied for longer periods by men not of the autochthonic stock. In places known to be most inimical to certain races, individuals of the latter have yet been found to live healthy and to enjoy life for a long time when careful and prudent to accord with the manners of the natives. Even in the port of the Rio Formosa, the most deadly portion of the Bight of Benin, Europeans have resided in good health for some years.† We all know that within the usual places of resort of Europeans within the tropics, the aliens can continue to reside with comparative impunity, if moderate caution be adopted. But this is all, whether it be the Rio Formosa or the Rio Colorado, Ashantee or Madras, whether it be Bengal or Jamaica, Cape Town or Canada, Hispaniola, Chili, Cuba, or Peru, no true European stock can *permanently colonise* the place. We have held India for more than two hundred years, yet we cannot colonise an inch of it. We have planted the white man in America, and there he degenerates! The ability of the *individual*, however, to live in countries very opposite in character to his own birth-place being well known, and great stress being laid upon this power of supporting climatorial extremes being almost indefinitely augmentable by courage, caution, and ingenious devices, some strange ideas have occasionally taken hold of the public. Some few years back an expedition was fitted out by Government, at the enormous cost of between 80,000*l.* to 100,000*l.*, which was to ascend to the Niger and Chadda confluence, by way of the Quorra, where, to use the words of an official report, "will probably be the head-quarters of the expedition for some time. . . Where an opportunity will be afforded for showing the Africans the best mode of cultivating the ground, and of distributing plants and seeds suited to the climate and soil." Three weeks after the steamers entered the river (the temperature being seldom under 84° on the lower deck), fever broke out among the crews, and soon produced such effects as to compel two out of the three vessels to return to the sea, and to oblige the other boat to follow them a few weeks afterwards. Now,

* An Exploration of Harar.

† See Daniell's Sketches of the Medical Topography and Native Diseases of the Gulf of Guinea, Western Africa, &c.

there are still people who believe that remnants of the crews of the Franklin arctic expedition, which left these shores fifteen years ago, may yet be existing within the barriers of the arctic circle, or upon the polar lands of the American continent. Around this belief range the important and interesting questions as to our experience concerning the influences which the particular relations of solar heat and light, and food, &c., pertaining to the northern regions, exert upon the systems of those who are not natives of the climates in which they occur. As our knowledge upon this subject has been lately much increased, the present time is not inopportune for us to lay before our readers a sketch of one of the most interesting biological questions of the day.

The most northerly *cultivated* spot upon the globe is in Norway (Valley of the Alten), just above 70° N., where, in good seasons, tolerable crops of potatoes, barley, and buckwheat may occasionally be obtained. This favoured locality is nearly upon the same parallel with Disco Island in West Greenland, and Icy Cape (above Behring's Straits) in North America, where certain secondary climatorial elements necessitate complete barrenness. Several settlements of civilized people exist beyond this degree both in Norway and Greenland. In the former we may instance Hammerfest and Kjevik; the latter being upon the parallel of 71° N., was visited (and described) by Von Buch in the summer of 1807, just fifty years before Mr. Taylor passed the spot in his trip round the most northerly point of Norway. According to Mr. Taylor, Von Buch's description is equally correct for the present day. It is a place "where scurvy carries off half the inhabitants, where pastors coming from Southern Norway die within a year, where no trees grow, no vegetable comes to maturity, and gales from every quarter of the icy sea beat the last faint life out of nature, yet where men will still persist in living in apparent defiance of all natural laws." (p. 264.) More northerly still are the Danish settlements of Upper Nivik (72° 50′ N.) and Yotlik (73° 40′ N.) in West Greenland. These last civilized haunts are nearly upon the same parallel with the native Eskimo village—Kapawroktolik—near Pond's Bay in Cockburn Land, and which was lately visited by Captain M'Clintock. Beyond these high latitudes a few snow bone-huts, and skin-tent settlements of native Eskimos are alone to be met with. Of these we may mention the tribe at Cape York, West Greenland (76° N.), which is so completely isolated, that we are told by an Arctic voyager that previous to its being visited in 1818 its members considered themselves to be the only people in the world. But the late explorations of Dr. Kane have made us acquainted with the habitats of still more northerly Eskimos. These are situated near the bottom of Smith's Sound, where, a little above 78° N., are to be found the settlements of Etah and Peteravik. Not any Eskimos have been met with more northerly than this, though Kane observed the remains of uninhabited huts and of "caches" at Dallas Bay (79° N.); while Mr. Morton, of the same expedition, found a well-formed sledge-runner made of whale's bone upon the shores of Morris Bay, as high as 81° N. latitude. But beyond this extreme enterprising travellers have placed their feet. The farthest northing* which has hitherto been attained was reached by Parry, across the ice, upon the 23rd of July, 1827, when he advanced to 82° 45′ on the meridian of 19° 25′ E. Mr. Morton reached the high parallel of 81° 2′ N. on the meridian of 65° 35′ W., from whence he sighted the most remote northern land known, Mount Edward Parry, in 82° 38″ 66‴; while Mr. Scoresby is credited by some with having (in the year 1806) reached 81° 12′ 42″ N. by observation, and 81° 30′ by dead reckoning. Between the Arctic circle (66° 30″) and 83° N. may be said to lie the range of hitherto effected exploration. Within it, towards its lower limit, are of course situated the more northerly settlements of Upper Norway, the Danish depôts in West Greenland, and the Russian establishments in Eastern Siberia (Oust Jansk, 70° 55″ N., e.g.). These and like places we here leave out of consideration, simply referring to Arctic explorers who have had to shift for themselves, away from all civilized settlements, and exposed to the severe contingencies of wintering in the ice, or upon the shores of or upon the islands of the Arctic Sea.

With these limitations, the longest and most northerly stay of which we have authentic records is that of some Russian sailors, who were left behind at Spitzbergen (between 77° and 80° N.) for six years. Four remained alive at this time, the others having begun to die towards the end of the first winter. Since then (1743) the Russians have, up to a comparatively recent period, frequently wintered their summer fishing vessels in certain safe harbours amongst the islands. Madame D'Aunet informs us, however, that the Spitzbergen fishing grounds are now almost abandoned both by Russians and Norwegians.† Previous to this time, however, both the English and Dutch had resorted to the island, and some sailors being left upon its shores on several occasions, were forced to pass the winter there. In some instances they died from scurvy, but in others they survived and returned home next season.‡ Parry, in his second voyage, passed two winters in the Arctic clime. Sir John Ross wintered three whole seasons, besides passing nine months of the spring and summer there; while the crew of the *Investigator* (M'Clure) landed from the *North Star* off Ramsgate in October, 1854, after an absence of four years and ten months from England, four years of which we may accept as passed within an icy zone. Leaving the Spitzbergen Russian sailors out of the question, Dr. Kane has wintered more northerly than any other Arctic explorer. He passed two winters within Smith's Sound, in Renssalaer Harbour (78° 39″ N.). To Madame D'Aunet belongs, we believe, the honour of having been the first lady who has penetrated so near to the Pole as 80° N. She accompanied her husband in the voyage of M. Gaimard (of the discovery vessel *Recherche*) to explore the Frozen Ocean near the stations of Spitzbergen and Greenland. Thus far, then, and for

* We pass by the fabulous "Northings," published by Daines Barrington, and which may be found by the curious in Shillinglaw's 'Arctic Discovery.'

† What were the nature of the depôts formed by the Russians in Spitzbergen we cannot say exactly, but we are told, in an article in the Edinburgh Review for Oct. 1853, that "M. Sharosten, a Russian, has passed thirty-nine winters in Spitzbergen, and resided there once for fifteen years without quitting the island!" Is there not some mistake here?

‡ For an account of some attempts at living upon Jan Mayen and Spitzbergen, see Lord Dufferin's "Letters from High Latitudes." London, 1857.

only a very few years, are we sure of what may occasionally be accomplished by picked and hardy voyagers. As we proceed it will be found against what an amount and how terrible a kind of inimical environments they have battled and withstood, and how many of their enterprising companions died instead of being able to uphold against them. In this problem of Polar life the chief factors which appear to be so antagonistic to the continued existence of all but the autochthones are, in the first place, the difficulty of procuring sufficient food of any kind, but particularly of that which men of the temperate zone seem so urgently to require—viz., a certain admixture of fresh meat and fresh vegetables. Secondly, the low temperature which prevails, especially during the winter period. Thirdly, the long-continued darkness of the Polar winter, when the sun is absent for a quarter of a year. Fourthly, the deleterious results arising, upon the one hand, from exposure to great alternations of temperature caused by the necessary artificial heating of the cabins, huts, tents, &c., and the transit from these to the external air; and, on the other hand, from the moisture, effluvia, &c., resulting from the close packing for warmth and other necessitous demands. Lastly the mental depression and *ennui* which creep over the sojourners in so inhospitable and dreary a clime. We shall proceed to examine some of these opposing elements in detail, and will refer to the question of *food* in the first place.

It would appear to be a law of the economy of the races of the temperate zone, that for the maintenance of health there must be a proportion of fresh meat and of fresh vegetables in their diet. So long as there is a due quantity of the latter it will suffice, along with a certain proportion of milk, eggs, and oily matter, in affording a diet sufficient for most conditions. Nevertheless, the natural desire is for animal flesh of some kind, and upon such a mixed diet the races of Europe appear to flourish best. The more northerly they live the larger is the proportion of "flesh, fish, or fowl" to the vegetable matter, and the more southerly the reverse. But yet, under all circumstances, a certain quantity of fresh vegetable food is requisite; if this be not obtainable a vital deterioration will soon infallibly ensue. To the tribes of the Arctic zone, and of the extreme north of the temperate region, this admixture of vegetable matter does not appear essential; on the contrary, it would seem that a purely animal or flesh diet is the more natural. Both theory and experience establish this fact. It is now generally admitted that every change in the organic constituents of the body in which their elements enter into new combinations with oxygen, must be a source of the development of heat. Such is particularly well seen taking place during respiration, in which a considerable part of the carbonic acid and water exhaled during its performance is formed within the body by the metamorphosis of its own tissues. When the temperature of the air approaches within a few degrees that of the body, the generation of animal heat by the burning of organic matter in the blood may be reduced in amount; but when it is far less, when the cooling process of the body from the surrounding low temperature is perilously great, then no mere combustion of the hydro-carbonaceous portion of the "waste" of the tissues, nor even that of the ordinary calorifiant articles of food, will suffice. Either a store previously laid up in the body, or the constant magazine of a most heat-generating food, must be resorted to. For the clear exposition of this truth (first taught by Collier, see this Review, vol. iv. p. 508) we are indebted to Liebig, who has shown that such store and such food are constituted *par excellence* by *fat*. That whilst 240 parts of starch, 249 of cane sugar, 263 of grape sugar, 266 of "spirits," and no less than 770 of lean meat must be consumed as food to keep the body at its proper temperature during equal times with the like amount of oxygen, the same thing is effected by 100 parts of fat. In proof of the theory, said Liebig, look to facts. The more northerly we go, the more of this, the very best combustive material, is used; the more southerly, the less! In the latter region the cooling process is *nil*, the waste of the body by combustion next to nothing, and rice, fruit, and vegetables are sufficient as food. In the former the air is cold enough to freeze mercury, the cooling process is extreme, and the waste by combustion excessive indeed. Here the blubber and fat of the cetacea and other animals alone enable the body to bear up against the intense cold. This doctrine has been widely impugned, both as regards its theoretic aspect and the assumed facts brought forward to support it. No doubt there are some difficulties, both theoretic and practical, which have not as yet been satisfactorily explained, but in spite of these and some exceptional circumstances the fact holds good—viz., the more northerly we go the more fatty matter we find eaten; the more southerly, the less is employed. We find this to be the case not only with all the nations of Mongolian descent which inhabit the shores of the Arctic Sea, but also with the Indian races who wander over the coldest portions of the North American Continent; with the Canadians and half-breeds who adopt the same life, and with the European traders whose stations are in the same latitudes. Mr. Taylor tells us to

"Eat the fattest food, and plenty of it . . . . . Braisted and I consumed about a pound of butter between us. This intense cold begets a necessity for fat, and with the necessity comes the taste—a wise provision of nature. The consciousness now dawned on me that I might be able to relish train oil and tallow candles before we had done with Lapland." (p. 40.)

A writer in a late number (No. 57) of the 'Dublin Quarterly Journal of Medicine' makes this observation:

"With regard to the influence of temperature, I am assured by Sir James Ross that no large increase of fat is necessary to our sailors in the Arctic regions; and by Professor Sandhal of Sweden, that the peasantry of Lapland do not take food materially differing from that of our own country."

Such may possibly be the case—though we doubt it—and yet without the general law we have laid down being much compromised. We would remark, however, *en passant*, that according to Dr. Armstrong (p. 468) the amount of animal food allowed daily to the men of the service is insufficient, and that food possessing the most highly carbonised qualities, such as pemmican, preserved meats, and bacon, should alone be supplied to Polar expeditions (p. 469); while Madame D'Aunet informs us that the repast of the Lapps of Kautokeino, of which she several times partook, was "composed of fish, flesh, reindeer milk, the whole well soaked in fish oil." (259.) Not only, then, is the kind of food—viz., the flesh of bear, walrus, seal, musk ox, and reindeer, with fish and fowl of any kind procurable, with its oil, fat and blubber, of a special character; but the amount of it required, or at least consumed, by the autochthones appears enormous. Kane estimates the average Eskimo ration—in a season of plenty—at eight or ten pounds of flesh, with soup and water to the extent of half a gallon. Another of our Arctic commanders having allowed a young Eskimo to eat as much as he chose, the latter consumed in twenty-four hours thirty-five pounds of various kinds of fat meat, including some tallow candles. An amusing account of a baby gourmand will be found in Dr. Kane's first vol., p. 419. The rapacity of the Arctic dogs is remarkable; they eat everything eatable which they can get hold of—an Arctic wag has said, even to a feather bed. Captain M'Clintock's twenty-nine dogs devoured from sixty to sixty-five pounds of seal's flesh in forty-two seconds! The European of the temperate zone having been transported to the Arctic circle, finds that not only is a full diet largely composed of fresh fat animal flesh absolutely necessary, but that he cannot consume the salted provisions with the same impunity which he can in more genial regions. This is no doubt partly due to their not being antagonized by fresh vegetable juices, but partly also to the fact that salted meats are, to a certain extent, actually poisonous within the Polar circle. Kane more than once experienced that a meal of salted food killed the dogs who ate it; and Dr. Armstrong expressly states that "Salt meat should be proscribed from an Arctic dietary." (p. 469.) Hence the Arctic explorer must look *mainly* to the diet which supports the Eskimo—fresh fat animal food. It has hitherto been held that the supply of this, or rather the power of actually getting possession of it in the north, is by no means considerable, that it is very unequally distributed, and to procure that portion of it represented by the walrus and seal requires the knowledge, habits, and dexterity of the native hunters. It was hence concluded that, from the difficulty of procuring a proper diet, and plenty of it, no large party of Europeans could live long in the lands of the Eskimos. But in recent times the theory has been broached that a more temperate climate, a richer vegetation, and an abundance of animal life, are found increasing as the voyager approaches the Pole.* At first there appears, it must be admitted, some fair data for such belief, but a more general and unprejudiced inquiry does not, in our opinion, tend to support this fashionable creed. In favour of it may be stated that Parry, in his first voyage, met with successful hunting in Melville Island; and the *Resolute*, from 1852 to 1854, obtained at

* We pass over the consideration of the allied question as to the existence of a "polar basin" or "polynia," as being one of purely a geographic kind. Those who may be desirous of looking into this subject of great interest, may be referred for arguments *pro* and *con* to Kane's "Narrative," vol. i. pp. 301–309; vol. ii. pp. 309, 429; to an article in the Edinburgh Review for Oct. 1853, pp. 348–352; and to the subject of "Polar Regions," in the new edition of the Encyclopædia Britannica, vol. xviii. p. 178. "An Arctic Boat Journey in the Autumn of 1854," by Isaac J. Hayes, Surgeon of the second Grinnell expedition (London, 1860), may also be referred to with advantage.

the same place not less than 114 musk oxen, &c., giving a total of 27,433 pounds, or 305 pounds per man, 148 of which were actually issued on board the ship. Captain Penny, on his return to England in 1851, reported that animal life existed in great abundance well to the northward in Wellington Channel; Dr. Kane and Captain Sherard Osborne afterwards confirmed this statement. These and analogous representations led at last to the most extravagant ideas concerning the richness of a Polar fauna and flora yet to be found. Whilst, e.g., the *Resolute* was fitting out, Mr. M'Dougall overheard a talented but eccentric Scotchman, connected with a public institution, inform one of the officers that—

"During the summer and autumnal months you will find the shores of North Devon abounding in a rich vegetation, which forms the food on which the numerous animals in that locality exist. There, interspersed with gay mosses, you will find brilliant lichens and luxuriant saxifrages, with the Arctic poppy." (p. 282.)

Now, if we ask the sojourners on board the *North Star* at Beechey Island to confirm or to confute this glowing description, we find that the numerous animals have dwindled down to an occasional solitary bear; and the mosses and lichens, though truly present, are so in far too scanty a degree to remind us of the Scotchman's paradise. In connexion also with the statements of Penny, Kane, and others, concerning North Wellington Channel, it must be borne in mind that Sir Edward Belcher, having unfortunately to spend a winter in the very locality thus pointed out as the veritable land of promise, did not obtain, it is said, so much as a *single bird.* That at Melville Island good hunting has been obtained there can be no doubt, for during the first week in July three shooting parties (of five officers and ten men), hunting thirty miles of coast line, six or eight miles in breadth, produced a quantity of game exceeding, writes one of the officers, "our most sanguine hopes, quite sufficient, indeed, to confirm the many *savants* in England in their theories respecting the abundance and variety of animal life in these regions." (M'Dougall, p. 276.)

But it must be remembered that Melville Island is the *favourite resort* of animals, in preference to the lands of Cornwall, Bathurst, and North Devon, including the southern shores of Barrow's Straits. The larger animals—musk oxen and deer—seldom, if ever, visit any other land bordering upon Barrow's Straits, save Melville Island. This is proved by events in the voyage of Sir James Ross (Port Leopold, 1848–49), in that of Captain Austin (Griffith's Island) in 1850, and by the still more prominent fact (1849), that while the crews of the *Resolute* and of the *Intrepid* were living upon fresh meat of every kind, their co-operators in the search (Sir E. Belcher in Northumberland Sound, and Commander Pullen at Beechey Island) were scarcely able to obtain a pound of edible meat (M'Dougall). Further, it must not be forgotten that Melville Island forms but a small portion of the Arctic regions, and some places like King William's Island and Smith's Sound, beside those alluded to, are known to be barren of life in the extreme. True it is that we find the following entry in Dr. Kane's diary—

"*December 9th, Saturday.*—The superabundant life of Northumberland Island has impressed Petersen as much as it did me; I cannot think of it without recurring to the fortunes of Franklin's party." (vol. i. p. 437.)

But it is equally true that the hardy adventurer who wrote this was a few weeks afterwards keeping himself from starving a few degrees more northerly by eating the rats which his own vessel had conveyed from America, and entered the following in his book a mouth later—

"*January 17th, Wednesday.*—There is no evading it any longer; it has been evident, for the past nine days, that the present state of things cannot last. We require meat, and cannot get along without it. Our sick have finished the bear's head, and are now eating the condemned abscessed liver of the animal, including some articles that were not given to the dogs." (vol. ii. p. 17.)

Near to this horrible locality the Eskimos of Etah themselves having been half-starved during the winter of 1854, had been obliged to kill their dogs, so that out of thirty but four remained, the others having been eaten.

Again, we have to learn how long such favoured spots as Melville Island and Northumberland Island would continue to be the resort of animals and birds, if the sound of the rifle became very frequent and continuous.* It has been urged, that the Eskimos often starve because they are improvident; that they do not store up a winter stock, but live gluttonously from hand to mouth, and that such things would be improved upon by more civilized people. But though there is improvidence, it is true, and not much laying by for a rainy day, there are some obstacles to storing up which are well to remember. In the first place, the apparently necessary enormous consumption by individuals, and the right of poorly fed tribes to demand help from more fortunate ones, stand greatly in the way of making depôts for the future. In the second place, it would appear that certain kinds of meat are far more liable to putrefy and become tainted in the colder regions than is commonly supposed. Dr. Kane writes—

"*February 24th, Saturday.*—A bitter disappointment met us at our evening meal. The flesh of our deer was nearly uneatable from putrefaction; the liver and intestines, from which I had expected so much, utterly so. The rapidity of such a change as low as — 35° seems curious, but the Greenlanders say that extreme cold is rather a promoter than otherwise of the putrefactive process. All the graminivorous animals have the same tendency, as is well known to the butchers. Our buffalo hunters, when they condescend to clean a carcase, do it at once; they have told me that the musk ox is sometimes tainted after five minutes' exposure. The Eskimos, with whom there is no fastidious sensibility of palate, are in the practice at Yotlik, in lat. 73° 40′ N., even in the severest weather, of withdrawing the viscera immediately after death, and filling the cavity with stones." (vol. ii. p. 5.)

But even admitting, for the sake of the argument, that a sufficiency of fresh animal food was procurable and preservable, we should still think it unlikely that any but the autochthones could continue to live upon it alone. Dr. Kane would appear to think otherwise; at least, he affirms that fresh raw meat alone is not only the proper but a suffi-

* See, upon this point, "A Journal of a Boat Voyage through Rupert's Land and the Arctic Sea," &c., by Sir John Richardson. London, 1851. Vol. i. p. 304.

cient food for all—"Had we plenty of frozen walrus I would laugh at the scurvy." (vol. i. p. 437.) We are inclined to believe that the European would require some fresh vegetable material, and that to an amount vastly more considerable than the "scurvy grass" and "sorrel" of the northern regions could supply. But we will appeal to Mr. M'Dougall—

"I am induced," says he, "to believe, that a small body of men well armed, with health and strength sufficient to enable them to range the hills around to a distance of ten or twelve miles from the ship, might, if only animal food were required, sustain life for a limited period on the product of the chase." . . . "I say, for a limited time, for we have now before our eyes, in the crew of the *Investigator*, living proofs of the injurious effects of a prolonged stay in these regions on the mental as well as the bodily capabilities of the human frame. Game was not wanting in the Bay of Mercy, and during their first winter there, when the crew were first placed on a limited scale of provisions, numerous deer, hares, ptarmigan, &c., were procured; but with the approach of the second winter in the Bay (the third within the Arctic circle), an apathetic indifference pervaded the men. With their strength, which had decreased with the continuance of the small allowance of provisions, all their energy seemed to forsake them, and as symptoms of scurvy were manifested in the system they lost all animation, and allowed a feeling of lethargy to master them which utterly precluded any hope of success in hunting. The small quantity of game that was procured during the winter of 1852–53, is to be chiefly attributed to the energy of the officers, all of whom were of opinion that another winter would have reduced them so much as to prevent them going any distance from the ship in search of game, which had now become very scarce in the immediate vicinity of their winter quarters. The above facts in my opinion tend to prove that even amidst comparative plenty, so far as animal life is concerned, the approach of a third season brings with it a deep depression of spirits, which few minds are strong enough to bear up against, more particularly when scurvy—one of the most dreadful diseases peculiar to seamen, and God knows they are subject to many—appears amongst them. The blood becomes stagnant, teeth loosened, gums and palate black and sore, flesh softened, all animation ceases, and with the sun, as he sinks beneath the horizon, leaving the dark and gloomy night of three months' duration to usurp his throne, the last ray of hope departs." (p. 277.)

To these remarks of Mr. M'Dougall it might be replied that the crew of the *Investigator*, like all other crews, had never half enough of fresh fat meat to eat, if we may take as a guide the practice of the Eskimos. But there is an answer to this—viz., that scurvy attacks the Moravian settlements in Greenland, and some of the places upon the shores of the Arctic Sea, where, we may presume, as much *animal* and fish food as is wanted is procurable.* If we apply these considerations regarding sustenance to Franklin's party, we may probably be met by the rejoinder that the remains of the expedition must have reached the continent of America, since traces of them have been discovered as far south as the Whirlpool Rapids of the Great Fish River, almost without the limit of the Arctic circle. So far as the present question is concerned, this, however, is of little moment. Franklin's party landed upon one of the most desolate and barren islands of the icy seas—an island, according to Captain M'Clintock, almost destitute of vegetation and of animal life—and from thence it

* See Kane, vol. i. p. 27.

proceeded to a district so destitute of all means of support as not to be visited by Eskimos. What the northern districts of the American continent offer in the way of means for subsistence, the journeys of Franklin, Richardson, Rae, Anderson, and Back have sufficiently made known to us. Let any persons read the narratives of these voyagers, and then say upon what they expect any native of the British islands could continue to exist for several years eastward of the Mackenzie River and north of the Bear Lake. The very party sent to relieve Sir John Ross saved themselves from starvation by consuming the food intended for that officer long before they reached the half-way house to him (King).

"Leaving two men," writes Dr. Rae, in 1847, "in a snow hut, in lat. 68° 48′ N., long. 85° 4′ W., to endeavour to fish and shoot . . . . the men we had left here were well but very thin, as they had neither caught nor shot anything except two marmots. Had we been absent twelve hours more they were to have cooked a piece of parchment skin for supper."

Mr. Isbister tells us that only a few years back one half of the Hare tribe of Indians perished around Fort Good Hope, after having killed and eaten two of the Hudson's Bay Company's people who imprudently ventured beyond the gates.* A party numbering not more than ten individuals stationed upon the borders of the largest and most productive lake in this part of the world, and aided by all the resources of the Hudson's Bay Company, barely contrived to subsist upon half rations through the winter, while of the unfortunate natives attracted towards Fort Reliance by the presence of the whites, from forty to fifty lay dead around the place, and so scattered, that it was impossible to walk in any direction for twenty miles without stumbling over a frozen body. But Captain M'Clintock puts this matter in a very clear light:

"There are two important questions which have been so frequently put to me, that I gladly take this opportunity to offer some explanation upon so deeply an interesting subject. The first question is, whether some of the 105 survivors may not be living among the Eskimos? The various families or communities of Eskimos met with by Rae, Anderson, and myself at different times and places, all agree in saying, 'No, they all died.' But let us examine for ourselves. The western shores of King William's Island, along which they were compelled to travel for two-thirds of their route, is uninhabited . . . . None of us have met natives at the south of the Back River, consequently it is fair to conclude that the Eskimos but seldom resort to so inhospitable a locality. Even much more favoured shores in this vicinity are but very thinly sprinkled with inhabitants, and their whole time is occupied in providing a scanty subsistence for themselves. In fact, their life is spent in a struggle for existence, and depends mainly upon their skill in taking seals during the winter, a matter which requires such long training that no European has ever yet succeeded in acquiring it. My two Greenland Eskimo men tried various methods at Bellot Strait, yet did not succeed; and without dogs trained to scout out the small breathing holes of the seals through the ice, and through the snow which overlays the ice, I do not think even the Boothian Eskimos could live. . . . It is evidently an error to suppose that where an Eskimo can

* See also "Kitschi Gami; or, Wanderings round Lake Superior." By J. G. Kohl. London, 1860.

live a civilized man can live also. Eskimo habits are so entirely different from those of all other people, that I believe there is no instance on record of either a white man or an Indian becoming domesticated amongst them. . . . . With regard to the probability of procuring the means of subsistence independently of the Eskimos, I will just state what was shot by my own sledge party—and we never lost a chance of shooting anything—during the journey along the lands in question that occupied us for seventy-nine days, and covered nearly 1000 geographic miles of distance. The sum total amounted to two reindeer, one hare, seventeen willow grouse, and three gulls." *

We are fully aware that the barrenness of the country near the Great Fish River has been lately and strenuously denied. We have been told of this stream as "flowing through a country so abounding with animals," and of its shores as luxuriating (upon its sandhills) in flocks of reindeer composed of between one and two thousand members. The geese were once at least "so numerous as to have left behind cartloads of quills." The musk oxen existed by thousands, the nets under the ice constantly yielded salmon, the locality, indeed, being then a sort of Arctic Arcadia. There is such a thing as proving too much.

The second difficulty which has to be borne up against near to and within the Arctic circle, is the *intense degree of cold* that is there met with —a cold so extreme that "bacon becomes like slabs of granite, requiring several days' proximity to a Sylvester stove to fit it for the process of boiling." The lowest degree of natural cold hitherto recorded is – 70°, or 102° below the freezing point of water, or 30° below that of quicksilver. This was observed by Back on the 7th of January, 1833, upon the continent of America at Fort Reliance, near the Slave Lake. The same low temperature was experienced by Kane in lat. 78° 37′ N., long. 70° 40′ W., in February, 1854, where the mean of eight instruments gave – 70°. Chloroform then froze, the essential oils of sassafras, juniper, cubebs, and of winter green were resolved into a mixed solid and liquid, and chloric ether was seen congealed by a natural temperature for the first time. Franklin, upon the continent of America, saw the thermometer sink to – 57°. Captain M'Clure noted the low degree of – 67° in January, 1853. Sir James Ross, in his Boothian voyage, experienced a temperature of – 60°; whilst Mr. Taylor, in Lapland (at the top of the Gulf of Bothnia), recorded a fall as low as – 50°, or 82° lower than the freezing point of water. At Nishni Kolymsk, in Siberia (68° 31′ N.), the mean annual temperature of the winter is only – 19° 03′, and the thermometer sinks in the month of January to *minus* 64; whilst at Yakutsk the Russian-American company, we are told,† bored for water to the depth of 380 feet, and the ground was still frozen, and no water to be obtained. In reflecting upon these extremely low degrees of temperature, we are bound to remind the reader how merely *approximative* only is their determination, and that in arriving at it, sources of disturbance exist which are unfelt when dealing with degrees higher in the thermometric scale. In Kane's expedition it was not uncommon for instruments which

* Report of Capt. M'Clintock, in a paper read before the Geographical Society, Nov. 1857.
† Revelations of Siberia. Edited by Colonel Lach Zyrma, vol. i. p. 148.

had afforded correct and agreeing measurements to as low a temperature as – 40°, to show at – 60° differences of from 15° to 20°. For example, on the 4th of February, 1853, six thermometers suspended near each other gave at noon – 71°, – 63°, – 54°, – 53°, – 50°, – 50°, whilst all at temperatures above – 40° agreed within 1° 8′. These instruments had been nevertheless tested and selected in America as the most consistent of a lot of thirty-six. Such was the case also with the well-constructed instruments of Sir James Ross at Leopold Harbour. The sources of these difficulties and differences appear to lie, upon the one hand, in certain mechanical errors of construction, and, upon the other hand, in physical perturbations arising from changes in the surrounding conditions. The mere approach, e. g., of the observer will cause a perceptible rise in the column when it stands at from *minus* 45° to – 50° (Kane). The contiguity of articles brought by the observer from a warmer apartment, the use of the lanthorn, and adjacency to or distance from the vessel, will always sensibly affect the indications given. Further, it has been suggested by Dr. Kane that the errors are partly due to our imperfect knowledge of the laws of the contraction of coloured alcohol at very low temperatures. The actual freezing point of mercury is therefore not settled. It varied with Kane between – 38° 5′ and – 41° 50′, while in its rate of contraction it went to – 44°, and with Sir E. Belcher to as low as – 46°.

In describing the effects of intense cold upon the frame, Mr. Taylor writes:

"It was a wonderful, a fairy world we beheld, too beautiful to be lifeless, but every face we met reminded us the more that this was the chill beauty of death—of dead nature. Death was in the sparkling air, in the jewelled trees, in the spotless snow. Take off your mitten, and his hand will grasp yours like a vice; uncover your mouth, and your frozen lips will soon acknowledge his kiss. Even while I looked, the same icy chills were running through my blood, precursors of that drowsy torpor which I was so anxious to avoid. But, no, it would come, and I dozed until both hands became so stiff that it was barely possible to restore their power of motion and feeling." (p. 148.)

When the "face-protectors" are badly made, the features will become frost-bitten, the breath solid, and even congelation of the eyelids from the action of the intense cold upon the moisture exhaling from the eyes, may ensue. It is then necessary to withdraw the warm hands from the mittens, and apply them to the eyes in order to melt the ice and restore vision. Mr. Taylor continues:

"Most of our physical sensations are relative, and the mere enumeration of so many degrees of heat or cold gives only an imperfect idea of their effect upon the system. I should have frozen at home in a temperature which I found very comfortable in Lapland, with my solid diet of meat and butter, and my garments of rein-deer. The following is a correct scale of the physical effects of cold calculated for the latitude of 65° 67′ N.:—15° *above zero*, unpleasantly warm; *zero*, mild and agreeable; 10° *below zero*, pleasantly fresh and bracing; 20° *below zero*, sharp, but not severely cold—keep your fingers and toes in motion, and rub your nose occasionally; 30° *below zero*, very cold—take particular care of your nose and extremities; 40° *below zero*, intensely cold—keep awake at all hazard, muffle up to the eyes, and test your circulation frequently, that it may not stop somewhere before you know it; 50° *below zero*, a struggle for life."

A sterner apprenticeship than Mr. Taylor's leads to a struggle beyond these degrees, as the following extracts from Dr. Armstrong will show:

"The frost collected on the beard and blankets at night, which not unfrequently attached them to each other. During the day I was often unable to write, from the ink freezing in my pen; and water or lime-juice kept standing on the ship's table became immediately frozen. . . . One is obliged to keep almost continually in motion to resist its effects; . . . 51° below zero causing incessant frost-bites on the least exposure to the light wind which blew. . . . My right hand was so severely bitten, and the mischief spread with so much rapidity, that on reaching the ship it was a stiff frozen mass. I had not the slightest ability to bend it; and on plunging it into a basin of cold water, a thin film of ice formed on the surface. I lost the use of it for a period of two months, and was for a time apprehensive of its safety. . . . This low degree of cold [61°·6 below zero], however, did not deter us from our usual pursuits; and on the 6th two deer were shot, and the day following a few ptarmigan; such was the value attached to them, that we considered ourselves well rewarded for what we had endured." (p. 554.)

At these extreme temperatures to touch any metallic substance with the naked hand in the open air is highly distressing, taking off the skin whilst producing the sensation of intense heat. In one of Sir James Ross's expeditions some foxes were trapped, and we are told that "the poor little animals, in attempting to escape, tried to gnaw the iron bars, when in many cases their tongues adhered to the iron, and were frozen off, when they were killed from motives of humanity." Dr. Kane, if we recollect aright, somewhere recounts a similar anecdote. A strong tendency to tonic spasm, probably produced by the lengthened and extreme cold, was one of the principal trials of Kane's party. This anomalous form of spasmodic disease, giving rise to epileptiform and tetanic seizures, was encountered medically with great difficulty. It extended to the dogs, and in spite of every effort not less than fifty-seven perished, many of them with symptoms not unlike those of hydrophobia. Under somewhat less intense cold, however, human necessities and perseverance are the parents of the almost incredible. The pastor Hvorliff, at Kautokeino, in Lapland, told Mr. Taylor that he had frequently preached at a temperature 35° below zero; "at such times," said he, "the very words seem to freeze from my lips, and fall upon the heads of my hearers like a shower of snow." As the mind is much influenced by the state of the body, it was not an inappropriate question of Mr. Taylor to the pastor, as to whether, under such circumstances, religion itself might not be somewhat at a reduced temperature. "Yes," was the reply; "there is not a doubt that all the better feelings become very faint when the mercury begins to freeze." Madame d'Aunet assures us that all the virtues of the Lapps are negatives, and that—

"Their softness is effeminacy; their chastity, coldness; their honesty, indifference. . . . They are, in fine, a miserable and rude race, vegetating under a sort of moral and physical lethargy, and well adapted for peopling this frozen extremity of the world, from where all life disappears with the sun." (p. 150).

Mr. Taylor appears to confirm the lady's account of the Laplanders —e.g.:

"The inhabitants of the north also seem to undergo a species of hybernation, as well as the animals. Nearly half their time is passed in sleep, they are silent in company or in comparison with the natives of other parts of the world; there is little exuberant gaiety and cheerfulness, but patience, indifference, apathy almost. Aspects of nature which appear to be hostile to man, often develop and bring into play his best energies; but there are others which depress and paralyse his powers. I am convinced that the extreme north, like the tropics, is unfavourable to the best mental and physical conditions of the human race. The proper zone of man lies between 30° and 53° N."

Mr. M'Dougall tells us of the men of the *Resolute* acting in the north when the temperature was at zero upon the stage, and —5° within three feet of a fire. The great differences of temperature to which the men are constantly and suddenly exposed become very trying to the European constitution. The transit from the lower deck of the vessel to the upper deck will sometimes make a change of 100°. Ten steps of a ladder, and you would suppose you had arrived in another world! The interior of an Eskimo's hut may cause the perspiration to stream down you; go outside, and expose your nose, and it becomes frost-bitten! But even without the differences produced by blubber lamps and human caloric, natural causes will sometimes make a remarkable change. "The thermometer at the brig," says Dr. Kane, "indicated + 26°, when we left it the temperature was – 44°. It had risen at least 70°. I defy the strongest man not to suffer from such a change." Under all circumstances the addition of wind to the cold is the most dreaded enemy of the arctic explorer. Experience taught Kane to prefer – 40° with a calm, to – 10° with a gale in the face.

Of all the discomforts attendant upon wintering within the Arctic circle, none, perhaps, is felt to be so depressing as the *absence of light.* The whole aspect of nature is changed, and the human system is affected in a powerful degree. This continued night is particularly injurious to the mind; the temper becomes irritable, the intellectual energies impaired, and the habits of some become gloomy and solitary (McDougall, Kane, *passim*). "All other evils," writes the officer of the *Resolute*, "sink into utter insignificance when compared with the disheartening nature of utter darkness for the space of ninety-three days." But in the higher latitude of the winter harbour of Kane (78° 39′ N.) the sun was absent for 120 days, astronomically, and in effect, for 134 days, in consequence of the horizon being obstructed by a mountain ridge, and this with all allowances being made for refraction. Light disappeared on the 24th of October and reappeared on the 21st of February. Eight degrees of the sun's limb below the horizon was Parry's lowest, and this depression still permitted him to read diamond type by his turning the print towards the South, whereas Kane was fifty-two days without being able to accomplish this, and looked forward to the coming of Parry's penumbral darkness as a joyful era. Dr. Kane observes:

"*November 7th.*—The darkness is coming on with insidious steadiness. We still read the thermometer at noon-day without a light, and the black masses of the hills are plain for about five hours, with their glaring patches of snow, but all the rest is darkness. Lanthorns are always on the spar deck, and the lard-lamps never extinguished below. The stars of the sixth magnitude shine out at noon-day. . . . . *December 15th.*—The last vestiges of mid-day twilight went. They could not see print, and hardly paper; the fingers could not be counted a foot from the eye. Noon-day and midnight were alike. The first traces of returning light were observed at noon on 21st of January, having just reached that degree of mitigated darkness which made the extreme midnight of Sir Edward Parry in 74° 47′ N. Even as late as the 31st, two very sensitive daguerreotype plates treated with iodine and bromine, failed to indicate any solar influence when exposed to the southern horizon at noon. The influence of this long intense darkness was most distressing." (vol. i. p. 155.)

According to Dr. Hayes,* who accompanied Dr. Kane, the darkness appeared to have, as might be expected, a bleaching effect, and tended to diminish the colouring matter of the blood,—the brick-dusty appearance of the latter, alluded to by Kane in his narrative, very much expressing its character. We have been lately told† that it is not easy to demonstrate with certainty the influence of darkness in deteriorating vitality, and that any such effect it has is not one of great extent. A recent Arctic traveller‡ suggests that—

"If any one doubt how necessary light is for our existence, just let him shut himself up for three months in the coal-cellar, with an underground passage into the ice-house, where he may go for a change of air, and see if he will be in as good health and spirits at the end of the experiment as before; at all events, he will have obtained the best idea one can form at home of an Arctic winter in a small vessel, save that the temperature of the Arctic ice-house is $-40°$, instead of $+32°$ as at home."

Dr. Inman himself tells us of an attempt made to establish an hotel for invalids in the mammoth cave in Kentucky, where the temperature is high and equable, and which signally failed in consequence of the patients suffering more from the absence of light than they gained advantage from the genial warmth.

Mr. Taylor confesses to a feeling of relief—

"When we turned our faces southward, and commenced the return to daylight. We had at least seen the Polar night, the day without a sunrise; we had driven our reindeer under the arches of the aurora borealis, and we had learned enough of the Lapps to convince us that further acquaintance would be of little profit; and it now seemed time to attempt an escape from the limbo of death in which we had ventured. Our faces had already begun to look pale and faded from the weeks of alternate darkness and twilight, but the novelty of our life preserved us from any feelings of depression, and prevented any perceptible effect upon our bodily health, such as would have assuredly followed a protracted experience of the Arctic winter." (p. 121.)

If we are told that we must regard the Eskimo and the members of the Arctic flora and fauna as, nevertheless, relatively flourishing under the peculiar relations of the solar light and heat of a Polar clime, we would reply, that such beings are naturally adapted for them, and this, in our view of the great question we are discussing, is of course the

* Transactions of the Ohio Medical Society, 1857.

† Foundation for a New Theory and Practice of Medicine, by Thomas Inman, M.D. London, 1860. p. 42.

‡ The Search for Sir John Franklin; from the Private Journal of an Officer of the *Fox.* Cornhill Magazine, No. i. p. 105.

all-important thing. To the organic beings of other regions, one or two Arctic winters would probably be death.

The most marked and primary effect which a struggle against the anti-hygienic influences of Arctic life has upon the constitution of the European, is the sure and rapid production of scurvy. This, as the depressing environments continue to operate, becomes developed in all its horrors. With only one exception, we believe, it has always made its appearance during the first wintering in the ice, and it even attacks the hardy and northern men who become residents at the Greenland settlements. An exemption from scurvy for the unprecedentedly long period of two years and three months occurred to the crew of H. M. S. *Investigator*, under the medical superintendence of Dr. Armstrong. The circumstances which appear to have assisted in producing this fortunate occurrence have been already passed under review,* and therefore need not be here alluded to. Suffice it now to say, we are told that in less than a year from the time mentioned, it was

"Lamentable to witness the pale, haggard aspect of our men, stalking about the deck like living spectres, cold and hungry, for daylight now revealed all their imperfections; so sadly altered were they from what they had once been . . . . it was generally remarked how vacant was the stare, and how fatuous and inexpressive their countenances when contrasted with healthy men, by those unaccustomed to view such objects as the *Investigator's* then presented; thus affording truthful evidence of the shock which the intellectual faculties had sustained, and the mental prostration that ensued after so long a period of complete isolation from the world under such trying circumstances." (Armstrong, p. 570.)

The crew were all found in a state of greater or less debility, and the scorbutic taint universally existed in various degrees of aggravation amongst the men. According to Mr. M'Dougall, they were utterly incapable of resisting the severity and privations attendant upon another winter in the Arctic zone. By the commencement of March, during the first winter of Kane's expedition in Smith's Sound only two men were exempt from scurvy. By March of the second winter, the party was quite broken down, and it was unusual to find more than three who could assist in caring for the rest. But such was the severity of the season, and so scarce were the means of subsistence, that the Eskimos themselves were reduced to the lowest stages of misery and emaciation (ii. 311). The men of the American party made a desperate attempt—one unequalled in the annals of Arctic voyaging—to escape, and finally the survivors reached a southern haven at Upper Nivik, in North Greenland.

In these high latitudes *raw* fresh meat is that which best and most surely sustains existence, and of such food fat must of course form a considerable proportion. Even at the commencement of the second winter there were few amongst Kane's party who did not relish a slice or a chunck of frozen walrus beef, whilst the liver, eaten with little slices of fat, was looked upon as forming a delicious repast. In fact, such food constitutes, in Dr. Kane's opinion, "a condensed heat-making and anti-

* Medico-Chirurgical Review, vol. xxii. p. 387.

scorbutic diet that has no rival." The natives of South Greenland prepare themselves for long journeys in the cold by a course of frozen seal. At Upper Nivik they use the narwhal in preference to the seal, while the bear, to use the local expression, is "stronger travel than all." The liver of this latter animal seems to disagree often with Europeans, producing vertigo, diarrhœa, and other symptoms of poisoning.* In M'Clure's expedition the men at length ate their meat raw, and although Dr. Armstrong admits that "the feeling which prompted us to the adoption of the practice appeared to be but little under the control of the will" (p. 546), yet he imagined it contributed to the deterioration of health. A consideration of Kane's theory and practice, and of the custom of the Eskimos of Smith's Sound, has impressed us with the belief that *raw* fat flesh must form the *pièce de resistance* of the Polar larder. *Everything which has life* that can be obtained appears to be eaten in these northern climes, nay, everything of everything, for entrails are swallowed in the same manner as maccaroni is indulged in by the Italians. Whether for a continuance even a sufficiency of raw animal food alone would prevent the outbreak of scurvy in the European, is a far more doubtful matter, and here we should be inclined to side with Dr. Armstrong rather than with Dr. Kane. We believe that sooner or later a certain quantity of fresh vegetable juices would be *absolutely* necessary. Now, can we feel surprised that a stay for any time within the Arctic circle should be antagonistic to the continued existence of alien tribes, associated as it must necessarily be with deficient and improper food, intense cold, prolonged night, mental lassitude, *ennui*, and despondency? Some, like Dr. Kane, are of opinion, however, that an European or an American can be acclimatized as high as 72° N., when cold, and cold only, has to be encountered, there being light enough for out-door labour. But more northerly, where the blighting darkness is present for so long, this cannot be expected. The adventurous and hardy spirit we are referring to considered that no degree of natural cold yet known can arrest *travel*, provided you are properly and abundantly fed, and possess a comfortable equipment. He both sledged and walked sixty and seventy miles over the roughest ice at – 50°. With two of his crew he made eighty-four miles in three consecutive marches. In Dr. Kane's opinion, the power of resistance in the Eskimo to exposure and fatigue is not greater, perhaps, than in a well trained voyager from a more temperate clime. We are unable to reconcile this belief with what we know of this matter. Baron Wrangell says that the Jacuti men slept at – 50° opposite the fire with their backs exposed. Kane himself states that the Eskimos of Smith's Sound always have an uncovered space between the waistband of the "nan nooke" and the "kapetah." To bend forward exposes the back to partial nudity, and no matter what the attitude, the entire chest is open to the atmosphere from below. A man so clad will sleep upon his sledge with the cold at 93° below the freezing point of water!

"When I saw Kalutanah, who guided the return party to the brig from Tessensak, the temperature was below – 50°. He was standing

* Upon this point, see also Lord Dufferin, op. cit. ante.

in the open air, comfortably scratching his naked skin, ready for a second journey, which in effect he made eight hours afterwards." (Kane.) Alluding to a more than usually severe night, Dr. Kane observes in reference to the Eskimos, that the only departure from their practised routine, which the bleak weather and open snow roof seemed to suggest to them was, that they did not strip themselves naked before coming into the hut, and hang up their garments in the air to dry like votive offerings to the God of the sea. (i. p. 381.) Kane, upon the contrary, laden with furs and woollens, layer upon layer, could not keep up his circulation upon a sledge, nor, indeed, without active exercise if the thermometer was below $-50°$. He had to run occasionally, or must have succumbed to the cold; he writes, too—"I feel that we are fighting the battle of life at disadvantage, and that an arctic night and an arctic day age a man more rapidly and harshly than a year anywhere else in all this weary world." (vol. i. p. 173.)

That original strength of constitution, gradual exposure, and great natural powers of resistance will occasionally enable a north-countryman to exhibit Eskimo endurance, must be admitted. Petersen, the well-known Danish interpreter, who resided for two years at Upper Nivik, seldom enters a room with a fire, whilst Riley, one of Kane's party, had so inured himself to the cold that he slept upon the sledge journeys without a blanket or any other covering to his walking-suit while the outside temperature was $-30°$. Kane thought that there must be many such hardy men in Franklin's expedition—North Orkney men, Greenland whalers, e.g.—men he looked upon as inferior to none in capacity to resist the deleterious influences of an arctic climate. That some remnant of these men still existed was Kane's fervent idea, the open spot of some tidal eddy had been chosen, where, under the teaching of the Eskimos, or perhaps of one of their own Greenland whalermen, they had set bravely to work and trapped the fox, speared the bear, and killed the seal, the walrus, and the whale. Alas for the futility of conjecture. We now know that what remained of the crews landed upon the arctic shores of the continent of America, upon which inhospitable and barren region they gradually died from starvation, after having been driven (according to one account—viz., Rae's) to the last resource of the most miserable, cannibalism, as a means of prolonging their wretched existence. At any rate, that they were here all starved is the opinion of by far the majority of those who are best fitted to judge of the matter. Even the Eskimos themselves are dying out in some places; the climate is so horrible, even *they* cannot endure it. Such is the case, according to Kane, within Smith's Sound:

"They confirm, however, a fearful conclusion which these poor wretches have themselves communicated to us—that they are dying out; not lingeringly, like the American tribes, but so rapidly as to be able to mark within a generation their progress toward extermination. . . . It pains me when I think of their approaching destiny—in the region of night and winter, where the earth yields no fruit and the waters are locked—without the resorts of skill or even the rude materials of art, and walled in from the world by barriers of ice without an outlet." (Vol. ii. p. 210.)

We admit, with Dr. Kane, that the mysterious compensations by which we adapt ourselves for a time to climate are more striking here than in the tropics. In the arctic zone the assault is far more immediate and sudden, and, unlike the insidious fatality of hot countries, produces its results rapidly. It requires hardly a single winter, according to Kane, to tell "who are to be the heat-making and acclimatized men." (Vol. i. p. 246.)

Bearing this in mind in connexion with a recently-advanced and well-known theory, it might be suggested whether or not, according to the principle of "natural selection" carried out upon a continuous and extensive scale, the races of the temperate zone could so change by the preservation and accumulation of successive slight favourable variations as to be able at some distant period to colonize the shores and islands of the arctic seas. *We* should as readily expect this as we should look for the colonization of the Bight of Benin by the Saxons and Celts of the British islands. But we are warned to conclude, and it must hence suffice us to say that, after an attentive study of the subject before us, we have become satisfied that when a second winter has been passed within the arctic circle by men of the temperate zone, a slow but sure diminution of vital power would be apparent in those who have endured its trials. Further, this diminution, though perhaps tardy and gradual in approach, would progress in an increasing ratio, both as respects rapidity and power, in relation to the time of further detention within the polar regions. That this deterioration of vital energy is not at first easily perceived by those who suffer it,* is not an improbable conclusion, seeing that—speaking generally—*all* suffer it to some extent, and thus have but a very imperfect standard by which to judge of their physiologic powers. But were it possible that a few could during the whole time maintain the maximum of the energies which they took with them to those regions of eternal cold—

"Where frost
Reigns everlastingly, and ice and snow
Thaw not, but gather,"

and by it weekly measure the energies of their companions once gifted with their own high standard of vigour, we do not doubt that they would discover polar wintering has a very malignant influence upon the vital powers of the British seaman. Whilst believing this, we are free to admit that men who have passed only two or three winters in the North, bearing well the struggle, rapidly recover upon their voyage home a fair amount of health and vigour.†

* See in particular upon this point Capt. M'Clintock's observations concerning Franklin's party; also Sir John Richardson's Narrative of a Journey to the Shores of the Polar Sea.

† Since this article was written, Dr. Hays' 'Arctic Boat Journey in the Autumn of 1854,' has appeared. Its perusal has tended to confirm the reviewer in the truth of the foregoing conclusions. He would simply add to them his suspicions that future explorers, when in the power of the Eskimos, will do well to employ more caution than has hitherto been thought necessary to adopt.

Review II.

A Medico-Legal Treatise on Malpractice and Medical Evidence, comprising the Elements of Medical Jurisprudence. By John J. Elwell, M.D., Member of the Cleveland Bar.—*New York*, 1860. 8vo, pp. 588.

Dr. Elwell deserves the thanks of the medical and legal professions for the very valuable work he has presented to them. It worthily upholds the high character which has already distinguished transatlantic jurisprudential literature. Dr. Elwell's special qualifications for the undertaking he has so ably completed, will be best inferred from his prefatory observations, in which he states:

"The active practice of medicine and surgery for several years having taught me something as to the magnitude of the duties and difficulties, the wants and liabilities of the medical profession, and a corresponding length of time devoted to the study and practice of law having deeply impressed me with the importance of the two professions, relatively as well as independently considered, developing also the obvious fact that legal men and legal works devote too little attention to medico-legal subjects, I have thought that in no way could I better serve the interests of the two great professions to which I have devoted my life, and promote the great ends of science and justice, than by endeavouring to embody in a concise, complete, and comprehensive work all the settled principles and known authorities, as well as the result of my own thought and experience, upon the subject of malpractice and medical evidence."

In pursuance of this design, the volume before us is divided into two parts, both manifesting enlarged experience and extensive research, and both affording unmistakeable evidence of the author's anxiety to perfect, as he has done, a volume worthy of the occasion. While entertaining this opinion of the author's labours, it may seem paradoxical to affirm that the perusal of his work has left a very painful impression on our mind—one which is entirely at variance with our recollection of many highly-gifted and most honourable American friends, distinguished members of both of the professions to which Dr. Elwell addresses himself.

According to Dr. Elwell's experience, there appears to prevail amongst the members of the learned professions of medicine and law, as well as amongst the general community in America, a vitiated sense of their social and moral responsibilities, to a degree of which we have had previously no adequate apprehension. We would fain hope that we misconceive Dr. Elwell's observations in reference to either of these learned bodies. That the public may be occasionally ungrateful to their best friends, requires but passing experience of life painfully to demonstrate; that unworthy individuals may on rare occasions disgrace honourable positions is, we grieve to say, too sadly true; but that on all sides, as a practice, should be found such a want of honesty, honour, and self-respect, as Dr. Elwell's work would lead us to infer exists amongst our American cousins, argues a condition than which we, habituated to English sentiments and associations, can conceive nothing more deplorable. Again, we affirm our unwillingness to believe that in America the law, through the co-operation of its "eminent" members, is capable of being habitually rendered an instrument for extortion, or medicine with the sanction of its illustrious disciples suborned for the purposes of fraud, or that individuals having any claims to respectability are ever on the watch for opportunities to victimize their professional attendants. Whether we do not misconceive Dr. Elwell's observations, or too severely draw our inferences, the following extracts will enable the reader to judge:

"Happily," writes Dr. Elwell (p. 1), "criminal malpractice is but rarely met with in courts of justice, while civil suits for damages are of a frequency alarming both to the profession of medicine and to the public. Suits of this class in some parts of the country seem to be on the increase. So common an occurrence is it for the surgical treatment of the oldest and best physicians and surgeons in general practice to be called in question and overhauled in courts of justice, that there is at this time a general feeling of uneasiness, and a conviction that the business is at least very dangerous, so far as property and reputation are concerned."

Again (p. 83):

"There can hardly be found a place in the country where the oldest physicians in it have not, at some periods of their lives, been actually sued or annoyingly threatened. The fact that actual damages are not often recovered, helps the matter but little: the damage to business, and the costs attending the suit, are usually great."

This latter paragraph refers to cases detailed in the previous pages, which we presume are of the average class on which Dr. Elwell's observations rest. We quote one of them as recorded:

"Drs. H—— and S—— were called upon to see a Mr. P——, whose leg had been crushed by a falling log while assisting to elevate it in building a loghouse in a new settlement. The injury was so severe, in the opinion of these surgeons, that amputation was deemed necessary. They were both old and experienced physicians and surgeons, having practised thirty or forty years in the locality where the accident happened. The operation was performed after due deliberation and consultation, the patient recovering from the operation in about the usual time. Some years after the events of accident and amputation, the manner and propriety of this amputation were discussed among the friends of the patient, the bones were dug up, cleansed, and made the basis of a suit against the surgeons. Damages were laid at 10,000 dollars. Eminent counsel were found to undertake and carry on the cause for a portion of the spoils. Several long trials were had, the jury not agreeing. Depositions were taken in Philadelphia, New York, and Washington, involving great expense. No judgment was obtained against the defendants, but the litigation was nevertheless ruinous to them."

We almost mistrust the accuracy of this report, as we do not find recorded any indignant remonstrance on the part of the medical profession, neither do we read that the "eminent counsel" were dis-barred. Doubtless both events took place, and have been omitted by the author as superfluous. Should it have been otherwise, it is consolatory to reflect that only in a new country, where the dignity of social status wants the stability of time, could the integrity of professional life have been so outraged and violated, without entailing on the offenders the hearty contempt of every honest man.

We are disposed to think that Dr. Elwell has either unintentionally overstated his case, or derived his illustrations from adventurous individuals residing and incidents occurring in localities wherein a rude, unsettled state of society exists. This view is in a measure corroborated by other parts of his work. In an action for malpractice, stated as having been tried before Judge Mallet, in the circuit of the Supreme Court, held in August, 1848, we read :

> "The examination of the numerous witnesses having closed, and the counsel having addressed the jury, the judge followed with a most pungent and impressive charge, in which the jury were instructed to disregard all mere appeals to their prejudices, and especially to reject that counsel which would advise them to look upon the medical profession as an oppressive and aristocratic monopoly, and to decide the case upon the facts, as drawn from the witnesses." (p. 100.)

"Oppressive and aristocratic monopoly" are terms which sound strange when applied to a profession whose mission it is to be familiar with suffering, while mitigating sorrow and distress. Such observations, if true in any sense, would argue strongly in favour of the presumption we have ventured to put forward—that the illustrations Dr. Elwell quotes are happily confined to districts of the character intimated; and in this we are to a degree further confirmed by the observation of the Hon. John Bredin, president judge on a trial for malpractice in 1850, who, when charging the jury, observed:—"If suits were more frequently brought, we would, perhaps, have fewer practitioners of medicine and surgery not possessing the requisite professional skill and knowledge than we now have." (p. 121.) This observation is certainly not in accordance with those of Dr. Elwell, which we have quoted, and seems opposed to the extract with which the volume before us opens.

America has accomplished great things both in medicine and law. No contemptible jealousy shall induce us to question her merits. We rejoice that, for the progress of science and the benefit of mankind, her claims to confidence and respect are well and substantially founded. In proportion as we do so, we regret the statements which Dr. Elwell sets forth. We fain hope the new will emulate the old country in that self-respect which is the safeguard of honesty. It is certainly no exaggeration to affirm that the medical profession throughout our United Kingdom is regarded with feelings of a far different character from those which we have been discussing. The public generally are satisfied that a deep sense of their serious responsibility, guided by adequate acquaintance with the principles of their science, regulates the conduct and practice of its members, who, with rare exceptions, are educated gentlemen. Society would not tolerate the institution of vexatious proceedings against its most zealous benefactors. In no instance that we can recal has any proceeding for malpractice been entered upon wherein both jury and judge did not question with a jealous scrutiny the grounds for so serious an imputation. The circumstances under which damages have been recovered have been both rare and peculiar; we rejoice to add, not more rare than a dispassionate estimate of our social status argues to have been just, and not less peculiar than the general efficiency of our medical men would have led us to anticipate. Of the legal profession it is not too much to add, that they worthily illustrate, as they ably expound, the beautiful working of those laws under which it is our happiness and privilege to live. Personal honour is ever regarded as common property, held by each of its members for the benefit of the whole; sorrow invariably falls upon him who lightly estimates, or rashly endangers, the sacred trust committed to his charge. Ere long may we be in a position to state that, throughout the entire American continent, a complete reciprocity of feeling will exist in reference to professions which, as they are the creations of progress, are also in their exalted practice guarantees for social comfort and strongholds of civilization. With these general observations, which Dr. Elwell's illustrations have induced, we proceed to investigate the special claims of his work.

In stating the general principles of law applicable to members of the medical profession, Dr. Elwell truly observes:—"The professional man does not agree or stipulate to carry the case through to a successful issue at all events and notwithstanding all contingencies, and he is not to be tried by the result." There is, however, nothing to prevent him, should he be rash enough to do so, from (by a special contract) absolutely engaging to perfect any particular act, or to accomplish any specified cure. In such a case the utmost diligence and skill will not excuse him should the result be unfortunate, because it is deemed to be his own fault and folly that he did not thereby expressly provide against contingencies, and exempt himself from responsibility in certain events. Of this medical men ought not to complain. It is right and proper, when the member of a learned profession places himself on the same footing with a trader, that the penalties attaching to the position he adopts should attend his doing so. Accordingly, in the instance of an absolute and general contract, the performance is not excused by an inevitable accident or other contingency, although not foreseen by, nor within the control of, the contracting party. Under ordinary circumstances, a generous construction is invariably put on the actions and motives of medical men. The law casts every protection round them in the conscientious practice of their profession. Both American and English judges agree in the nature of the obligation under which medical men rest. Mr. Justice Story observes:

"In all those cases where skill is required, it is understood that it means *ordinary* skill in the business or employment which the bailee undertakes; for he is not presumed to engage for extraordinary skill, which belongs to a few men only in his business or employment, or for extraordinary endowments or acquirements. Reasonable skill constitutes the measure of the engagement in regard to the thing undertaken."*

Lord Chief-Justice Tindal, in the same spirit, remarks:

"Every person who enters into a learned profession undertakes to bring to the exercise of it a reasonable, fair, and competent degree of skill."†

* Story on Bailm. 433. † Lamphire v. Phibos; 8 C. and P. 475.

This doctrine of the common law implies on the part of the practitioner *bona fides* and good common sense. These must ever be the protection of professional men where ordinary skill is brought into conflict with extraordinary difficulties. Dr. Elwell, acting on the dicta quoted, observes:—"The physician and surgeon are not responsible for the errors of an enlightened judgment where good judgments may differ." Neither, it may be added, is the non-professional individual who, "with some degree of skill," honestly exercises to the best of his ability his judgment, even though success does not attend his efforts. Mr. Justice Bullock, of whom Lord Chief-Justice Hale observed, "that a sounder lawyer or a stronger-headed man was never known in the profession of the law," well remarked, "many persons would be left to die if irregular surgeons were not allowed to practise." While, then, for the general welfare of society, all having knowledge are invited to use it for the public advantage, and protected in its exercise; for the special protection of those who suffer, each one who undertakes their treatment does so at his own risk, and the onus lies on him to show, should necessity arise, that ordinary skill, care, and diligence were by him exercised. The determination of what "ordinary skill" is may involve considerations which embrace circumstances in connexion with, though not of, the cure—as, for instance, where unqualified individuals, under the pressure of necessity, exercise their best skill and judgment, however limited that might be, they are not, nor will they be, held responsible for the eventualities thereof.

"When, however," to use the words of Lord Lyndhurst, "proper medical assistance can be had, and a person totally ignorant of the science of medicine takes on himself to administer a violent and dangerous remedy to one labouring under disease, and death ensues in consequence of that dangerous remedy having been so administered, then he is guilty of manslaughter."*

In such a case humanity does not demand nor does necessity require unqualified interference, and accordingly responsibility attaches to its exercise. The medical man who neither rashly undertakes nor hastily adopts a method of practice, though it be new, will not be held responsible for its failure if it be administered in the reasonable scientific belief of its applicability. Were it otherwise, it is obvious that science would degenerate into routine, and mankind be prevented the blessing of progress. This license implies the exercise of such discretion as precludes the charge of rashness. The leading case on the subject being Slater *v.* Baker,† in which the Lord Chief-Justice says, "rashness is ignorance, and because the party wished to try an experiment that he was not warranted in doing, he acted unskilfully." A judgment further confirmed by Mr. Justice Bazley, who in Rex *v.* Simpson observed:

"I take it to be quite clear, that if a person not of medical education, in a case where medical aid could be obtained, undertakes to administer medicine which may have a dangerous effect, and thereby causes death, such person is guilty of manslaughter. He may have no evil intention, and may have a good one, but he has no right to hazard the consequences in a case where medical

* Rex *v.* Webb, 1 M. and Rob. 405.

† 2 Wilson, 259.

assistance may be obtained. If he does, it is at his peril. It is immaterial whether the person administering the medicine prepares it himself or gets it of another."*

It is evident from these opinions that the capability of obtaining properly-qualified advice invests the administration of the remedies with their special significance.

While the law is thus protective of the public in reference to medical practitioners, it requires that patients co-operate with their professional advisers, and conform to the necessary prescriptions. It was well observed in the case of McCandless *v.* McWha:† "If the patient will not, or under the pressure of circumstances he cannot so do, his neglect is his own wrong or misfortune, for which he has no right to hold his surgeon responsible. No man may take advantage of his own wrong, or charge his misfortune to the account of another."

The inherent elementary difficulties connected with the practice of medicine and surgery are discussed by Dr. Elwell with an ability which proves him to have been a highly-informed member of the profession, the immediate practice of which he has forsaken. When, however, he seeks, as he does in Chapter III., to set forth what definite knowledge is possible and essential for the physician and surgeon, it is to be feared that he endeavours to govern physiological principles by legal rules. Dr. Elwell writes: "The courts hold that the surgeon and physician must be master of that degree of knowledge which is reasonably within their reach," and adds, that to this end "a minute and correct understanding of the real character and importance of inflammation is essential;" further writing: "What the terrible power of steam is to the engineer, inflammation is to the surgeon." A great deal has been and might be written on this subject. We forbear to enter on the discussion. If Dr. Elwell means to affirm that elementary knowledge is essential, we cede his proposition; when, however, he draws a parallel between a known power and fixed mechanism, and an unknown power and undetermined organism, and so contrasts physical and vital force, we confess our inability to follow out his arguments, and prefer adhering to the more immediately legal considerations of his work.

In his chapter on "Malpractice from Amputation," an analysis of the various forms of apparent warrant for so serious an operation is instituted. This is followed by able observations on "Fractures, Dislocations, and Deformities," wherein these several subjects are reviewed in a most masterly manner; we pass observations worthy of close study to set forth the able opinion of Mr. Justice Bell, who, in case of Leighton *v.* Foster,‡ arrives at the following propositions as the opinion of the court:

1. A physician or surgeon, without a special contract for that purpose, is never considered as warranting a cure.

2. His contract, as implied in law, is,—1. He possesses that reasonable degree of learning, skill, and experience which is ordinarily

* 4 C. and P. 379. † 22 Pen. State Reports, 268. ‡ 7 Foster's Reports, 460.

possessed by others of his profession. 2. That he will use reasonable and ordinary care and diligence in the treatment of the case committed to him. 3. That he will use his best judgment in all cases of doubt as to the best course of treatment.

3. He is not responsible for want of success, unless it is proved to result from want of ordinary skill, or from want of ordinary care and attention.

4. He is not presumed to engage for extraordinary skill or for extraordinary diligence and care.

5. He is not responsible for errors of judgment or mere mistakes in matters of reasonable doubt and uncertainty.

6. Where the declaration against a surgeon alleged that the plaintiff sustained injury from the *want of skill* and *mere neglect* of the surgeon in the treatment of a fracture, it was held that evidence that the defendant had received a good surgical and medical education, and was a regularly-educated and skilful surgeon, could not properly be shut out from the jury, because it tended to disprove a material allegation of the declaration.

This opinion we believe to express the several dicta which from time to time have fallen from the English bench, and to be one which may with the greatest confidence be adopted. The justly celebrated case of St. John Long,* according to Dr. Elwell, forcibly illustrates "what degree of ignorance, negligence, and hardihood, can at times pass the ordeal of an English court, and not meet with condemnation and punishment," as the case of Rex *v.* Williamson, previously quoted, demonstrated "how ignorant a distinguished Lord Chief-Justice of England may be as to the science of medicine." Perhaps Dr. Elwell himself supplies the best criticism on these observations when he writes (p. 231)—"These cases have been given at some considerable length, because they contain the principles upon which this class of cases have heretofore been considered and decided in England. They have been the cases to which American judges have referred, and by which they have been guided. They have generally been followed." The profession is familiar with the circumstances of this remarkable trial. We cannot better conclude our notice of this section of Dr. Elwell's work than by renewing their intimacy with certain dicta which at present express the judicial mind on the points they then embraced.

Mr. Baron Garrow observed: "It matters not whether the individual consulted be the President of the College of Physicians, the President of the College of Surgeons, or the humblest bone-setter of the village; be it the one or the other, he ought to bring into the case ordinary care, skill, and diligence." Mr. Justice Park added: "Whether the party be licensed or unlicensed is of no consequence, except in this respect, that he may be subject to pecuniary penalties for acting contrary to charters or acts of Parliament." While in refence to failure of honestly directed efforts for medical or surgical relief, where necessity had demanded their exercises, quoting Lord Chief-Justice Hale,

* 6 Bing. 440.

he observed: "God forbid that any mischance of this kind should make a person guilty of murder or manslaughter;" and proceeds to add—

"I agree with my learned brother, that what is called *mala praxis* in a medical person is a misdemeanor; but that depends upon whether the practice he has used is so bad that everybody will see that it is *mala praxis*."

Further, we read—

"It is God that gives health; man only administers medicine; and the medicine that the most skilful may administer may not be productive of the expected effect; but it would be a dreadful thing if a man were to be called in question criminally whenever he happened to miscarry in his practice. There are things for your (the jury's) consideration, when you are considering whether a man is acting wickedly; for I call it acting wickedly when a man is grossly ignorant, and yet affects to cure people, or when he is grossly inattentive to their safety."

No one, we apprehend, will question the justness of these views.

The second part of Dr. Elwell's work, in which is discussed the question of medical evidence, must be regarded as a valuable addition to our juridical literature. It includes the medico-legal consideration of insanity, poisoning, and the several other questions in which opinions of experts are available for the furtherance of justice, and is written throughout in a thoroughly philosophic spirit. Dr. Elwell's observations on the duties and responsibilities of medical witnesses specially deserve perusal. In the contrast which he institutes between ordinary and scientific evidence, we read—

"All the common witness has to do is simply to tell the truth; when he has done this, his highest duty is accomplished; but the medical witness must know the facts first, constituting the case upon which his opinion is desired; then he must apply to these facts the special knowledge he has of other scientific facts, established perhaps by many difficult experiments of different experimenters in various countries, and possibly, in different ages. Upon this chain of facts, one end or the whole of which may lay completely beyond the reach and sight of the court and jury, his reasoning must be correct, or his opinion will be erroneous.

"The medical witness, then, can only be prepared to do credit to himself, justice to the parties interested in the issue of the case upon which he is called, and honour to the profession he represents, by a thorough, well-ordered, will-digested knowledge and complete understanding of his profession in all its extensive and intricate departments, upon questions in any of which he may be called to give an opinion.

"The witness is entitled to the right—and should insist upon it—of having the question fairly and clearly stated; and he should not attempt an answer until he fully comprehends its bearing."

Having, in terms of just and merited censure, alluded to ignorant and self-conceited "doctors," who often intrude themselves upon the court and the bar, Dr. Elwell offers a masterly sketch of the principles which should guide the expert in the discharge of his duties:

"Strength of character, candour, intelligence, manly diffidence on questions that science cannot yet fathom, characterize the true medical man, who, by hard labour and severe study, has comprehended his science as far as possible. It gives to him a proper confidence and self reliance; enabling the court and attorneys to distinguish the true representative of science from the impostor, who will be glad to retreat to an oblivion that can alone shield him from the consequences of his guilt and presumption."

These opinions, expressing high and exalted moral sentiments with accurate and enlarged scientific views, caused us to exclaim, "Happy the people and privileged the profession who experience and practise in accordance to this teaching!" We began to question the fidelity of the impressions the perusal of the previous section had left, and sought again throughout the volume for their refutation. We found close reasoning and correct law, just criticisms and able opinions, practical expositions of principles in their operation, and careful examination of the relative value of medical facts on which scientific judgments not infrequently rest. We found controversy and personality in medical writing advisedly censured, and the gifted and amiable Professor Taylor rebuked for certain observations in his last volume. We found his scrutiny of the evidence in Palmer's trial the ground for Dr. Elwell's remark: "If his professional brethren in England are half as bad as he represents them, the condition of medical testimony in that country is truly deplorable." We stood rebuked. We felt that here at home there was much to regret, much to improve, much to wish otherwise. We fain would have drawn our pen across the pages we had written; before doing so, we sought for a practical illustration wherein these admirable doctrines for the guidance of the American profession and public might be manifest in their operation, when we read:

"Physicians alone are permitted to give their opinions as to the existence, nature, or extent of disease in any person. As, when it is alleged that a slave was unsound at the time of her sale, in consequence of her having the venereal disease, evidence of physicians is competent to show that the disease did not at the time prevail in the neighbourhood in which she was sold, but did prevail in the town, about seventy-five miles distant, to which she was taken by the purchaser soon after the sale."*

We closed Dr. Elwell's most learned work, and felt thankful that we lived in England.

* Lusk *v.* McDaniel, 13 Ird. 485.

REVIEW III.

1. *Kritische Geschichte des Ursprungs, der Pathologie und Behandlung der Syphilis, Tochter und wiederum Mutter des Aussatzes.* Von Dr. FRIEDRICH ALEXANDER SIMON, praktischem Arzte in Hamburg, mehrer gelehrter ärztlicher Gesellschaften und Vereine correspondirendem und Ehrenmitgliede. 8vo.—*Hamburg.* Erster Theil, 1857, pp. 261. Zweiter Theil, Erste Abtheilung, 1858, pp. 324. Zweiter Theil, Zweite Abtheilung, 1860, pp. 144.

A Critical History of the Origin, Pathology, and Treatment of Syphilis, the Offspring and again the Parent of Leprosy. By Dr. F. A. SIMON, Practising Physician in Hamburg, Corresponding and Honorary Member of several learned Medical Societies.—*Hamburg.* First Part, 1857; Second Part, First Section, 1858; Second Part, Second Section, 1860.

2. *Der Kampf mit einem Lindwurm, oder unerwiesene Existenz der konstitutionellen Syphilis* VOR *dem Jahre* 1495. Von Dr. F. A. SIMON, &c.—*Hamburg,* 1859. 8vo, pp. 85.

The Conflict with a Dragon; or, the Unproved Existence of Constitutional Syphilis BEFORE *the Year* 1495. By Dr. F. A. SIMON.—*Hamburg,* 1859.

3. *Declaratio Defensiva cujusdam positionis de Mala Franzos nuper per Professorem Leporinum oppugnatæ. Der Morbus Gallicus sive Neapolitanus ist im Jahre* 1495, NICHT FRÜHER *und* ZUERST *in Italien ausgebrochen.* Offnes Sendschreiben an Herrn Professor Haeser, von Dr. Fr. Alex. SIMON.—*Hamburg,* 1860. 8vo, pp. 66.

A Defence of a Certain Statement with reference to Syphilis recently assailed by Professor Hare. The French or Neapolitan Disease broke out in the Year 1495, *not earlier, and appeared first in Italy.* A Public Letter to Professor Haeser. By Dr. F. A SIMON.

BEFORE entering on a review of the controversy indicated in the title of the above works, we must protest against the very undignified course pursued by their author, in punning miserably upon the names of his opponents. Not only is such a proceeding beneath the dignity of the medical profession, and of a scientific inquirer, but it is calculated to produce, at first appearance, an effect prejudicial to the cause advocated by the person who condescends to its adoption. We glanced for a moment or two at the designation of the work placed second in the above list before we could believe that the writer really wished to appear in the character of a punster; all doubt on the subject was, however, removed by the perusal of a portion of the title-page, which we considered too long for quotation—viz., "compare Doctor Lindwurm (*nomen et omen!*) Canstatt's Jahresbericht, &c." and by an unpleasant tone of irony in part of the work itself. Dr. Simon's Latin translation of Professor Haeser's name, which nevertheless, finding it before us, we felt bound to attempt to render into English, is far fetched and absurd.

Endeavouring, however, to dismiss from our mind any unfavourable impression which may have been produced in it by the circumstance just alluded to, we shall now proceed to the consideration of the subject matter of Dr. Simon's works.

The author is of opinion that probably in very remote antiquity, but certainly in the middle ages, virulent local diseases of the genital organs of every kind occurred, completely analogous to those which we now call syphilitic; but that there is no distinct historic proof that constitutional symptoms followed these affections previously to the year 1495, unless leprosy be somewhat gratuitously assumed to be the lues venerea of the time; and he presents us, in the first part of his "critical history," with a sketch of the virulent, or supposed virulent, diseases of the sexual organs and the parts about them, both before and subsequently to the appearance of the true lues venerea at the close of the fifteenth century. In the first section of the second part, Dr. Simon reviews the origin, pathology, and treatment of the morbus Neapolitanus sive Gallicus, which broke out in Italy in the year 1495; and in the second section he describes the pathology and treatment of the same during the seventeenth century.

From 'Der Kampf mit einem Lindwurm,' to the title of which we have already taken exception, we learn that Dr. Lindwurm, in 'Canstatt's Jahresbericht' for 1858, Band iv., p. 342, designated Dr. Simon's views as containing a wholly untenable hypothesis, referring the reader for their refutation to his report for 1854, p. 353. The author is thus led to revert to his observations on the "questionable and at least unproved existence of syphilis or lues venerea previously to the year 1495." Some passages in the Hippocratic writings,* which it has been attempted to apply to this affection, Dr. Simon considers as referring to the plague or pestilential fever, which in the time of Hippocrates raged in Athens and in many of the Grecian islands.

"It is well known that the plague in Athens, so masterly described by Thucydides (lib. ii. cap. 49), was attended with carbunculous abscesses of the genital organs, which were often wholly destroyed by gangrene; as we learn also from Lucretius, who, in the sixth book of his celebrated didactic poem, 'De Rerum Natura,' gives a fearfully comprehensive sketch of the same plague. It is true we have mention, in the description of the plague, of frequent affections of the throat, of aphthous ulcerations of the mouth, of falling out of the hair of the head and beard, of denudation and decay of the bones, of large pustules, and lichenous eruptions. But these are not unusual as symptoms and results of malignant pestilential fevers, and have also been observed in later epidemics of the plague. And supposing that in the plague of the fifth century before Christ, an, as it were, malignant syphilitic contagion had been in operation, why does it again suddenly disappear from history without leaving a trace behind, why do we learn nothing of its further effects in the next and subsequent ages? Why do we find in the writings of no Grecian physician of a later period the slightest indication, I will not say of lues venerea, but even of contagious affections of the sexual organs? That the only passage in Galen† which has been referred to gonorrhœal infection from inter-

* Epidem., lib. iii.

† De Sanit. tuenda, lib. vi. cap. 14.

course, has been grossly misunderstood, and has no reference to it,* I have incontestably shown." (p. 20.)

In like manner the author states that the "Egyptian and Syrian ulcers," spoken of by Aretæus,† were not syphilitic, and were nothing but the ulcerated and gangrenous quinsies which, according to Prosper Alpinus,‡ were in his time also very frequent in Egypt, and prevailed annually among children, as, according to Pruner,§ is still the case in the present day. The author finally comes to the conclusion—

"That in the whole of antiquity the idea of a syphilitic or venereal contagion is wanting, although impure genital affections and analogous local diseases of other kinds have occurred, such as we now observe in consequence of syphilitic infection. It is not until the middle ages, soon after the crusades, that the suspicion of an impure matter or virus, as the cause of many affections of the sexual organs, arises; but in neither period is the observation or the idea of constitutional or secondary venereal diseases met with, although many symptoms of the then so generally diffused lepra have been pointed to as such. Proofs of the existence of constitutional syphilis, as we know it, and according to what we understand by that term, are not discoverable prior to the year 1495. On this subject, the physicians who lived before and after the outbreak of the lues venerea or morbus gallicus, are the most competent judges. None of them, various as may be their opinions as to the cause and essential nature of the disease new to them, states that he himself or his predecessors had before observed a similar disease. They compare it with the worst form of leprosy, at that time already obsolete, with the saphati or yaws, with the malum mortuum, with elephantiasis, or they consider it to be new and unheard of. They contend with the greatest vehemence for or against the antiquity, or rather for or against the leprous nature of the morbus gallicus; but none of them ventures to say that he himself or the physicians before him, have seen such symptoms proceed from genital affections." (p. 22.)

Dr. Simon, however, points out that Dr. Lindwurm's 'Refutation' of his views was published three years previously to the appearance of his 'Critical History.' The paper to which Dr. Lindwurm's remarks applied may be found in Virchow's 'Handbuch der speciellen Pathologie und Therapie.'|| The views contained in it are thus epitomized by Dr. Lindwurm, and his abstract is, with the exception to the first proposition which we shall point out, accepted by Dr. Simon.

Simon having briefly mentioned the several hypotheses as to the antiquity and origin of syphilis, professes the opinion that syphilis is a degenerated or bastard form of the ancient lepra. The following are the reasons he assigns:

"1. That in remote, and particularly in the middle ages, urethral discharges, genital ulcers, warts and fissures in the sexual organs and the nates, very frequently accompanied leprosy; and that affections of these parts, which are by many physicians of the middle ages designated as results of connexion 'cum muliere fœda, sive leprosa,' prevailed also to a certain extent as precursors of leprosy."

Dr. Simon, in a note on the foregoing proposition, admits that a

* Part I. of my Critical History, p. 31.
† Lib. i. cap. 9, De Caus. et Sign. Morb. Acut.
‡ De Medicinâ Ægypt., lib. i. cap. 14. § Krankheiten des Orients, p. 206.
|| Band ii. erste Abth. pp. 424–429.

repeated and conscientious examination of the historical traditions with respect to the leprosy of ancient times, forces him to acknowledge that the genital affections observed and indicated as virulent, especially in the middle ages, by no means appear in the character of precursors of leprosy. "We find," he adds, "only the above-mentioned statement, that connexion with leprous individuals gave rise to excoriation and similar local affections, but it is not said that these were followed by the constitutional symptoms of leprosy." He therefore withdraws the latter part of the foregoing proposition.

"2. That impure connexion is by some writers adduced as a cause of lepra.

"3. That many symptoms of leprosy present a striking similarity to those of secondary, but particularly of tertiary syphilis.

"4. That both ancient and modern physicians speak of the transition of lues venerea into leprosy. Scherlievo, falcadina, sibbens, yaws, Canadian disease, radesyge, &c., are of syphilitic origin with partly leprous symptoms.

"5. That lues venerea, as well as leprosy, is entailed by parents on their children.

"6. That children of syphilitic parents, even when the latter at the time of procreation did not labour under visible symptoms of the disease, sometimes become the subjects of incurable cutaneous affections, corresponding to the known forms of leprosy.

"7. That syphilis and leprosy may slumber for years in the system as latent dyscrasias.

"8. That the most renowned surgeons of the middle ages have tried mercury as a remedy in many forms of leprosy."

Both Dr. Lindwurm and Dr. Simon agree in considering that syphilis is not a spontaneous disease of recent date, but they disagree as to its relation to leprosy. The latter writer considers—(we still quote Dr. Lindwurm)—

"The urethral discharges, the ulcers, condylomata and fissures, which were, even in the time of the Roman empire, of such frequent occurrence, to be, as it were, a local reflex of the then generally diffused leprosy. This, to a certain extent, particular leprosy of the genital organs and the surrounding parts, which unmistakeably consisted in nothing else than what we now call primary syphilis, increased, according to Simon, in consequence of the fearful immorality of the middle ages, in virulence and independence, while the genuine general leprosy, as a cutaneous disease only imported from the east to the west, progressively decreased, as since the cessation of the crusades the intercourse of the masses with the former no longer took place. The expedition of the French to Naples in 1494, accordingly afforded the opportunity for the outbreak of an already long-prepared disease. The virulent tinder glimmered for centuries in impure and often malignant genital affections, and needed only the opportunity of the sexual mingling of different nationalities to kindle in such a manner that it at last broke out in a raging disease, poisoning the whole constitution; a disease which, previously to the expedition to Naples was unknown throughout Europe. Simon finally concludes, that the morbus gallicus, syphilis, or lues venerea, having proceeded from the contagious leprous genital affections, is the offspring of leprosy, and may under certain circumstances again become its parent."

Dr. Lindwurm contends, on the other hand, that Dr. Simon's arguments prove rather that the origin of syphilis is not to be traced, but that the disease has existed in the most distant ages, and as far as our historic knowledge reaches :—

"If primary syphilitic affections of the genital organs occurred so frequently in ancient times, as would appear from the evidence of the oldest medical and non-medical writers, and if, on the other hand, the old leprosy bears the strongest resemblance to our constitutional syphilis, it is certainly simpler and more natural to assume an inverted causal relation between the two, as exists at present, to bring primary syphilitic affections into causal relation to leprosy, than gratuitously to assume an unproved, inexplicable transformation of leprosy into the so-called morbus gallicus—that is, to consider primary syphilitic affections as local reflexions and as results of lepra. The writer is far from considering the leprosy of the ancients to be exactly constitutional syphilis, but he is firmly convinced that it was for the most part of syphilitic origin, just as the investigations of Boeck, Sigmund, &c., have shown that such forms of disease as scherlievo, falcadina, radesyge, &c., hitherto referred to lepra, and considered to be peculiar endemic affections of the skin, are really syphilitic cutaneous diseases.

"What great difficulties do we often, in the absence of the history of the case, experience in the diagnosis of an inveterate syphilide; and the ancients had no assistance whatever from the history of the case, as they were unacquainted with chancre as a cause of syphilitic cutaneous diseases. To them the connexion between primary and constitutional syphilis was unknown, they would therefore look upon an extensive syphilide as an independent disease of the skin. If we add to this, that many writers of the middle ages looked upon genital affections as precursors of leprosy, and impure connexion as a cause of the same; that several speak of the transition of venereal disease into leprosy; that, moreover, in leprosy affections of the bones, ulcers of the throat, destruction of the nose, alopecia, falling off of the nails, &c., occurred in severe cases, we may well consider it more than probable that the leprosy of the ancients, like the spedalskhed, radesyge, &c. of the present day, in great part belonged to constitutional syphilis; that syphilis consequently was known in the most remote periods, and that the conversion of lepra into syphilis is unnecessary and untenable. The fact is simply, that from the time of the notorious Neapolitan epidemic, the course of syphilis became more malignant and more violent, but that we are not justified in dating the *origin* of syphilis from it, the less so as the history of this epidemic is involved in great obscurity."

From the foregoing it is evident that both writers agree in considering that a certain relation existed between syphilis and the ancient leprosy, and that they differ only as to which was cause and which effect. Dr. Lindwurm is of opinion that leprosy was in fact a form of constitutional syphilis, consecutive to the virulent genital affections which existed prior to the outbreak of lues venerea at Naples in 1495; Dr. Simon, on the contrary, holds that these

"Virulent affections were originally of a leprous nature, and were a local reflexion of the ancient leprosy, and that from them, as they increased in virulence and independence towards the end of the fifteenth century, under favourable circumstances, constitutional syphilis proceeded as a new and previously unknown disease. And further that the foul diseases of the genitals had an independent character, and are not to be considered as primary symptoms of leprosy, is apparent from the fact, that while leprosy decreased remarkably in the fifteenth century, the malignity and frequency of foul genital ulcers, to judge from the reports of the surgeons of that period, progressively increased. The inference that the leprosy of the ancients for the most part belonged to constitutional syphilis, because in it too we meet with affections of the bones, ulcers of the throat, destruction of the nose, alopecia, &c., is not," according to Dr. Simon, "legitimate, because these symptoms in leprosy were of quite a different character and ran a different course, from those attendant on the more modern syphilis."

One of the strongest arguments brought forward by Dr. Simon against Dr. Lindwurm's views is probably this: that, admitting that leprosy in some particular cases commenced with genital symptoms, it would still contrast decidedly with constitutional syphilis, which almost never, or only in doubtful exceptions, occurs without such previous symptoms. Dr. Simon denies that he assumes, as many do, a metamorphosis of lepra into syphilis at the end of the fifteenth century; and states that he simply deduces the latter, as a contemporary of its outbreak (Vella) so decidedly and clearly expresses himself, from the genital ulcers, which existed and were known from time immemorial.

While we have felt it our duty to animadvert in strong terms upon the undignified tone of irony adopted by the author in the works now noticed, we must cheerfully award to him the meed of praise to which he is so fully entitled, for the learning and research he has displayed in the investigation of his subject. We agree with him, that while there can be no doubt as to the existence from time immemorial of local affections analogous to those we now term venereal, there is no distinct evidence of constitutional syphilitic contamination having been observed prior to the close of the fifteenth century. That the local affections alluded to proceeded, in many instances, from intercourse "cum muliere fœda, sive leprosa," is extremely probable; and it is, in our opinion, most unlikely that the leprosy of the ancients was a secondary disease, the result of impure connexion. If, therefore, it be admitted that the above-mentioned local affections and leprosy were allied, we are inclined to adopt Dr. Simon's rather than Dr. Lindwurm's view of their relative positions as cause and effect. In the absence of all exact knowledge as to the real nature and symptoms of the ancient leprosy, it is extremely difficult to say how far the forms of disease known in the present day as syphilitic lepra may resemble it; if we may suppose that some analogy exists between them, syphilis would thus become, to some extent, as Dr. Simon terms it, "the offspring (*Tochter*), and again the parent (*Mutter*) of leprosy." It is, however, but fair to the author to state that he promises to investigate this part of his subject more fully in a continuation of his critical history. It is to be hoped that in his future work he will avoid those petty personalities which mar and weaken his past performances.

Review IV.

1. *The British Soldier in India.* By Fred. J. Mouat, M.D., F.R.C.S., Surgeon H.M.'s Bengal Army, and Inspector-General of Gaols, Bengal.—*Lond.* 8vo. pp. 87.

2. *Report on the Gaols of the Lower Provinces of the Bengal Presidency for* 1857–58.—*Calcutta,* 1858. 4to, pp. 213. And for 1858–59. —*Calcutta,* 1859. 4to, pp. 275. By Fred. J. Mouat, M.D., F.R.C.S.

3. *Notes on the Despatch of Troops by Sea.* By CHARLES J. KIRWAN, Esq., L.R.C.S., Assistant-Surgeon H.M.'s 13th Light Infantry.—*Calcutta*, 1859. 8vo, pp. 61.

4. *Sketch of the Medical Topography or Climate and Soils of Bengal and the North-Western Provinces.* By JOHN MCLELLAND, F.L.S., F.G.S., Surgeon H. M. Bengal Service.

5. *The Indian Annals of Medical Science for July,* 1858.—*Calcutta.* 1858. 8vo, pp. 450. *And for January,* 1859.—*Calcutta,* 1859, 8vo, pp. 370.

THE recent disturbances in India have certainly had the effect of rousing the attention of persons in this country to the condition of the British soldier in India, and of stimulating the medical officers of the army to investigate, with renewed energy, the causes of the great disease and mortality which prevail among European troops in that country, and to suggest means for their alleviation and prevention. We have already had occasion to allude to several able works on the diseases and hygiene of our Indian army,* and we purpose to devote the present article to several additional memoirs bearing upon the same subjects, and to the consideration of certain diseases as observed in India.

The object of the work at the head of the list is to point out the hygienic wants of the British soldier in India, and the best means of remedying them. Few of the suggestions which it contains are very original, and, indeed, the whole pamphlet is little more than an epitome of the larger work of Dr. Julius Jeffreys, already brought before the notice of our readers. Its very conciseness may, however, ensure a greater share of attention from those for whom it is mainly intended, than it would otherwise have received. The observations are made under the following heads :—1. Clothing and equipment of the European soldier. 2. His arms and accoutrements. 3. His barracks. 4. Can he be acclimated, and if so, how and to what extent? 5. His food and drink. 6. His wife and children. 7. His parades and punishments.

Under the head of clothing, Dr. Mouat most justly condemns the use of collars, stocks, and the whole tribe of ligatures which press on the large bloodvessels of the neck, as not only great nuisances in the sweltering heat of India, but as having no small share in the production of sun-stroke or heat-apoplexy. The beard of the soldier, which should never be removed, is the best protection for his throat against heat and cold, or sun and shade. With regard to the colour of the uniform, attention is drawn to the recent experiments of M. Coulier, Professor at the French Imperial School of Military Medicine, whose researches show that, for under garments, the colour is of no consequence, but that for coats and tunics the heat-absorbing colours should be discarded, and that the nearer the approach to white the cooler will be the dress, whatever the fabric. It is singular, and at the same time satisfactory, that the best colours for stragetic purposes are those most suited to

* British and Foreign Medico-Chirurgical Review for Jan. 1859, p. 110; and for Jan. 1860, p. 175.

maintain health in the field. The very worst, hottest, and most uncomfortable colours that could have been selected, are the dark blue of the artillery, the dark green of the rifles, and the dark Saxon blue of the new Indian light cavalry. The old silver grey of the native cavalry, in Dr. Mouat's opinion, was one of the best that could have been pitched upon, and ought not to have been changed. Dr. Mouat, like other writers, advocates the propriety of the under clothing consisting of flannel, as best suited for absorbing the perspiration, without causing any immediate loss of animal heat. The loose, puckered pantaloon of the Zouave, with its pockets, strap and buckle, is recommended as far superior in many ways, to the tight German fit of the trousers still worn in the British army. Dr. Mouat agrees with all parties in condemning the shako, bearskins, and brass helmets. The forage cap, with a French peak, and a quilted white cover, as recommended by the Home Commission, and by Dr. Chevers, is objected to, as perfectly inefficient for protecting from the direct rays of the sun. The complicated helmet of Dr. Jeffreys is also condemned, on the ground that the tin, upon which the free radiation and rapid removal of heat depend, could never retain its polish, while the whole thing would soon fall to pieces in the rough treatment of a campaign. Dr. Mouat predicts that the most efficient head dresses will yet be made of aluminium, which is as light as cork, as strong and malleable as iron, receives as bright a polish as iron, is as indestructible as platinum, and will be inexhaustible in its supply. In the meantime a good head-dress for the British soldier serving in India is still a desideratum.

Millions of money have been expended on the barracks of India, and still we are without a model lodging for the British soldier. This is most justly attributed to an unwise neglect of sanitary laws, and to a disregard of the opinions of medical men, who alone are capable of giving sound counsel upon such matters. All experience shows that in malarious countries soldiers ought never to be allowed to sleep upon the ground floor. Dr. Mouat, however, objects most strongly to the piling of story upon story, as has been done in the palaces of Fort William, and affirms that the most healthy Indian barracks he has ever seen are those of Moulmein, which consist of a number of straggling wooden barns, raised some feet above the ground upon wooden posts. The necessity of thorough ventilation and abundant space for respiration is urgently insisted on.

Dr. Mouat expresses himself very plainly on the subject of acclimatisation. The Saxon and the Celt, he says, are utterly unfitted for colonizing the plains of India. They could not propagate a healthy, vigorous race, with the physical and moral powers of Europeans; and those who did not fall a sacrifice to the climate would soon degenerate. The whole tribe would disappear in the third or fourth generation. This question may be looked upon as finally settled, by the concurrent testimony of all observers most competent to form an opinion. At all elevations, however, above three or four thousand feet, the European can live and thrive as vigorously as in Europe, and thus counteract the temporary injury his constitution may have sustained in the plains. The propriety is maintained of forming permanent establishments in the hills for the wives and families of the British soldier. The injurious moral effects of the present barrack system are pointed out, and the necessity of affording to the married soldier the comforts and decencies of a home are strenuously urged. Some able remarks are made upon the present reprehensible system of education of the class of women destined to become soldiers' wives. It would seem that in the Lower Orphan School and in the European Orphan Asylums of Calcutta and Madras, the children, in place of being taught needlework, cookery, reading, writing, and arithmetic, and the domestic duties of a wife and mother, are instructed in subjects which might be expected in a London boarding-school. Dr. Mouat says he has often heard steady soldiers declare that they preferred an uneducated native wife, to the best of the inmates of the institutions just mentioned. The former, they said, was gentle, quiet, obedient, fond of staying at home, very careful of her children, and anxious to minister to the comfort and happiness of her rough companion; whereas the latter was far too often a fine lady, alike regardless and ignorant of domestic duties, fond of gossip and flirtation, and altogther ill calculated to produce happiness in her husband's household.

There is one remark which Dr. Mouat makes under the head of Punishments, with which we entirely concur. The ordinary duties of the soldier should never be made the means and instrument of correcting his errors and punishing his vices. Penal drills and parades are serious inflictions on the steady, well-conducted non-commissioned officers who superintend their execution, and generate a distaste for the very duties on which the real efficiency of the soldier depends.

Dr. Mouat agrees with Sir James Annesley, Dr. John Macpherson, and many others, in denouncing the practice of sending growing lads out to India on service, and insists on the necessity of their being thoroughly trained and drilled in England prior to embarcation for India. The remarks under this head are well worthy of perusal.

We have little to say upon the two bulky volumes of Reports on the Gaols of Bengal. They contain an immense mass of statistics, which have little medical interest, and into which it is extremely doubtful if any one will enter save the compiler. Every page savours strongly of red tape, and the object would appear to have been to swell out a report, rather than to impart useful knowledge, or to arrive at important conclusions from the means of information placed at the writer's disposal. From a medical inspector with Dr. Mouat's acknowledged talents and zeal for investigation, we might have expected something more original and practical. The volumes before us constitute an imposing, but, we fear, perishable monument of his industry.

Diarrhœa and dysentery would appear to be the most destructive diseases among the native prisoners. These affections, we are told, are susceptible of considerable diminution, by increased space, improved drainage, better ventilation and sewerage, a dietary more suitable to persons in confinement, and warmer winter clothing for the aged and sickly. Apart from such general observations, we find no specific information as to the etiology of dysentery, or, indeed, of any other disease. From instructions issued to the local magistrates, it would appear that the view is adopted according to which cholera is communicable by the dejecta from the bowels, but no facts are given in corroboration of this opinion, although no one can have more ample opportunities for observation on the matter than Dr. Mouat.

Mr. Kirwan's little book consists of a series of regulations for the prevention of sickness among troops despatched by sea. Many of the suggestions are of considerable value, and are well deserving of attention. One of the most original is the proposal to introduce a class of ships solely devoted to the transport service. Mr. Kirwan justly observes that few merchant vessels are constructed with the object of carrying large bodies of men, and that much sickness among the troops may be traced to this cause. Many important hints are thrown out regarding the arrangements and appliances necessary on board troopships; and the instructions generally throughout the volume are, for the most part, highly judicious.

Dr. McLelland's work on the Medical Topography of Bengal and the North-Western Provinces, is perhaps the most interesting on our list in a medical point of view. It affords an admirable illustration of the *practical* advantages which may be derived from members of our profession having some knowledge of those natural sciences, which at first sight appear to be related in no way to medicine. If any of our licensing boards were to *insist upon* their graduates being examined in geology and mineralogy, we suspect there would be at once an outcry to the effect of "What can these sciences have to do with practice? Has not the student quite enough to learn already?"* We point to Dr. McLelland's work as an instance where a knowledge of geology and mineralogy has served to throw a flood of light upon the etiology of a very important and obscure disease, and we would express our conviction that some knowledge of these sciences is absolutely necessary to the many members of our profession, whose especial duty it is to investigate the causes and mode of prevention of disease.

A good many years ago, Dr. McLelland published a series of observations which he had made in the Kemaon district of the North-Western Provinces, and which showed that an intimate relation subsisted between the occurrence of limestone in the sources from which the inhabitants derived their drinking-water and the prevalence of goître and cretinism. The work appeared in India, but

* It is worthy of notice that in 1859, in an order from the War-Office, dated Oct. 7th, the competitive examination of Candidates for commissions in the army medical service embraced, in addition to other subjects, Natural History, Botany, and Physical Geography, including Meteorology; and at that time a knowledge was required of such works as Carpenter's 'Zoology,' Rymer Jones and Milne-Edwards' 'Natural History,' Somerville's 'Physical Geography,' Kemptz's 'Meteorology,' and Lyell and Page's 'Geology.' In a subsequent order from the War-Office, March 24th, 1860, a modification of the above is observable; for, although a knowledge of Botany, Comparative Anatomy, and Zoology is demanded, Geology and Meteorology are not included with the other necessary subjects; and it is stated that "candidates who may desire it may be examined in the Elements of Physical Geography," for which purpose the 'Elements of Natural Philosophy,' by Golding Bird and C. Brooke, and the 'Physical Geography' of Mrs. Somerville, are recommended. We are given to understand that the examiners for Indian medical appointments are always anxious to obtain candidates whom they can specially recommend and report upon for their knowledge of Geology and other physical sciences. On this point the published examination papers may be consulted.

has been long out of print, and few copies ever reached England. Indeed, Dr. Watson, to whose admirable lectures the profession is entirely indebted for a knowledge of the circumstances,* says that he had never been able to obtain the book, and that he was indebted for his information to an article which appeared in the fifteenth volume of the 'British and Foreign Medical Review.' For these reasons, Dr. McLelland has wisely determined to republish his original observations in Kemaon, to which he has appended the results of similar inquiries, in which he has more recently been engaged in the plains of Bengal.

While occupied in collecting information on the climate and vegetation of Kemaon, Dr. McLelland was struck with the frequency of goître in one portion of the district, while the other was almost perfectly exempt from the complaint, and he was naturally led to investigate the circumstances by which these districts were distinguished from one another. He visited 126 villages scattered promiscuously over an area of upwards of 1000 miles. Five of these villages were built upon hornblende and mica slate, or on siliceous sandstone, or on green sandstone; they contained 290 inhabitants, not one of whom was a cretin or affected with goître. Seventy-one of the villages, with a population of 3957, were built upon clay slate, and in these there were only 22 cases of goître, or scarcely more than one in 200, and there was not a single cretin. Lastly, thirty-five of the villages, having a population of 1160, were built upon alpine limestone, and here 390, or more than one-third of the inhabitants, had goître, while thirty-four, or about one person in every thirty-five, were cretins.

Moreover, it was ascertained that every village is not equally affected in the same neighbourhood, but that some are quite exempt, while others are affected to the extent of half their population. This difference was found not to depend on any accidental or transitory cause, for the disease always affected the inhabitants of a particular village, while those of adjoining hamlets continued perfectly and permanently free from the complaint. Closer investigation showed that the circumstances in these cases confirmed in a remarkable manner the view, which it is Dr. McLelland's object to establish. The following may be cited by way of illustration:

Panorah is a village which contains seventy Brahmins and twenty Domes; of the former, one only is affected with goître, while six of the latter have large tumours. The Brahmin or high-caste inhabitants of this village derive their water from a spring in clay slate; and as the prejudice of the Hindoos denies to Domes the privilege of partaking of the water of the same spring, the excluded caste are forced in this, as in many other parts of Kemaon, to obtain their water from what they, as well as the Brahmins, believe to be impure sources. In this instance it is taken from a stream that issues from the same limestone rocks which supply water to the neighbouring villages of Bajeetee and Popdeon. Popdeon is only a mile distant, and Bajeetee a mile and a half. In these two villages both classes of the inhabitants use the

* Lectures on the Principles and Practice of Physic, third edition, 1848, vol. i. p. 786.

limestone-water, and here the Brahmins are not exempt. Thus Popdeon has a population of eighty, of whom fifty belong to the higher and thirty to the lower caste. Of the former, eight, and of the latter, ten, are affected. Again:

"Deota is a lengthened village, which occupies half a mile of the foot of Durge mountain. One extremity of it is inhabited by Brahmins, the other by Rajpoots and Domes. Of the first caste there are about twenty persons, all of whom are free from goître; of the second, there are forty, of whom two-thirds are affected more or less; and of the third class, nearly the whole are affected, forty in number: so that, including the Brahmins, there are only about forty persons in this village exempt from goître out of a population of one hundred. To what cause can we ascribe the immunity of one caste of the inhabitants of this village, and the almost entire affection of the other two castes? They are all alike well-fed, and have little toil, their land producing the requisites of life almost without labour. Difference of caste does not here imply a difference in pecuniary circumstances, and consequently of the comforts of life. In these respects the three castes in this village are on a perfect equality; nor will hereditary predisposition, acquired by intermarriages between affected parties, be sufficient to explain the interesting fact, for the affected parties are confined to the castes of Rajpoots and Domes, who cannot intermarry, while the Brahmins and Rajpoots may.

"The village is raised about one hundred feet above the level of the valley, and the mountain, at the foot of which it is situated, rises with a gentle slope, and is not in this vicinity at all rugged. It is chiefly composed of compact limestone; and the village is erected on a conglomerated rock composed of calc tuff, inclosing masses and fragments of other rocks. There is a spring situated in the valley, at the distance of about a hundred yards from the village, which from its first appearance has the character of a mineral spring. The water bursts forth with strong ebullition from numerous veins, in the quantity of at least forty gallons a minute, and communicates adhesive properties to the sand and gravel by which it is surrounded. The temperature and quantity of the water are the same at all seasons.

"The former inhabitants of this village, aware perhaps of the noxious effects of the spring, had an aqueduct formed, by which water is conveyed into the Brahmin portion of the village from a distant source. The aqueduct having been allowed to get out of repair, the quantity of water it transmits is reserved exclusively for the Brahmins; but during the rainy season, when water is plentiful, the Rajpoots also use the water of the aqueduct; but the Domes have no alternative at any season but to use the water from the spring."

Within a mile of this village of Deota, stands the village of Ninee, with eighty inhabitants, all Rajpoots, not one of whom is affected with goître. The village is built on clay-slate. Sunn is a village, three miles from Deota, inhabited by ten Brahmins, five of whom have goître. This village is watered by a stream which descends from the limestone rock.

Again, Ager and Ducygong are two villages situated within half a mile of each other, in the valley of Barabice. The residents in Ager use the water as it issues from the drift of an old copper-mine, situated in the limestone, whereas Ducygong is supplied by water from a spring in clay-slate. The former village contains fifty inhabitants, forty of whom have large goîtres, and twenty of them are cretins; the latter village has the same number of inhabitants, not one of whom is affected in either way.

Again, many instances were observed of persons who, having changed from a healthy to an unhealthy village, became subject to goître, while in others, the tumours of those affected became stationary, or even disappeared entirely, during a residence in a healthy village.

Chemical examination of the water used by the inhabitants of the different villages confirmed the conclusions drawn from observation of the sources from which it was derived. The water drunk by the inhabitants who suffered from goître, was found to contain a large quantity of carbonated lime, whereas that derived from the clay-slate rock, and which was drunk by the inhabitants who did not suffer from goître, contained none.

High elevation above the sea, and the drinking of snow-water, have been thought by some observers to have something to do with the generation of goître. But both goitre and cretinism are met with in the low burning plains of Bengal, as well as in the hilly districts of Kemaon, with an elevation of 4000 feet. Moreover, the circumstances under which these affections are met with in the plains, were found by Dr. McLelland to form a striking corroboration of his previous observations in the hills of Kemaon. Goître and cretinism are very prevalent in different parts of the district of Goruckpore. The soil of this district is of two sorts. One, to which the natives apply the name of *Bhat*, characterizes the lands bordering the river Gunduk and its branches, and is remarkable for the large proportion of calcareous matter which it contains. One specimen was found, on analysis, to contain upwards of twenty-five per cent. of carbonate of lime. Now, in the villages built upon the *Bhat* lands, goître and cretinism are very prevalent. In some of them ten per cent. of the population are affected, and of the children in villages where goître prevails, ten per cent. are cretins. Dogs and cats also in these districts are often affected with goître. On the other hand, the lands on the banks of the Gogra consist of a soil which the natives designate *Bangar*, and which differs remarkably in its general characters from the soil of the *Bhat* land, being much less retentive of moisture, and requiring irrigation for the production of winter crops. This *Bangar* soil is very siliceous, and contains scarcely any lime, in some cases not more than one part of carbonate of lime in five hundred. In the villages built upon this soil goître and cretinism are unknown.

An experimental inquiry into the causes of goître could not have been planned in a better manner, or been crowned with more decisive results. These observations of Dr. McLelland's show that neither the atmosphere, elevation above the sea-level, the physical aspect of the country, nor locality, have anything to do with the production of goître ; but they prove almost to demonstration that this affection is due to the presence of a large proportion of lime in the drinking water. This view is in accordance with that of others who have made observations upon goître in our own country. The only districts in England where goître is at all prevalent, are some parts of the counties of Nottingham and Derby, where the geological formation is limestone, and where the water is notoriously *hard*, and yields on analysis large quantities of either the carbonate or sulphate of lime.

The circumstance that goître and cretinism are intimately connected in their origin with the absorption of lime into the system, acquires an additionally important signification from the facts bearing upon the post-mortem appearances found in cretins, which were communicated by Professor Kölliker to the meeting of the Swiss Association at Basle, in 1856. The substance of Kölliker's paper was communicated to us by a gentleman who heard it read, and is alluded to by Dr. McLelland. We have not been able to meet with a published copy of this memoir, and we have been not a little surprised that no abstract or notice of Kölliker's observations has appeared in any of our English journals. Kölliker found that in cretins the base of the cranium becomes at an early period unusually ossified and thickened, with the effect of narrowing, or even obliterating, the foramina through which the nutritious arteries pass to the brain. Now it appears justifiable to connect the unusual quantity of lime taken into the system, with this ossification at the base of the brain as cause and effect. Why the lime, which is absorbed, should select by preference this portion of the skeleton it might be difficult to say, but we have no evidence before us to the effect that it does. It remains for future observation to show whether all the bones of the skeleton do not contain an unusual amount of lime. Be this as it may, the narrowing of the orifices through which the arteries pass to the brain, appears to offer a rational explanation both of the goître and the cretinism. On the one hand, the diminished supply of arterial blood to the brain interferes with the normal nutrition and functional activity of this organ; while on the other, the obstruction to the onward flow of the blood might have the effect of directing a larger quantity towards the thyroïd, with the effect of stimulating its growth and increasing its size. There are at present few questions in pathological anatomy the investigation of which holds out such hopes of being attended by important results, as that of the appearances found after death from goître or cretinism.

Dr. McLelland's work contains many other valuable observations on the medical topography of Bengal and the North-Western Provinces, and on the influence of soils in the production of disease; but the most important are those to which we have directed attention.

The two numbers of the 'Indian Annals of Medical Science' at the head of our list contain several memoirs of no small interest and value, upon various subjects connected with the etiology, pathology, and treatment of disease, and with operative surgery. Of these, by far the most important is Dr. Norman Chevers' Review of the Means for preserving the Health of European Soldiers in India, which we have already had occasion to notice.*

Dr. Payne publishes some very interesting observations on the occurrence of pigment in the urine, in connexion with diseases of the liver. The cases which he has collected appear to show that when the

* British and Foreign Med.-Chirurgical Review, January, 1859, p. 115.

function of the liver is not duly discharged, whether the organ be itself primarily affected, or whether it be involved consecutively in the course of some other disease, an increased proportion of pigment in the urine will generally bear evidence to the fact. This increased amount of pigment, he thinks, may be taken as positive evidence of hepatic derangement, even when there are no other corroborative symptoms; while, on the other hand, the fact of there being no increase of the urinary pigment affords a very strong presumption that the liver is healthy. The pigment alluded to is not that of bile, but what is commonly designated *purpurine*, which recent observations would show to be probably derived in some way from bile pigment. The urine which contains it is often passed of a natural colour, or even paler; and hydrochloric acid, the test proposed by Dr. Golding Bird, will often fail to make the pigment apparent. Nitric acid, according to Dr. Payne, is a much better test. When a drop of this acid is added to the boiled urine, the tint which it assumes is sometimes indigo blue, at other times smoky brown, both finally passing into a distinct red. Dr. Payne's investigations have led him to the belief that in all diseases such as pulmonary and renal affections and dysentery, when this pigment is increased in the urine, some derangement of the liver is to be suspected. The importance is dwelt upon of looking for this pigment in cases of dysentery, as a means of throwing some light upon the pathology of the individual case, and of indicating the probable benefit to be derived from a mercurial treatment. The whole subject is one of great importance; we trust that Dr. Payne will prosecute his researches, and we would recommend to his notice the recent work of Professor Frerichs, of Berlin, on 'Diseases of the Liver,' a translation of which is being issued by the New Sydenham Society. He will find there much new matter on the subject to which he has been devoting his attention.

Dr. Edward Goodeve publishes a clinical lecture on Typhoïd Fever, delivered at the Medical College Hospital of Calcutta. The lecture contains the histories of seven cases of typhoïd fever which had come under Dr. Goodeve's observation in Calcutta. None of these cases proved fatal, so that no opportunity was afforded of ascertaining positively the existence of disease of Peyer's patches. The entire train of symptoms, however, including the eruption of rose-coloured spots, left not the slightest doubt that the cases were genuine instances of typhoid fever. Cases of this fever in India, in which the characteristic lesions of Peyer's patches were detected after death, were published several years ago by Drs. Scriven and Ewart, of the Bengal Medical Service. Typhoïd fever must therefore be regarded as, without doubt, one of the endemic diseases of India. On the other hand, there is no good evidence that true typhus is met with in the tropics. Mr. H. M. Greenhow, it is true, speaks of "true typhus" fever having been observed in both the European native garrisons of Lucknow. The only case, however, of which he records the symptoms, was more probably an example of typhoid fever. It is true that we often hear of typhus in India, but most instances of reported typhus have been examples of malignant remittent fever, which not infrequently presents a train of symptoms bearing a striking resemblance to the typhus of our own country—dry, brown, retracted tongue, dorsal decubitus, low, muttering delirium, contracted pupils, and petechial spots. These are merely, however, the train of symptoms which may be said to make up "*the typhoïd state,*" which exists in its typical form in true typhus, but which may be developed in the last stages of most acute diseases. During a considerable experience of fever in India, we did not meet with a single instance of true typhus, with its characteristic eruption, and we have never read the account of a case recorded with sufficient accuracy and detail to satisfy us that it really was typhus.

Dr. John Shortt records a case in which death occurred suddenly, owing to rupture of the superior vena cava within the pericardium and extravasation of blood into the pericardial sac. The exact pathological nature of the lesion which led to this rupture is not very apparent. All that we are told concerning it is, that the serous lining of the vein for half an inch around the rent was destroyed, while the surrounding tissues were infiltrated with blood. The rent itself was circular, and of about the size of a shilling.

A case of accidental poisoning by strychnia, in the person of a member of our profession, is recorded. The quantity swallowed amounted to two grains. It was made up in pills which were taken in mistake for laxative pills. Within three hours, the patient died. There was nothing remarkable in the symptoms or post-mortem appearances. Decided evidence of the presence of strychnine was found in the stomach after death.

Review V.

A Treatise on Vital Causes. By James Newton Heale, M.D., &c. *London,* 1859. 8vo, pp. 289.

This is a work of no ordinary pretension; the author seeming really to imagine that he has rectified many pernicious errors in physiology, and established new and important facts and doctrines in their place. But it does sometimes happen that a writer finds it necessary to prove every one else wrong in order to make room for showing that he is right himself; and it does also sometimes happen that a writer who possesses more fluency of language than strength or originality of conception, contrives to dress up, in pompous phraseology, commonplaces so obvious that, as no one would dispute, so no one has thought it worth his while particularly to insist upon them, and which may thus, at first sight, appear to present an aspect of novelty. The chief peculiarities of Dr. Heale's work seem to us to be derived very much from these two sources.

The first chapter relates to "Mechanism in General." We here meet with a profusion of similes, which the author seems to mistake for scientific analogies, and among which those derived from the steam-engine make a conspicuous figure. We shall perhaps be excused for passing by these curious illustrations.

Chapter II. relates to Inorganic Operations. The views intended to be conveyed are drowned in a superfluity of words; nor do they appear to be of sufficient importance to encourage much labour in their resuscitation. At p. 18, we are informed that motion, momentum, fermentation, decomposition, growth, and sound, "are sometimes called imponderable matter." We should be glad to know when, and by whom. We also learn, in the same page, that fermentation, decomposition, and growth, have relation to time only, and not to space. We had always imagined that growth in particular involved, in its very essence, the occupation of more space than was occupied before.

Chapter III. is on "The Signification of the word Life." It would be superfluous for us to dwell on the definitions and disquisitions herein contained; but we cannot refrain from animadverting on such expressions as the "oscillations" of "the entity" (p. 1.) This becomes still more absurd when contrasted with the "phases" of the "vital pendulum" (p. 114): the entity is made to assume an action peculiar to the pendulum, while the latter, whose express business it is to oscillate, presents us with "phases."

Chapter IV. is on "Organic Operations: their analogies and contrasts." We here find certain comparisons between animal and vegetable functions, which the author reverts to, and enlarges upon, in Chapter VIII., where we shall have occasion to notice them.

We pass over Chapter V., "On the Function of Breathing in its relation to the Circulation," and proceed to Chapter VI., "On the Changes which the Blood undergoes." This contains some very remarkable matter. Dr. Heale appears to have mystified himself in an extraordinary manner concerning the relations of the pulmonary to the systemic circulation. He says—

"It will be well to explain how consecutive events, like those which occur in the capillaries of the lungs and of the system, can also be co-instantaneous." (p. 111.)

The Doctor would indeed be clever if he could explain this; for the terms *consecutive* and *co-instantaneous*, are, under every possible circumstance, in direct and irreconcileable opposition to each other. He continues, however—

"The changes in the lungs whereby the venous blood is altered into arterial, are consecutive to those by which the same arterial blood in the system is converted into venous, and these also are subsequent to those by which it was before made arterial; but notwithstanding these admitted facts, it still remains indubitable that the transition of one part into arterial, and of another into venous, is perfectly synchronous, and in obedience to one operation, in which both participate. An illustration will, perhaps, explain this. Two players at ball may be supposed to exchange, by simultaneous throws, two balls from one to the other. A throws a ball to V at the same time that V throws one to A; A catches V's ball at the same time that V catches A's: the balls alternate in each segment of the circle, but the throws and catches are simultaneous. Now, instead of the balls being thrown from one to the other, let it be supposed that each player rolls them through a powder of a different colour. V rolls his blue ball through a red dust, and therefore A receives the ball changed to a red colour; but A at the same time rolls his red ball through a blue pigment, and V receives it tinged with that colour; and so they continue to receive each a ball of his own colour. V always receives a blue ball, and A always receives a red one. The right auricle and ventricle are represented in this simile by V, who receives the blue-coloured ball from A, representing the left side of the heart, and who has rolled it through the capillaries of the system, whereby it has become dyed of a blue colour; and V rolls it back through the capillaries of the lungs, and in so doing renders it red. But, as two balls have been used simultaneously, it results that, at the same moment while A has been rendering a red ball blue, V has been changing a blue one to red; the transition from blue to red, and from red to blue, has been isochronic, and there have constantly prevailed a red ball and a blue one in opposite segments of the circle of rotation." (pp. 111—13.)

We have almost to apologise to our readers for taking up their time and attention with such a passage as this; but we have given it as a specimen of the 'ineptiæ' with which Dr. Heale's book abounds. The facts of the case, namely, that while one portion of the blood is undergoing a certain change in the lungs, another portion is simultaneously undergoing a different change in the systemic capillaries, are plainly enough intimated at the commencement of the foregoing extract; but our author, in his overstrained attempt to make something new out of so familiar a matter, has stumbled upon what is vulgarly called "a mare's nest;" and if we were to liken his blue and red balls to eggs contained therein, we should be using a simile quite as apposite as some which are to be found in his own pages.

Chapter VII., "On the various Systemic Operations," is a rambling disquisition on many points of physiology. The principal notions that run through it, and serve in some sort to connect its heterogeneous materials, appear to be the following: first, that although the blood visits every part of the living system, and is propelled through all by the same heart, each organ, or system of organs, which ministers to a special function has, as it were, an isolated circulation of its own, receiving blood through its own arteries, and returning it through its own veins: thus there is one circuit through the structure of the heart itself; another through the tissue of the lungs; a third through the brain and nervous centres; a fourth through each kidney, and so on; secondly, that the blood returned from these systems and organs, is of necessity variously affected in its chemical composition and vital properties by the changes induced by the particular functions to which it has ministered during its course; thirdly, that it is not until the blood has received an accession of new materials from the contents of the thoracic duct, and undergone the changes effected by respiration, that it becomes once more adapted to supply the wants of the system at large. The author seems to insist on a more complete vascular isolation of the different systems than an appeal to anatomy would warrant—otherwise, all we have to say of his view of the subject is, that it is perfectly true—so obviously true, indeed, that the exposition of it was quite unnecessary.

Chapter VIII., "On Causes conducive to Vitality," is the most important in the book, since it is here that the author enters most fully into the views in which the peculiarity of his opinions seems chiefly to reside.

These are, firstly, that the vital force is generated during the changes effected in the blood by respiration; and, secondly, that this force is instantaneously propagated to every part of the system by virtue of a certain polarity in the blood-discs; and that such propagation of the vital force causes a perfect consent and unity of purpose between the actions going on in the pulmonary and in the systemic capillaries.

In relation to the first of these, Dr. Heale reverts to the vegetable character of the functions carried on by the absorbent system, which, however, here sometimes assume the less confident appellation of "quasi-vegetable." To these he refuses the application of the term *vital*—they are merely "conducive" to vitality, which makes its first real appearance during respiration. But what is there in all this, except a peculiar and evidently improper restriction of the terms *vitality* and *vital*, and an incorrect use of the term *vegetable*, as if it excluded the notion of vitality? We apprehend, therefore, that physiologists in general will continue to admit, what there is not the smallest ground for denying, that the functions here called vegetable by Dr. Heale, are just as vital as respiration itself.

With respect to the second opinion—that of the polarity of the blood-discs, and the propagation of the vital force by means of these—it appears to be as good and no better than any other mere hypothesis. We do not find any tangible evidence adduced in its favour. Dr. Heale, indeed, speaks of a "spontaneous arrangement of the red corpuscles (called blood-discs) in a manner peculiar to themselves, such as in any other fluid would at once be called a 'polar state;'" and he regards this as "evidence of the transmission through the blood of a 'current' force, which is by no means identical with the mechanical one under the influence of the heart's pulsations by which its mere onward movement is governed."

But we do not exactly understand the nature of the peculiar arrangement of the blood-discs here alluded to, nor do we find throughout Dr. Heale's volume anything more explicit; we are at a loss, also, as to the meaning of the following:

"The rapidity with which the nerves telegraph backwards and forwards the regulating influence belonging to them is very remarkable; but it bears no sort of comparison with that with which the vital force itself is transmitted through the vascular circle." (p. 156.)

The nervous influence is propagated instantaneously, or at least with a rapidity which is too great to be measured in reference to the short distances which it traverses. How, then, can the "current vitality" move with a rapidity beyond comparison greater than that which is instantaneous?

Chapter IX. and last, is headed "Views advocated in the foregoing Chapters contrasted with prevalent Doctrines." It consists chiefly of an attack on the theory of "secondary assimilation," which Dr. Heale thinks it not unlikely—

"Has only been adopted to meet the inaccurate requirements which *vivâ voce* lecturing calls forth; and thus, by the frequent repetition of symbolical expressions, the actual undiluted truth is in danger of being lost sight of." (p. 272.)

This is a vague and unmeaning sort of accusation as applied to the theory in question, but we cannot help thinking that if our author will look nearer home he may perhaps discover an undue frequency of symbolical expressions, and very great dilution of the truth. He accuses the advocates of this theory of ignoring the vitality of the blood, and maintaining that a man is a mere collection of polypes or corallines. (p. 274.) We cannot, however, perceive that the theory is in any way amenable to these censures.

On the whole, we wish Dr. Heale had not given himself the trouble of writing this book, as it appears to us to be most elaborately nothingful. We will not be so unpolite as to call it "a wind-bag;" but we suspect that Mr. Thomas Carlyle, if translated to our critical chair, might be apt to affix to it some such disrespectful designation.

Review VI.

1. *Archiv für Pathologische Anatomie und Physiologie und für Klinische Medicin.* Herausgegeben von R. Virchow.—1854–1859, passim.

Virchow's *Archiv.* 1854–1859.

2. *Die Cellular-pathologie in ihrer Begründung auf Physiologische und Pathologische Gewebelehre.* Von Rudolf Virchow. 1859.

Cellular Pathology, based upon Physiological and Pathological Histology. By R. Virchow. 1859.

3. *Journal de la Physiologie.* Publié sous la direction du Dr. E. Brown-Séquard. Nos. V., VI., VII.—1859.

Brown-Séquard's *Journal of Physiology.*

4. *An Inquiry into the Existence of Amylaceous Compounds in the Human Body.* By Dr. Bristowe and Mr. Ord. ('Transactions of the Pathological Society of London,' vol. x. p. 299.) 1859.

5. *On the Nature of the Substance found in the Amyloid Degeneration of various Organs of the Human Body.* By Francis Harris, M.D. Cantab. 1860.

If we are ever to arrive at a correct pathology, it must be by first ascertaining the symptoms and appearances of disease in relation with the particular organ affected by the light of physiological anatomy and physiology; and, secondly, by discovering those general alterations in the nutrition of the blood and tissues which give origin to what are denominated inflammations, degenerations, diatheses, and morbid growths, or in other words, to diseases of nutrition. The first part of this road to knowledge has been fairly opened up by the researches of the last ten years, but of the second part, the rough work of a few pioneers is at present all that can be found to indicate its onward direction. But though depraved nutrition and its consequences be yet most imperfectly understood, yet the advance made during the last few years in physiology and pathology, particularly by the aid of the microscope and chemistry, may well encourage us to hope to establish ere long some principles of general pathology having equal precision with the truths of natural philosophy. A step towards so desirable an end appears to us to have been gained by the determination of the morbid condition known as amyloid degeneration, a sketch of the pathology of which formed the subject of an article in our last number (vol. xxvii. p. 442). Moreover, we deem it a subject of sufficient importance to warrant its further examination. It opens up a new field of inquiry, full of interest, and very promising to the pathological student.

The principal results arrived at in the paper quoted, were: that there is a peculiar degeneration, the expression of a general pathological state, but, at the same time, one which more particularly affects certain organs; that this degeneration may be recognised by the physical changes it induces in the tissues involved, and by certain micro-chemical signs; that it is due to the formation of a substance, primarily found in connexion with the capillaries and small vessels, and recognisable by its appearance, but better by the microscopic alterations it induces; that this matter is suspected by many to be of an amylaceous nature; that its accumulation is sooner or later destructive of the nutrition and function of the invaded organs; and, lastly, that it has more commonly been found associated with certain diseased states, all agreeing in this particular, that they are productive of a general depraved or cachectic state of the body.

One important matter, however, remained unconsidered, or at least was only imperfectly touched upon—viz., the chemical nature of the substance which constitutes the pathological element of amyloid degeneration. An examination of this question will constitute the leading subject of the present article; and we would gladly have followed it by a discussion of the signs or symptoms which, on the assumption that the degenerative process in question has a special character as a pathological fact, are discoverable in relation with it during life. However, we regret that there is yet so great a deficiency of material for this purpose, that an attempt to elaborate the history of amyloid degeneration as a special lesion referable to the category of ordinary diseases, is impracticable. To the minds of some, therefore, the entire subject may appear like a barren fact in pathology unavailable to the practical physician. However, such a notion would be unfortunate as well as unfair, inasmuch as the pathological change in question is yet of very recent discovery, and time is required to make out the symptoms associated with, and indicative of it during life; and inasmuch as it moreover enlightens us respecting the nature of certain otherwise obscure lesions, and affords a most valuable clue to further research.

It has been usual to distinguish two varieties of amyloid matter; one, as it exists in the form of corpuscles, very generally laminated, like starch grains, and which we may call the corpuscular variety, as found in the ependyma ventriculorum of the brain, in the pulmonary tissue, and in the follicles of the prostate; the other, as found in tissues and organs which are in the often so-called state of waxy degeneration—for instance, in the waxy spleen, liver, and kidneys, where it is granular, and may be spoken of as granular or amorphous amyloid matter.

The chemical relations of the first, the corpuscular variety, were pretty fully discussed in the previous article quoted (pp. 443—449), and therefore will need consideration here only in their connexion with the question of the nature of the amorphous amyloid substance.

Though the presence of an amylaceous compound in animal tissue appeared on its first announcement scarcely credible, owing to the then-prevailing notion that such compounds were peculiar to vegetable structures, it is now a common-place fact, and has latterly been proved to have a very much wider signification than would have been anticipated. The existence of cellulose in the envelopes of the low grades of animal life represented by the ascidians and salpinæ, the first discovery of this class, has been followed up by the determination of the chemical alliance of chitine—the organic basis of the external skeleton of the crustacea, insects, and the like; and of tunicine, that of the envelopes of tunicata—with compounds of the amylaceous series. But this association of the allies of starch with the animal economy has been shown to be very much more extensive than is implied even in the above instances, by the discovery of the production of a substance, called indifferently glycogene, amyloid matter and zoamyline, in the process of nutrition of apparently all animals, including man; a discovery we owe to the genius and research of M. Bernard. This important fact was first promulgated in connexion with what M. Bernard concluded to be a sugar-forming (glycogenic) function peculiar to the liver. However, more recent researches, both by Bernard himself, by M. Rouget of Paris, by Dr. Pavy of Guy's Hospital, and several others, have proved that the sugar detected in the liver is not directly produced there, but owes its formation to post-mortem decomposition or to some catalytic force acting upon a true product of the economy, separable by chemical means as a white powder, and proved to be a near ally of starch;—a glycogenic substance of which the most convenient of the three names above cited seems to us to be "*zoamyline.*" Moreover, the production of zoamyline is no special function of the liver, but proceeds at every part of the system, though probably attracted, so to speak, in a higher degree to the hepatic tissue.

Now the production of an amylaceous compound in the normal processes of the economy furnishes of itself an argument for the probable formation, retention, or transformation of it as an abnormal condition; and indeed, in the phenomena of diabetes mellitus we have an example of the irregular transformation of the zoamyline into sugar, and of its apparent rapid production at the expense of the tissues at large. In this instance, the zoamyline loses part of its nature as an organic compound, becomes a waste soluble material, unfit for the purposes of nutrition, and is therefore rapidly eliminated. But we may suppose a change in it in a contrary direction—that it may become more organic, more associated, whether after the laws of pure chemical affinity or not, is of no consequence—with albuminous or nitrogenous material, and in some way less available for its normal purpose in the nutritive processes; and then, being insoluble in water, it could not be carried off like the sugar of diabetes, but would accumulate in the organs of the body, to the detriment and final destruction of their functions and normal structure. However, without pushing such conjectures further, let us examine the evidence put forward for considering the so-called amyloid degeneration due to an actual amylaceous compound, together with that which may be adduced against the hypothesis.

So far as its chemical basis is concerned, the doctrine of the degeneration of tissue into an amylaceous substance rests mainly upon the application of the common tests for starch and cellulose—viz., iodine alone or with the addition of sulphuric acid, or iodine and chloride of zinc. For besides the evidence thus furnished, whether for or against the opinion in question, we are acquainted with only one or two analyses, proximate and ultimate, which can be looked upon as in any way satisfactory or trustworthy. Carl Schmidt records two ultimate analyses undertaken by himself.* One was of a choroid plexus, rich, as it is reported, in amyloid bodies; the other was of a portion of waxy spleen: and his conclusion was that the matters thus examined were of an albuminoid nature, and contained nitrogen. This conclusion is also favoured by the only other ultimate analysis we have met with—viz., that by Friedreich and Kekulé, of which we shall have presently to speak in detail.

But to return to the tests for starchy matters, and assuming them to be satisfactory as such, there is a remarkable want of agreement in reference to the indications obtained by those tests among different observers. For instance, it is agreed on most hands that particles of tissue in waxy or amyloid degeneration assume a blue or violet colour on the addition of iodine and sulphuric acid, or of iodine and chloride of zinc—Schultze's solution, prepared according to Mr. Busk's plan.† On the other hand, however, Messrs. Bristowe and Ord failed to develop this coloured reaction;‡ the waxy matter coloured yellowish-red, melon, or deep reddish-brown by solution of iodine, and was not altered in character, excepting that its tint was after a time rendered lighter, when sulphuric acid, whether applied pure or dilute, and for a shorter or longer period, was added:

"In some cases, a bluish edge-tint was produced when the sulphuric acid was first added. This was evidently due to the precipitation of iodine in a molecular form, and almost always occurs when sulphuric acid comes in contact with iodine solution and animal matter. No tint at all approaching the starch or cellulose tint was obtained during the course of several hundred experiments. With Schultze's solution, the same colour was obtained as with iodine alone."

How are these results to be reconciled with those of every German, and, we may add, so far as our information extends, of every French, and of every other English pathologist, who all agree in stating that a blue colour tints the amyloid matter of waxy degeneration when sul-

* Annalen der Chemie und Pharmacie, Band cx. p. 250. 1859.
† Quarterly Journal of Microscopic Science, vol. ii. p. 100. 1803.
‡ Transactions of Pathological Society, vol. x. p. 301.

phuric acid and iodine are applied to it? These numerous observers cannot have been invariably deceived by the precipitation of molecular iodine around the periphery of amyloid particles; for some of them describe the blue colour as penetrating the interior, and as colouring the mass unequally. Even Friedreich and Kekulé, whose ultimate analysis of waxy spleen obliges them to decide against its amylaceous composition, describe the blue colour as appearing in the morbid matter, not only after the ordinary application of the sulphuric acid and iodine test, but also after the morbid material had been submitted to several chemical processes in the course of proximate analysis.

It might be supposed that some peculiarity in the mode of applying the test led to the different results obtained by the two English observers; but, so far as we can discover, no such peculiarity obtains. They do not, indeed, tell us of what strength their iodine solution was, but the circumstance of this differing from that employed by others surely cannot account for the divergence in the results obtained. Virchow* tells us that to demonstrate the true blue colour with the iodized sulphuric acid requires practice, because the acid soon breaks up the tissue, and then the colour grows indistinct and is at length lost. The same industrious observer also states, and on this point is supported by Paulizky, that the weaker the solution of iodine, the less the quantity of it used, and the more gradual its action, the clearer is the blue colour developed. When the iodine solution is strong, the blue or violet tint rapidly passes away, mixed with reds and browns, until at last a deep brown colour, looking almost black, succeeds.

Here, then, amid the conflicting results arrived at, scope is afforded to the students of amyloid disease to determine on which side truth lies, and to point out the circumstances which have produced the divergence. But although we advocate an extensive re-examination of the question, we are at present unprepared, in the face of so much evidence to the contrary, to range ourselves on the side of Dr. Bristowe and Mr. Ord, in denying the production of a blue colour with the cellulose tests. However, we must do those gentlemen the justice to say that they appear to have carried on their investigation in the most painstaking manner, and to those who would re-examine the question we would recommend the study of their mode of proceeding, as fully detailed in their valuable paper already quoted.

On one point, touching the effect of iodine alone on amyloid tissue, there is unanimity. It is described as peculiar and definite. Virchow has noticed this circumstance, and it is confirmed by Dr. Bristowe and Mr. Ord, as well as by Dr. F. Harris (op. cit., p. 21). The last named author writes:

> "The colour produced by iodine alone on organs which are the seat of this pathological process is perfectly distinctive. When, for example, a solution of iodine is brushed over a liver which has undergone this change, the affected parts in a few minutes assume a deep red-brown colour, very different from the colour produced by iodine on organs in any other condition—once seen, it cannot be mistaken."

* Cellular-Pathologie, p. 336.

Dr. Bristowe and Mr. Ord whilst admitting "the fact of a definite iodine-reaction belonging to the so-called waxy substance," add to it the statement that, "in diphtheritic effusion, and in the villi of a villous cancer, a colour approximating to that of the waxy liver was produced, the resemblance being far closer than that between any specimen of the waxy liver, and the true starch or cellulose colour." This approximation of colour between two sets of morbid products, we are not disposed to regard either as so important, or as in so great a degree indicative of their agreement in kind, as Messrs. Bristowe and Ord intimate. Iodine stains albuminoid matters yellow, and when used in larger quantity, yellowish brown; and the difference in colour by a few shades is surely not of much value as a distinction between such materials in general and those of the like chemical constitution, though of the class of morbid products. The immediate colouring of the amyloid matter with iodine only, is a feature distinguishing it from cellulose and from cholestearine. H. Meckel* committed himself to the statement that waxy degeneration was due to cholestearine; but the error of this hypothesis was at once pointed out by Virchow,† who showed that the reaction in colour of amyloid matter with iodine and sulphuric acid is entirely different from that of cholestearine; and that, on the other hand, tissues rich in the latter—e. g., the nervous tissue—exhibit none of the peculiar reactions of the former. The principal distinctions between the two substances stand thus:—1. Cholestearine is unchanged in colour by iodine alone. 2. The corpora amylacea dissolve in warm or boiling water. Cholestearine is insoluble in water. 3. Cholestearine melts on the application of heat; the amyloid bodies do not melt, but only dry, and still give the same reactions with iodine. 4. Cholestearine dissolves into a brown fluid on the addition of concentrated sulphuric acid; amyloid bodies swell, but do not dissolve, with a change of colour. 5. Cholestearine is soluble in ether, amyloid not. It must be stated, however, that all these differences do not hold good between every one of the varieties of amyloid substance and cholestearine. For instance, the swelling up and final solution in water is true only of the corpuscula amylacea of the brain; even the very similar and equally amylaceous granules from the prostate do not dissolve in water. Nevertheless, after admitting the existence of variations in chemical reaction, and allowing for the different results arrived at by different experimenters, sufficient evidence is furnished by the tests just considered, of the formation and accumulation of one or more peculiar matters in the tissues and organs of the body. On this point there is general concurrence; but of the chemical nature and relations of the peculiar substance opinions are varied and unsettled. Even were the iodine tests more sure and constant in their effects, they would be inadequate as proofs of a genuine amylaceous nature, and need the support of ultimate analysis to prove the non-nitrogenous nature of amyloid matter, and its conversion into sugar. To obtain these proofs is, from the nature of the case, exceed-

* Annalen der Berliner Charité krankenhäuser, and vol. xiv. p. 413 of this Review.

† Archiv, Band vi. p. 419. 1854.

ingly difficult, except with regard to the prostatic corpuscles, and to the rare occurrence of amyloid degeneration in such a high degree that the original histological elements of the tissue affected are almost entirely replaced by the morbid matter.

The corpora amylacea of the brain and spinal cord are too minute and intermingled with the surrounding tissue to be collected for ultimate analysis, or for the attempt to convert them into sugar. Carl Schmidt, indeed, undertook, as already noticed, the analysis of a choroid plexus, reported as rich in amyloid corpuscles; but we view this analysis with great scepticism. We feel very uncertain about these choroid granules having been amyloid corpuscles at all: calcareo-albuminous corpuscles are well-nigh constant in the choroid plexuses, constituting one variety of brain-sand;* but amyloid bodies are strangers in them, though common in the ependyma ventriculorum beneath. The impression, therefore, obtrudes itself on our mind that Schmidt's analysis applies to the calcareous bodies in their early or albuminoid stage, before their impregnation with earthy matter, at least in any quantity; but whatever the corpuscles examined were, the conclusion drawn was that they contained nitrogen, and were of an albuminoid composition, and did not belong to the non-nitrogenous series of hydrated carbons.

The other ultimate analysis performed by Professor Schmidt was of a portion of waxy spleen, and it led to the same result as the foregoing. The details of Schmidt's analysis are very brief, and we cannot determine how far it applied to pure amyloid substance, or to an admixture of this with the usual elements of the spleen. There is, however, a much more satisfactory analysis recorded by Friedreich and Kekulé,† of a portion of spleen so affected by amyloid degeneration that nearly every trace of primitive tissue was obliterated, and the nearest approach possible afforded to a specimen of pure amyloid matter. The altered white waxy material gave a very distinct blue reaction with iodine and sulphuric acid, and under the microscope presented the appearances detailed in our last number. (p. 451.) Having so excellent an opportunity for prosecuting a minute chemical investigation, the able pathologists who record the case proposed for solution the two following questions:—1. Does the amyloid spleen contain an unusual quantity of cholestearine, and if so, is this the cause of the iodized sulphuric-acid reaction? 2. Does the amyloid spleen contain an amylaceous substance chemically related to the starch series, to which such reaction is supposed peculiar? The investigation entered upon gave a negative reply to both these questions. The following were the chemical relations displayed. Cold and boiling water extracted only a trace of albuminous material, the mass acted upon appearing unchanged. Alcohol and ether produced no material alteration, and after their action, sulphuric acid and iodine developed the blue colour even more readily and clearly than before. By boiling fragments for a long time in very weak sulphuric acid, a clear solution was obtained, holding

* Rev. vol. xiv. p. 470.
† Virchow's Archiv, Band xvi. p. 50.

in suspension morsels of the bloodvessels from which the amyloid substance seemed to be extracted. This clear solution of amyloid matter did not reduce an alkaline solution of copper, and therefore contained no sugar; on the contrary, it gave a pale violet hue on the application of Trommer's test, behaving in this respect like an albuminous fluid. In a dilute solution of potash the substance first swelled up, then became transparent, and ultimately by boiling, or merely by long maceration at a lower heat, dissolved, with the exception of the remnants of the bloodvessels, as in the case of the sulphuric-acid solution. The addition of acids to this alkaline liquor let fall a white flocculent precipitate; and in this respect, therefore, it also behaved like a solution of albuminous matter.

Thus far the chemical examination indicated the relation of amyloid to albuminous material; but to make more certain, an ultimate analysis was next undertaken. The purest-looking amyloid was carefully removed and cut up into small pieces; the soluble albumen was extracted, and the residue boiled in dilute and absolute alcohol, and then again in ether. Little was extracted by these means, but this being separated, yielded on analysis a considerable quantity of chloride of sodium, some crystals of leucine, and cholestearine, together with some acicular crystals of fat and oil-drops.

The principal mass left after the removal of the supernatant fluid containing these matters, appeared under the microscope, after the ether had evaporated, in the shape of white granules and flakes, hyaline and formless, mingled with a few relics of bloodvessels. Sulphuric acid and iodine still produced in an equal degree the same blue colour, but it was less permanent; vanishing quickly in the smaller flakes, and in the larger changing to green and then to pale yellow. The remnants of vessels acquired a reddish-yellow hue. Having by mechanical means separated as far as practicable the vascular fragments, the white amyloid matter was next submitted to ultimate analysis, with the following results:—0·1978 parts burnt with chromate of lead, produced 0·3890 of carbonic acid, and 0·1246 of water; 0·2451 parts gave 0·5894 of ammonio-chloride of platinum, indicating 0·0369 of nitrogen. Reducing these results to their equivalents per cent., the composition stands thus: $C = 53·58 : H = 7·00\ N = 15·04$.

Thus a parallel is established with the composition of albumen:—

Albumen.

	Dumas and Cahours.				Lieberkühn.		Rüling.
C =	53·5	53·4	53·5	...	53·5	...	53·8
H =	7·1	7·2	7·3	...	7·0	...	7·1
N =	15·8	15·7	15·7	...	15·6	...	15·5

This analysis certainly shows an almost perfect chemical identity between the substance of waxy spleen and albumen, and the very small difference in the proportion of nitrogen in the two sorts of material is certainly not sufficient to establish a different chemical alliance.

The propositions put forward by Friedreich and Kekulé are therefore thus solved:—1. Waxy spleen contains a considerable quantity of cholestearine, but this is not the cause of the reaction with iodine and sulphuric acid. 2. Waxy spleen contains no matter allied chemically with starch or cellulose.

The pathologists just named conclude their valuable contribution by the following remarks:—That their researches apply actually to only amyloid spleen, and cannot therefore be with absolute certainty regarded as conclusive of the nature of the same matter in other organs similarly degenerated: still they render it highly probable that the condition known as amyloid degeneration, and yielding the distinctive reaction with iodized sulphuric acid, is in all cases only due to a peculiarly modified albuminous material. In farther support of this view, the observations heretofore made on the mode of development of laminated amyloid corpuscles in the pulmonic tissue from coagulated fibrine, may be cited, as well as the detection of amyloid reaction in old fibrinous layers within the sac of an hæmatocele. Although, however (they remark), compelled to refer amyloid matter to the proteine series, yet by reason of its special morphological characters, of its peculiar reaction with iodine, and of its connexion with and indication of a particular constitutional disorder of nutrition, its interest to pathologists will remain unabated, and the term "amyloid degeneration" may with advantage be still retained.

So far as we have proceeded, the weight of evidence is against the hypothesis of a starchy material constituting the basis of the so-called animal amyloid substance the product of disease. And we might on the same side advance it as a farther argument, that though iodine and sulphuric acid may afford a blue colour with cellulose as met with in plant-tissue, it cannot confidently be asserted that, because the same reagents give a similar colour with a particular morbid matter found in animals, such material is necessarily cellulose or a chemical ally of cellulose. Organic chemistry, and particularly animal chemistry, is not sufficiently advanced to enable us to predicate what slight modifications and vital admixtures of organic substances may or may not suffice to vary the effects of tests applied to them, and especially of the fluctuating and less certain colour tests. Thus, in the very instance of the amyloid matter of disease, no *à priori* argument can be raised against the hypothesis that it is an albuminous compound, and, that as such an animal product it can, equally with the cellulose of plants, develop a blue colour with iodized sulphuric acid. But the chemistry of waxy degeneration may receive some elucidation from the chemical history of the corpora amylacea of the nervous system, and of the corpuscles of the prostate gland: for it has been generally taken for granted that it is chemically the same substance in the two. On this supposition the conclusion respecting the chemical nature of the amyloid corpuscles of the brain would involve that of the material of amyloid degeneration, and *vice versâ*. Without at present discussing the truth of the above supposition, the evidence advanced in favour of the amylaceous nature of what we started by calling "corpuscular amyloid" should be here passed under review, but as this has already been done in a preceding paper in the last number of this Journal (vol. xxvi. p. 444 et seq.), very brief notes only are required. Referring to the paper quoted, it is seen that the corpuscula amylacea of the brain, and of most of those of the prostate, afford with iodine alone a blue colour, identical or almost so with that produced in starch globules; that these corpuscles have concentric laminæ and usually a hilum; that they assume a violet hue with iodine and sulphuric acid; that, according to most observers, they resemble starch granules when examined by polarized light; and more than all, that, in the case of the corpuscles of the prostate, which only are available for the experiment, sugar may be chemically produced from them, and demonstrated by Trommer's test and by fermentation.

Presuming on the accuracy of Paulizky's experiments, no doubt can therefore be raised against the conclusion that the prostatic corpuscles —those at least in the early stages of growth, are really referable to the amylaceous series of chemical compounds. There is, moreover, a general concurrence among observers, that though slight shades of difference may be noted between the "corpuscular amyloid" and both starch and cellulose, as well in physical characters as in chemical reactions, they are chemically allied.

Several eminent microscopists have in fact asserted the exact chemical identity of the corpora amylacea with starch, and Mr. Carter has, whilst indeed denying the presence of cellulose, exceeded all in claiming a universal prevalence of starch grains in the body, as well of man as of many of the lower animals. There is, too, an observation recorded by Mr. Stratford, of Toronto, Canada, of the occurrence of starch granules in the blood of an epileptic patient.*

Mr. Carter's researches appear in the 'Edinburgh Medical Journal' for 1855–56 (p. 130), and for 1857–1858 (p. 789), and the conclusions he draws from them are:

"1. That the presence of starch in the animal body is necessary for the well-being, if not for the preservation of the lives of individuals belonging to the principal groups of animals; as shown by its constant occurrence in well-marked members of those groups. 2. That the corpuscles undergo processes of development, growth, and decay, as proved by their variable dimensions, the diverse conditions of their outward wall, and the different appearances of their contents. 3. That its function is not local but general; as indicated by its tolerably equal dissemination throughout nearly the whole of the textures of the body. 4. That some of the starch found within the organism in its healthy state is apparently functionless and excrementitious; as shown by its presence in the urinary excretion and in the mucus of the bronchial tubes, &c."

This wholesale discovery of starch, as such, throughout the animal economy, implies necessarily a remarkable lack of exact observation among all previous and all contemporary microscopists in their having overlooked it; and in partial explanation of what would be so singular an occurrence, Mr. Carter suggests that many of the apparent globules supposed to be of oil, have actually been starch granules. Few observers, however, are, we believe, inclined to accept Mr. Carter's representation either of the abundance of starch corpuscles in the tissues, or of the important part it plays in the functions of life. The impression is, that he has been led into error; and with reference to

* Journal of Microscopical Science. 1855.

the solitary observation of the Canadian practitioner, neither Virchow nor other pathologists deem it of sufficient weight to enter into the discussion of the presence of amylaceous compounds in the body. It is curious to note that, by substituting zoamyline for starch, in the conclusions deduced by Mr. Carter, these might be read as conveying pretty accurately the prevailing conviction regarding the former substance and its purposes in the system. Letting this pass as a casual remark, we may go on to state that the *accidental* presence of starch corpuscles in microscopical examinations is common, and offers the best explanation of Mr. Carter's statements concerning their wide diffusion.

On this matter M. Rouget enters at considerable length in his able paper, "Des substances amyloides; et de leur rôle dans la constitution des tissus des animaux,"* and cites his own experience and that of M. Balbiani. An epitome of the examination of this point is given by Dr. Harris (op. cit. p. 7), who appends his own observations, as follows:

"In examining the epithelial glycogenic papillæ of the amnion of ruminantia, M. Rouget found, when he had crushed the pulpy substance between his fingers, a considerable number of grains of starch; but he found that these grains were always on the surface, never in the substance of the epithelial layers. He suspected from this circumstance their origin; and he found that even after repeated washings, the fingers deposited grains of starch on all surfaces, especially when they were moistened. M. Balbiani found, too, that the most frequent washings could not remove the starch grains from the hands. He therefore washed one of his hands in a solution of potash, and then covered this hand with a glove; at the end of eight hours he could not discover a single grain on the hand which had been thus protected, whilst on his other hand he found the starch grains as numerous as before. M. Rouget found starch grains also in the dust on the outside of windows, on roofs, on stones, in the layer of dust deposited daily in his laboratory, on glass slides exposed to the air; in short, on almost everything to which the ordinary air had access.

"My own more limited experience (subjoins Dr. Harris) tends to confirm that of M. Rouget; for I have certainly found starch granules in the dust of books, on glass slides, and in the scrapings of my hands, but in far less numbers than I was led to suppose from M. Rouget's description. Perhaps the rarity of corn mills in London may account for the difference. In microscopical preparations, too, I have occasionally found a few starch grains, but I never could persuade myself that their presence was other than accidental. On the whole, I think it must be concluded that the presence of starch as starch in the human tissues, is a matter at present 'not proven.'"

This is the conclusion of the majority of pathologists; but it is nevertheless necessary to confirm it by a new series of observations, carried on in an unbiassed manner and with the precaution which the fact above pointed out, of the accidental introduction of the material in debate, indicates to be so necessary. However this question of the distribution of actual starch through the tissues, both in health and disease, be determined, the fact remains, that many prostatic corpuscles are members of the amylaceous series. Moreover, the

* Brown-Séquard's Journal de la Physiologie, tome ii. p. 83.

close analogy between those prostatic corpuscles which offer the amylaceous characters in the highest degree, and the corpora amylacea of the brain, renders it well nigh certain that the latter are of the same chemical nature—a point not capable of determination by the usual modes of chemical examination.

The gist of the inquiry now rests on the admission or the denial of the chemical affinity of the formless amyloid matter of waxy degeneration with that of the foregoing amylaceous corpuscles. This affinity has been universally assumed, chiefly on account of a general agreement between the two varieties of material in their reaction with iodine and sulphuric acid. But the agreement in this respect is by no means perfect, for, as already seen, iodine alone acts quite differently on the two, and when the sulphuric acid is added, there are considerable variations in the colour produced in the granular amyloid not noticed in the corpuscular form. Again: the belief in an affinity between the two is seriously shaken by the results of the proximate and ultimate analyses recorded in preceding pages.

On the other hand, the detection of zoamyline as a normal constituent of animals, renders its production in an abnormal manner a matter of the highest probability; and it may be well to notice here some of the properties of zoamyline, and of other members of the amylaceous group. Likewise, in reviewing the modifications of which starchy matters are capable, it will be instructive to refer to the history of the prostatic corpuscles in their several phases of growth, as put forward by Paulizky.

Glucogene, or zoamyline, when obtained pure, is a whitish, tasteless, inodorous, neutral, non-crystalline powder, insoluble in ether, alcohol, caustic potash, and acetic acid, but soluble in water. In its chemical properties it holds an intermediate place between starch and dextrine; it gives a rosy violet or bluish red, and at times a chesnut-brown colour with iodine; it does not reduce the alkaline copper tests, nor enter into fermentation, but if boiled with dilute mineral acids, or placed in contact with saliva, blood, pancreatic juice, or diastase, it is converted into sugar, which ferments, and reduces Barreswil's solution. When in a state of solution, alcohol and heat coagulate it, or precipitate it in a granular form.

Comparing these chemical relations with those of the amyloid matter obtained from waxy spleen by Friedreich, it will be seen that they are widely different. The latter was dissolved neither in hot nor cold water; when boiled with dilute acid no sugar was generated, nor did it reduce the copper tests; and lastly, it dissolved in liquor potassæ, not only at a boiling, but at a lower temperature. Likewise the amyloid matter obtained by Billroth from diseased lymphatic glands, is stated to have dissolved in caustic potash and in strong sulphuric acid; and, again, neither Carl Schmidt, nor Virchow, has been able to form sugar from the amyloid material of degenerated organs. Lastly, both Friedreich and Schmidt determined the presence of rather more than fifteen per cent. of nitrogen in the amyloid substance they examined.

Now, although the presence of nitrogen and the inconvertibility of amyloid matter into sugar apparently separates it from starch and its immediate allies, still there is an admitted member of the starch series—viz., chitine—which contains nitrogen, and which, moreover—and in this point it agrees with tunicine, another amylaceous compound—may be boiled with dilute acids without the production of sugar; though at the same time it must be stated that by persevering chemical processes, sugar may be formed and fermentation set up in solutions both of chitine and tunicine.

Thus far our review of the chemical relations of the material of amyloid degeneration is unfavourable to the hypothesis of its amylaceous nature. But there are two or three other points connected with the modifying effects of intermixture on the reactions of starchy substances deserving notice. Among the prostatic corpuscles are many which iodine alone does not colour blue, and others again in which this tint does not appear even after sulphuric acid has been superadded.

> "As growth proceeds" (writes Paulizky),* the amyloid in the prostatic corpuscles gradually disappears; and in such of them as have advanced to the dimensions of concretions by the addition of calcareous and pigmentary matter, the starchy reaction is no longer discernible."

So again there are corpuscles the centre of which becomes blue with iodine, but the wall of a violet or reddish hue; and others there are, evidently containing albuminous matter, giving a yellow or brown colour with iodine. Thus it is common to see prostatic corpuscles coloured variously by the admixture of blue and yellow in different proportions, under the action of iodine. Moreover, Paulizky failed in his attempts to obtain the evidence of sugar in most such prostatic granules as gave a yellowish-brown colour with iodine.

If we turn to the chemical history of the hydrated carbons—starch and its allies—we notice how, with an isomeric constitution, widely they differ from each other in their behaviour with the same reagents; for example, starch gets intermingled with cellulose, and this becomes modified in a particular manner and converted into lignine, and then no longer exhibits its characteristic reactions, but acquires new chemical features. M. Rouget has pointed out the varieties of vegetable amyloid, which he reduces to two; 1, amorphous or granular, as found in cell-contents; and, 2, in the condition of cell-wall or of intercellular substance—cellulose. The former he considers to be represented in animals by zoamyline; the latter by chitine and tunicine. But besides these, he has remarked on intermediate forms, just such as occur between starch and cellulose in the vegetable kingdom, as for instance the starch of the seed-coats of chelidonium.

From the above facts it may be urged by the advocates of the amylaceous nature of the deposit in waxy degeneration, that though differing much in many of its reactions from zoamyline and other amylaceous matters, it may yet be merely another variety of them; that many of its peculiarities may be due to an intimate, possibly to a chemical

* Virchow's Archiv, Band xvi. p. 147.

mixture of amyloid with albuminous matter; that if it contain nitrogen, it has an admitted parallel among amylaceous compounds in chitine; and that if investigators have hitherto failed to convert it into sugar, it is nothing more than what happened with chitine and tunicine, until a complex process, not yet tried upon it, was invented by M. Berthelot.

With the present amount of knowledge, it is clear no absolute decision can be arrived at on the question whether the material of so-called amyloid degeneration belongs to the starch series. To recapitulate the argument;—the affirmative rests mainly on the general similarity of reaction with iodine and sulphuric acid, between the substance in dispute and cellulose; but it is strengthened by the discovery of zoamyline as an integral element in the animal constitution. On the contrary, the identity between the corpora amylacea of the nervous tissue and of the prostate, and the amyloid matter in degenerated organs, is not proved: since, therefore, the argument by analogy with proved amylaceous corpuscles in the body will not hold good, its relation to starch compounds must be separately shown. Moreover, the strongest evidence of an amylaceous nature, as afforded by iodized sulphuric acid, is *per se* quite unsatisfactory; it cannot be contended that because a certain colour is educed by the test that the substance yielding it is identical with any one or more substances which have a similar coloured reaction. The fallacy of such reasoning may be illustrated by what happens with the cupro-potassic tests (Barreswil's and Fehling's) for sugar—viz., that besides sugar or glucose, glycerine, cellulose, and choloform, though of unlike chemical composition, produce the same reactions. Since the subject was started by Virchow, the hypothesis of the amylaceous nature of waxy degeneration has been pretty extensively accepted; but it appears to us, from the details entered into, elaborated from a mass of scattered papers and essays, that the balance of evidence is against it; still, the whole subject calls for a much more complete examination than it has yet received, and we should be pleased to see it taken up by some of our fellow-countrymen, for British pathologists and chemists have hitherto, with but one or two exceptions, entirely neglected the whole subject of amyloid degeneration, both in its pathological and chemical aspects.

Review VII.

Mémoires de l'Académie Impériale de Médecine. Tome vingt-troisième, accompagné de quinze planches.—*Paris*, 1859. 4to, pp. clxviii. et 515.

Memoirs of the Imperial Academy of Medicine. Vol. xxiii.

Passing by an official Éloge upon M. Guéneau de Mussy from pen of M. F. Dubois, the secretary, we come to,

I. M. Trousseau's *Report on the Epidemics which prevailed in France during* 1857.—He reiterates the complaint made by all former reporters, that the insufficiency and imperfection of the materials placed in his hands quite preclude the production of other than a meagre and imperfect document, nowise representing the real extent of epidemic visitation during the year in question. The object in appointing medical officers throughout France to report to the Academy of Medicine upon the epidemics which appear in their respective localities, is an admirable one; but unfortunately, like many other institutions, it looks better upon paper than it works in practice, and either owing to insufficiency in the organization, or to the ignorance and incapacity of some of the local reporters, a great number of departments are not reported upon at all, and a yet greater number only very imperfectly. Still, an acute observer like M. Trousseau cannot peruse even such imperfect data as these without extracting something of interest. He remarks that in 1857, following several years of scarcity, the inhabitants of the rural districts were exposed to much privation, articles of food being consumed which at other times would have been rejected, and persons furnishing their contingent to epidemic visitations who, under the influence of a better regimen, are ordinarily preserved from these. The reigning constitution was, so to term it, "*abdominal*," diarrhœa of varying intensity having been observed in nearly every department reported on. It especially prevailed during summer and autumn, while at the commencement of winter an epidemic of *influenza* spread over almost the entire country, without, however, notably augmenting the mortality, being infinitely less fatal than in former visitations. There was this remarkable about it, that it was the only epidemic affection which put on an *ambulatory* character, so that while others of these of a more serious character remained stationary at their point of origin, or only spread with extreme slowness, the influenza successively invaded almost the whole extent of France, nowhere becoming sufficiently intense to complicate or to peculiarize other prevailing affections.

The epidemics which, by reason of their frequency, prevalence, or gravity, characterized 1857, were typhoid fever, dysentery, variola, and croup, or diphtheria. These gave rise to 10,341 deaths from among 57,859 individuals attacked in sixty-eight departments, i. e., 1 death in 6 cases—a proportion which would be truly enormous if the fact were not borne in mind, that many cases which were cured never came under the notice of the reporters, while no instance of death escaped their cognizance.

1. *Typhoid fever* is reported to have occurred in thirty-five departments, furnishing 16,795 cases and 2339 deaths—i. e., 1 death in 7. Many of the reports furnish instances of the importation of the disease by an inhabitant who had contracted it elsewhere. M. Trousseau quotes several of these, and fully adheres to the doctrine. 2. *Dysentery*, occurring in twenty-nine departments, furnished 37,264 cases, with 7119 deaths, or 1 in $5\frac{2}{10}$ths. 3. *Diphtheria*—

"Sound ideas respecting this affection (says M. Trousseau) "are far from being so diffused as might be wished. Many practitioners confound under the same name all the pseudo-membranous productions of the buccal or pharyngeal cavity. Even muguet, which supervenes on typhoid fever or severe dysentery, has been designated, and that by distinguished observers, as diphtherite. As in general, that is only seen which we know how to look for, the descriptions are vague, even the local characteristics of the disease being often ill-indicated. Practitioners little conversant with its mode of evolution and distinctive phenomena, lay weight on mere accessory considerations."

Of the reports sent in from eighteen departments, not one was of a satisfactory character, although the malignity of the disease was such as to arouse solicitude; 736 deaths being reported as occurring in 1322 cases, a proportion never attained by variola in its worst days. 4. *Variola* prevailed epidemically in seventeen departments, furnishing 2398 cases and 177 deaths, or 1 in 14. Doubtless many cases, in consequence of their slightness, have never been reported; but taking the mortality even as here stated, variola descends to the rank of the least destructive of epidemics, although during the last century the deaths amounted to 1 in 3 cases. Almost all the patients who died in the year now reported upon were unvaccinated. There are communes in France in which public vaccinations have not taken place for years.

II. Next comes a Report by M. Laugier on the essays sent in to compete for the *Argenteuil Prize for Improvements in the Treatment of Stricture.*—As none of these (twenty-five in number) were deemed of sufficient value to command the prize of twelve thousand francs placed at the disposal of the committee, the sum was divided amongst the authors of the most notable improvements, in the following proportions:—Four thousand francs each were awarded to MM. Mercier and Gaillard, and one thousand each to MM. Marquez, Désormeaux, Charrière, and Dr. James Arnott. The next award will take place in 1862, and will comprehend the improvements effected since 1856.

III. M. Guerard, reporting on the annual accounts furnished concerning the *Mineral Waters of France*, makes the same complaint as does M. Trousseau, of meagreness and inefficiency, careful statements being quite the exception. This is certainly not for want of encouragement, such as it is, for both the reporters on epidemics and on mineral waters are recipients of an abundant shower of gold and silver medals. Are the French practitioners getting tired of working for these baubles?

IV. On the occasion of the public assembly of the Academy in December, 1858, M. Devergie delivered an interesting address upon *Transitory Homicidal Mania*, intending it to form a kind of *pendant* to Marc's celebrated address on Monomania delivered before the same body in 1833, and indicative of a great progress of opinion with respect to this class of subjects. The acquittal of a young man on the ground of transitory insanity who had killed his stepmother without any obvious motive, and became rational again immediately after the commission of the act, was the immediate cause of the orator's congratulations. After adverting to the labours of Pinel, Esquirol, Ferrus, Falret, Georget, Leuret, and Marc, he exclaims:

"Thus, in the short period of thirty years at most, have we passed from incredulity, or I may say the most profound ignorance respecting the shades of insanity, until so surprising a progress has been attained that the magistrates and juries have now accepted as evidently well-founded, not only the doctrine of partial insanity (monomania), but even that of those temporary aberrations of reason which heretofore, in the eyes of the world, transformed the honest man into a criminal of all the blacker dye that he had carried his perversion of heart so far as to conceal for years together the turpitude of his actions under the mask of the most irreproachable conduct. It is no longer merely advocates who appeal to science in aid of their clients, but magistrates themselves, who, struck by the enormity of the crime in face of the insignificance of the motive which could actuate its author, have turned to men of science and interrogated them concerning the criminality or non-criminality of the deed." (p. 5.)

If, however, monomania—in its several varieties of suicidal, homicidal, imitative, &c.—has been generally accepted by judges and juries as accompanied by a fixed irresistible idea entirely vanquishing all moral liberty, it has been so especially in cases attended with hallucinations, as in the case of the girl who attempted to kill Esquirol, under the delusion that he was the lover who had betrayed her. In cases unaccompanied with such hallucinations, magistrates and men of the world, however great their capacity and information, often entertain great doubts, especially when they rely upon their own judgment alone. Mere intelligence and sagacity do not suffice for the determination whether a brain is sound or diseased. There must also be the continued observation of patients suffering under the various forms of insanity. Alienists, however, admit that, independently of dementia, mania, or monomania, there may exist an instantaneous temporary form of insanity, termed *transitory*, under the influence of which an individual, until then to all appearance of sane mind, may suddenly commit a homicide, and then as suddenly recover his reason. There are no apparent prodromes, and no appreciable cause whether proximate or remote. Still, although this term is most just as a popular one, inasmuch as the insanity is only temporary, although the act accomplished may be of the highest criminality, yet it is scarcely of sufficient exactitude for the physician. Individuals of this description cannot be considered as sound in mind, when the idea of crime has suddenly arisen, and has proved too dominant and irresistible for the will. Various circumstances may be detected by inquiry into the history of the case, and the passage from reason to insanity can never be entirely sudden in the medical view—prodromes always existing, in this as in any other disease. In the absence of all such prodromes, it would be impossible to declare that a reputed criminal action was really an act of insanity. A second important element for coming to a decision in these cases is the utter disproportion which exists between the enormity of the act committed and the motive or interest for its commission. Again, when we examine into the conditions under which the individual has committed the reputed criminal act, these will be found to be characterized by the extremest improvidence, perhaps the most unfavourable moment of all being selected when numerous eligible opportunities presented themselves. In most cases, in other respects, a man of honourable conduct, the lunatic, so far from evading justice, now become aware of the enormity of the crime he has committed, at once delivers himself up. Moral liberty has regained its predominance, and the self-accused criminal ceases to be a madman. For the practitioner, the test which leads to the soundest appreciation and conviction is the examination of the fact under the opposite hypotheses of a criminal and an insane action. In order that one of these should be well-founded, all the facts must be explicable without forcing, while the other presents a tissue of improbabilities. Various insane conditions under which homicidal acts have been perpetrated have been confounded with the one under consideration; but in these the individual continues just as insane subsequent to as prior to the commission of the act.

V. *The Pathological Anatomy of Ovarian Cysts, and its consequences in relation to the Diagnosis and Treatment of this Affection.* By M. BAUCHET.—This prize essay, of about 150 pages in length, contains very little original matter, being in great part indeed a *résumé* of a celebrated discussion upon the subject which took place a few years since at the Academy.* We will merely extract the author's statement of the indications for the employment of iodine injections. Ovariotomy is not an operation recognised by French surgery.

"We have now to consider a very important question—viz., what is the best period for undertaking the radical cure of these cysts by means of iodine injections? I shall endeavour, by the aid of the facts precedingly established, to sum up in the form of propositions the various points which present themselves to the observer. 1. We have no concern here with small tumours, which not infrequently pass unnoticed, those which attain at least the size of the fist or of the fœtal head alone interesting us. 2. The smaller the tumour the greater the chance of success. Usually, however, the practitioner is not consulted until it has attained a considerable size, or the patient refuses to allow of interference until by its size the tumour impedes various functions. On the other hand, the practitioner consulted while the tumour is still small and well limited, hesitates to propose an operation which in some cases has been followed by terrible accidents. Such hesitation is, under most circumstances, mischievous. 3. The age of the patient and the progress of the tumour have to be taken into consideration. 4. From eighteen to thirty-five or forty, an ovarian cyst is certainly mortal, death occurring in a very limited time, from about five to eight years. 5. At the critical age, and especially after the menopausis, the ovarian cysts are more slowly developed, and patients having such tumours may live a long time (from ten to twenty years and more) without suffering much inconvenience from them. 6. In the first case, therefore, if the tumour is found to be increasing in size, it is best to practise the operation at once. 7. In the second case, when the tumour is either stationary or of slow growth, we should wait until by its size or weight it interferes with the functions of neighbouring organs. 8. If the patient is advanced in life (from fifty to seventy, for example), we may content ourselves with a palliative puncture. 9. When, even after the menopausis, the tumour increases rapidly in size, we should prefer the iodine injection, if the age and constitution of the patient do not prohibit it. 10. For large cysts, or for those the contents of which are viscous, unctuous, or hæmatic, successive punctures have been recommended. But it often happens that a patient who has undergone a first puncture refuses

* See Medical Times and Gazette, vol. xxxv. pp. 223 & 244.

to submit to a second operation until the tumour has regained its original size. In these cases, too, we gain nothing by these successive punctures, while we risk the patient's becoming enfeebled and lose precious time. If the patient is willing that a radical cure by means of iodine should be attempted, we should have recourse to it at the second puncture at latest." (p. 171.)

VI. *Cases of Anaplasty of the Urethra.* By M. GAILLARD.—In this memoir the author gives the details of three cases of plastic operations upon the urethra on account of traumatic injury. An abridgment of the details, unaccompanied by the plate which illustrates them, would be scarcely intelligible.

VII. *On Operations for Artificial Anus.* By M. ROCHARD.—We referred to this paper when it was read at the Academy,* and we have now only to state that it was produced in reply to doubts thrown out by Velpeau, that any subjects upon whom an operation for artificial anus had been performed to remedy a congenital deficiency, had reached the adult age. Owing to an instance of success in the hands of Duret, of Brest, in 1793, many cases have subsequently been brought to that town; and at least ten of these, operated upon by Littre's method, have proved permanently successful. Of five of these M. Rochard furnishes the particulars to the Academy, two being still living, and the other three dying at the ages of forty-three, thirty, and fourteen and a half years respectively.

VIII. *Anatomico-Pathological Description of the various Forms of Cataract.* By M. ROBIN.—Since 1852, M. Robin has been much engaged in the examination of the specimens of cataract which have been forwarded to him by the principal Parisian surgeons, or have been met with in the dissecting-rooms. The following is the *résumé* which he gives of the results of his investigations:

"1. *Capsular Cataracts*—(1) *Pseudo-membranous Cataract.*—This variety of cataract may be produced independently of any adhesion of the capsule to the iris. The first appearance of the neo-membranous tissue consists evidently in the production of cells analogous to the elements termed fibro-plastic fusiform bodies. Frequently narrow and much elongated, as in the laminated subcutaneous tissue of the embryo, these are nevertheless generally broader and shorter than in other normal or morbid tissues. These cells may be seen scattered on, and adherent to, the anterior surface of the capsule, even where this remains still transparent, and they are rarely accompanied by free nuclei, which are only found in small numbers when they do exist. Examining the opaque portions more closely, these cells are found to be more and more numerous, superposed on each other, circumscribing the rounded areolar spaces, and taking on the character of fibres of lamellar or cellular tissue. The morbid tissue gets more and more opaque from this superposition, and greyish granules closely adhering to each other are interposed between the fibres. Where the opacity is complete, the morbid production consists in a firm non-vascular tissue, having a striated aspect, and which is torn with some difficulty, the rents being lamellar rather than fibrous. After a certain time this tissue is usually incrusted with a variable quantity of roundish microscopic granules, chiefly composed of carbonate of lime.

* British and Foreign Medico-Chirurgical Review, v. xxiv. p. 546.

"(2) *Cretaceous Capsular Cataract.*—This is characterized by the production of rounded, yellowish microscopic granules, having deep-coloured borders and a shining centre. At first they are isolated, being rarely in juxtaposition, and only in small numbers. They adhere to the iridian surface of the capsule, and are incrusted in this membrane to the depth of about the hundredth of a millimetre; opacity occurs when these granules are sufficiently large and close together to impede the passage of the light, forming masses visible to the naked eye. They sometimes so increase in quantity as to form a fragile layer at the surface of the capsule, half a millimetre or more in thickness. It is termed *ossification of the capsule*, but this really remains intact behind the calcareous deposit, which, however, closely adheres to it. These layers, as well as the isolated granules, are composed chiefly of carbonate of lime, with a small quantity of phosphate. There is also an almost imperceptible trace of transparent organic matter present.

"2. *Lenticular Cataract*—(1) *Soft Cataract.*—This alteration has its seat in the soft superficial or cortical layer of the lens, and gives rise to whitish or greyish opacities, under the form of lines, points, &c., variously disposed, but nearly of an uniform tint. The molecular changes consist especially in the production of a more granular condition; the tubes, losing their nuclei, are flattened into bands. This granular condition is also sometimes found in the denticulated fibres; the cells of the crystalline disappear, from being hyaline and homogeneous becoming granular. Between the flattened tubes are also produced free molecular granulations, together with limpid droplets, and drops of an oily aspect, which have exuded from the elements, or have resulted from their destruction. Moreover, in this superficial layer are formed solid corpuscles, either rounded or of various figures—sometimes homogeneous, sometimes granular, and sometimes set in a substance of a waxy consistence; finally, sometimes carbonate of lime, with traces of phosphate, is deposited here. By reason of these various alterations the soft layer of the crystalline, and sometimes its hard nucleus, are converted into a heterogeneous condition, so that the light in place of traversing these tissues is reflected by the various particles, and assumes a white or greyish tinge, which is always the case when light impinges on any granular or heterogeneous substance.

"(2) *Liquid Cataract.*—This is of a lactescent appearance, and in the cavity of the capsule a whitish fluid is found, in which the hard nucleus of the crystalline is floating. The liquid is composed of a fluid holding fatty drops of solid corpuscles and granules in suspension. It is the passage of the normal elements of the superficial layer of the lens into the liquid state, holding in suspension and emulsifying the corpuscles and droplets, which renders the passage of light so imperfect, reflecting it with a whitish colour, as does any liquid holding solid corpuscles or droplets of a heterogeneous fluid.

"(3) *Hard Cataracts.*—These cataracts have essentially the same anatomo-pathological composition as soft cataracts, no elements differing from the normal being produced, except the solid corpuscles, whether granular or not, and the fatty drops which exude from the altered elements. The lesion consists especially in the solidifying of the individual elements of the crystalline, and in their more intimately adhering to each other than in the normal state. The elements at the same time become more granular, which is one of the essential causes of the opacity, the other causes being the production of solid corpuscles and the exudation of fatty droplets.

"(4) *Lenticular Cretaceous Cataract.*—This is rare, and is due to an incrustation of the anatomical elements of the soft and hard parts of the crystalline, these not being destroyed. In this variety of cataract the lens is of a greyish white or of a chalky white. It is sometimes hard and compact, and friable at its surface, and at others friable throughout its substance. In some cases the surface alone is affected, the nucleus undergoing but little change. The lesion consists essentially in a deposit of carbonate of lime, with a little phosphate, which incrustates the elements of the lens, molecule by molecule, without preventing their recognition when the salts have been dissolved by weak acids. The action of these agents also exhibits spherical corpuscles analogous to those of hard and soft cataracts, but incrustated with the calcareous carbonate." (pp. 264–267.)

IX. *On a Point of Pathological Anatomy relating to Cirrhosis of the Liver.* By M. Sappey.—The author thus states the object of his very interesting memoir:

"The object of this memoir is the determination of the channel through which the blood of the vena portæ is returned to the vena cava inferior when it no longer finds a free passage through the liver. In certain diseases of this organ, and more especially in cirrhosis, the capillaries become partially obliterated, and then only furnish an insufficient passage for the blood of the vena portæ, which has to find a new channel into the torrent of the circulation. But the question, which are the vessels by which the reflux is accomplished, has been, during the last fifteen years, debated by various writers with very unsatisfactory results. In my turn I approach it with what appear to me to be precise and conclusive facts.

"In order to indicate more distinctly my aim, I may at once state that when the blood of the vena portæ meets with an obstacle to its free passage through the liver, it flows backwards from this organ towards the umbilicus, and thence towards the principal venous trunk of the lower extremity; so that not being able to discharge itself into the terminal portion of the vena cava ascendens, it describes a long circuit in order to reach one of its affluents. In its reflux from the liver towards the umbilicus, the blood traverses a vein situated in the suspensory ligament above the cord of the umbilical vein, the course of which it follows, although remaining entirely unconnected with it. The trunk of this vein communicates with the sinus of the vena portæ, and its opposite extremity ramifies in the substance of the umbilical region, anastomosing with the epigastric and internal mammary veins, or with the subcutaneous veins of the abdomen. It is almost always in its usual condition of a very small calibre, and would deserve no mention were it not that in certain diseases of the liver—as cirrhosis, for example—it acquires an importance which until now has escaped attention—showing how details in appearance quite futile, may have their interest when the progress of inquiry, so to say, exhumes them from the bosom of pure science, in order to introduce them into the more brilliant domain of applied science. Sometimes, however, this vein becomes so dilated as to attain a volume scarcely inferior to that of the femoral vein; and when so dilated it resembles the umbilical vein so much in calibre, situation, and direction, that all the examples of its dilatation have been hitherto regarded as so many instances of persistence of this vein." (p. 270.)

The author firmly believes that such persistence has never been observed in the adult; and he submits the few cases of its presumed existence which have been recorded to critical examination. All observers, in fact, on finding a large vein within the suspensory ligament, have at once come to the conclusion that it must be the non-obliterated umbilical, and that the blood must have flowed from below upwards, i.e., from the umbilicus towards the liver. Notwithstanding the unanimity of this opinion, M. Sappey always remained sceptical with respect to its correctness; and two cases which have recently come under his notice, and which are the occasion of this paper, enabled him to demonstrate that his doubts were correct. The body of a man of about forty was brought for dissection, exhibiting cirrhosis of the liver; and a vein of the size of the little finger passed through the thickness of the suspensory ligament, from the left extremity of the sinus of the vena portæ towards the umbilicus, where it anastomosed with the epigastric arteries, which were themselves much dilated. On examining the suspensory ligament, it was found that the cord of the umbilical vein coursed along in the substance of its free border, presenting its ordinary dimension, situation, and direction, and exhibiting the reality of the obliteration in the most peremptory manner. The second case occurred in the practice of M. Trousseau, in the person of a man fifty years of age, suffering from cirrhosis. A continuous murmur was heard through the stethoscope placed on the abdomen; and this was found after death to have arisen from the presence of a voluminous vein extending from the sinus of the vena portæ to the umbilicus, where it ramified and anastomosed with the epigastric veins, which seemed like its continuation. In the substance of the free edge of the suspensory ligament, the obliterated umbilical vein was found, differing in nowise from its normal state. Since the paper was read at the Academy, its author has met with three other cases exhibiting the same facts. Thus the communication between the sinus of the vena portæ and the epigastric veins was established by a vein which accompanies the cord of the umbilical vein having undergone enlargement, and not by means of the persistent umbilical vein. That this disposition has hitherto escaped notice has arisen from the fact that the observers, being so persuaded that they had to do with a persistent umbilical, have never made any search for its obliterated cord. M. Sappey sums up his paper with the following conclusions:

"1. That there is no well authenticated fact of the persistence of the umbilical vein in the adult, and all the cases which have been regarded as exemplifying such persistence should be considered as so many instances of dilatation with hypertrophy of one of the small veins traversing the suspensory ligament of the liver. 2. That this veinule, by becoming thus hypertrophied and dilated, gives rise to the dilatation and hypertrophy of the veins with which it anastomoses, and thus becomes the point of departure of a great derivative channel extending from the sinus of the vena portæ to the principal vein of the lower extremity. 3. That this derivative channel is traversed by the blood in the direction from above downwards, and not from below upwards, as has been generally believed. 4. That it may sometimes implicate the sub-aponeurotic veins, and sometimes the subcutaneous veins of the abdomen. In the first case neither varices nor varicose tumours are developed along its course, while in the second case one or more of these tumours are almost always produced. 5. The venous current directed from the liver towards the femoral vein gives rise to a *frémissement* perceptible to the hand, and to a continuous murmur audible by the ear. 6. That the existence of this current may be considered, in the great majority of cases, as a symptom of cirrhosis; and that this symptom, while it indicates an old and incurable cirrhosis, must yet be regarded as a favourable sign, seeing that it removes the fear of the occurrence of an abdominal dropsy." (pp. 277-8.)

This memoir has been made the subject of an elaborate report from the pen of M. Robin,* who has paid much attention to the subject of

* Bulletin de l'Académie, tome xxiv. pp. 943—994.

the obliteration of the umbilical vein. In this he thoroughly goes into the entire subject, describing the normal condition of the anastomoses of the vena portæ with those veins the dilatation of which leads to a pathological collateral circulation, critically examining the value of the facts which have been supposed to prove the umbilical vein to be the channel establishing his collateral circulation, and describing the changes which take place in the course of the blood through the dilated vessels. He reproaches former observers with the careless inexactitude of their statements, and fully endorses M. Sappey's statements. We regret that our space will not admit of giving an analysis of this report, which is itself really a very valuable memoir.

X. *On Hypertrophic Elongation of the Cervix Uteri in Affections known as Descent or Prolapsus of the Uterus; and on their Treatment by partial or entire Amputation of the Cervix.* By M. Huguier.—This, although the longest memoir in the volume before us, will not demand much space at our hands, as the gist of the paper may be stated in a few words. The author maintains, that in the very great majority of cases of the so-called prolapsus uteri, the pathological condition really is an elongated and hypertrophied state of the cervix uteri. With the usual bias of supposed discoverers, he is not content with the admission that such elongation may sometimes exist, but maintains that it is the general rule, and that the remedy almost always should consist in partial or general amputation. At the discussion which ensued on the reading of the paper at the Academy,* the one-sidedness of the author's views and the dangerous character of his therapeutical procedures were well exposed by M. Dépaul.

Review VIII.

A Book about Doctors. By J. Cordy Jeaffreson, Author of "Novels and Novelists," "Crewe Rise," &c. &c. 2 vols.—*London.* 1860.

Modern Literature seems to be peculiarly fertile in the production of heterogeneous compounds. Such things are undoubtedly one of the requirements of our present social condition; and in the market of *literæ humaniores*, as in that of corn and other necessaries of life, the supply is generally equal to the demand. Mr. Cordy Jeaffreson, in the volumes before us, has achieved that which places him strictly within the limits of *la mode.* With a measure of Dr. Doran's erudition, and a yet larger measure of his flippancy, he has given us a very readable book, replete with anecdotes of "men and manners," doctors, quacks, old women, and—we know not what besides. In nothing, indeed, does he so much resemble the learned author of 'Monarchs Retired from Business,' as in the occasional effort to engraft one story upon another, the connexion between the two being frequently anything but obvious. An unnecessary relation serves but to introduce a more unnecessary one; and "this reminds one of another story," &c., of the author, does

* Bulletin de l'Académie, tome xxiv. p. 672, &c.

not meet and satisfy the "where on earth are we going to now?" of the reader. Many of the anecdotes here collected are well known and authenticated; they have long been a sort of stock-in-trade with the medical annalist. Others are quite new to us, and constitute an agreeable relief to the worn-out records of Abernethyan roughness.

We offer no apology for giving to the profession a hurried glance of the varied field which Mr. Jeaffreson has mapped out for us; for we are certain that an occasional retrospect into the manners and customs, and lives and experiences, of those from whom we have received our Æsculapian mantle, must be as serviceable to us as the most recent discoveries of the chemist or the physiologist.

Who does not know of the now exploded badge of office—the physician's "gold-headed cane?" We are familiarized by Hogarth and others with the later development of the *rabdos*, the fasces of the Roman lictors, the caduceus of Mercury, the wand of Æsculapius, the rods of Moses and the contending sorcerers, "the stick," which, according to the Egyptian proverb, "came down from heaven." And we still see staffs or sticks in the hands of our sheriffs and constables, and gold-headed canes in those of well-fed flunkies in May-fair. But, alas for our professional dignity! with the shoe-buckles, and the breeches, and the periwig, and the cocked hat, is gone that most imposing of official insignia without which no physician would formerly have paid a visit to his patient. We can but invite those who have an objective curiosity to Pall Mall, where, in the Royal College of Physicians, they may feast their eyes upon the magic wand successively borne by Radcliffe, Mead, Askew, Pitcairn, and Baillie.

> "In one respect (says Mr. Jeaffreson) it deviated from the physician's cane proper. It has a cross-bar, almost like a crook, whereas a physician's wand ought to have a knob at the top. This knob in olden time was hollow, and contained a vinaigrette, which the man of science always held to his nose when he approached a sick person, so that its fumes might protect him from the noxious exhalations of his patient." (vol. i. p. 3.)

It was not at that time known, for science had not then revealed, that deodorants and disinfectants are not synonymous, and that a morsel of charcoal would have been more efficient than the elegant bouquet of aromatic vinegar. Perhaps the abolition of the ancient custom of sprinkling herbs before criminals during their trial has but arisen from the more recent discoveries of the disinfecting power of chlorine and charcoal; and to the same cause may be assigned the absence of the imposing nosegays once carried by the sheriffs' chaplains during their official duties. The physician's wand, however, and his vinaigrette have quite disappeared, and the pompousness of the last century has yielded to the active and business-like attitude of the modern practitioner. But the "particoloured pole" of the barber still remains amongst us, and suggests the inquiry of its former connexion with the *ars medica*, or rather with the *ars chirurgica*. In a speech delivered in the House of Lords, in 1797, Lord Thurlow remarks:

> "By a statute, still in force, the barbers and surgeons were each to use a pole. The barbers were to have theirs blue and white, striped with no other

appendage; but the surgeons', which was the same in other respects, was likely to have a gallipot and a red rag, to denote the particular nature of their vocation." "But (says Mr. Jeaffreson) the reason why the surgeon's pole was adorned with both blue and red, seems to have escaped the Chancellor. The fact is, the chirurgical pole, properly tricked, ought to have a line of blue paint, another of red, and a third of white, winding round its length in a regular serpentine progression—the blue representing the venous blood, the more brilliant coloured the arterial, and the white thread being symbolic of the bandage used in tying up the arm after withdrawing the ligature. The stick itself is a sign that the operator possesses a stout staff for his patients to hold, continually tightening and relaxing their grasp during the operation—accelerating the flow of the blood by the muscular action of the arm. The phlebotomist's staff is of great antiquity. It is to be found amongst his properties, in an illuminated missal of the time of Edward I., and in an engraving of the 'Comenii Orbis Pictus.'"

We are happy to think that the operation of phlebotomy, by which thousands have been hurried out of life, is now all but abolished. Many of the idle and would-be wise nobility and clergy are still given to make themselves prominent in the establishment of mesmeric or homœopathic institutions; but they are not so officious as was Lord Radnor, in the middle of the last century, who pulled out a lancet which he habitually carried, and bled Lord Chesterfield before they proceeded together to the House of Peers. The latter was so impressed with the surgical skill of the noble operator, that he was awed into giving him his vote on an important division which took place that night in the Upper Parliament.

"Steele tells of a phlebotomist who advertised, for the good of mankind, to bleed at 'threepence per head.' Trade competition, however, has induced practitioners to perform the operation even without 'the threepence.' In the *Stamford Mercury* for March 28th, 1716, the following announcement was made:—'Whereas the majority of apothecaries in Boston have agreed to pull down the price of bleeding to sixpence, let these certifie that Mr. Clarke, apothecary, will bleed anybody at his shop *gratis.*'"

Possibly, suggests our author, in ancient times the physician's cane and the surgeon's club were used more actively, for bodily ailments, as well as moral failings and social delinquencies.

"This process, Antonius Musa employed to cure Octavius Augustus of sciatica. Thomas Campanella believed that it had the same effect as colocynth administered internally. Galen recommended it as a means of fattening people. Gordonius prescribed it in certain cases of nervous irritability. 'Si sit juvenis, et non vult obedire, flagelletur frequenter et fortiter.'"

And we are ourselves sure that nothing would be so generally effective in the treatment of the hysterics of false introverting pietism and drawing-room indolence.

Next in importance to the physician's cane was his wig. It is not with any malicious intent or desire for libellous application that we remind our readers of a saying that "Wigs were made to protect obstinate old heads from the rays of truth." But surely it is a happy thing for the working practitioner that the present requirements of fashion, imperious and exacting as they are, do not necessitate a return to these odious encumbrances. Official dignity, indeed, still needs their retention in our courts of law; but we have frequently been amused in hot summer days by observing the manner in which barristers are driven to resent the artificial pressure of their bristling skull-caps. Here is a unique picture of the physician of the last century:—

"Each son of Sol, to make him look more big,
Had on a large, grave, decent, three-tailed wig;
His clothes full-trimmed, with button-holes behind,
Stiff were the skirts, with buckram stoutly lined;
The cloth cut-velvet, or more reverend black,
Full-made, and powder'd half-way down his back;
Large decent cuffs, which near the ground did reach,
With half-a-dozen buttons fixed on each.
Grave were their faces—fixed in solemn state,
These men struck awe; their children carried weight.
In rev'rend wigs old heads young shoulders bore,
And twenty-five or thirty seemed threescore."

It appears that the last of physicians who adhered with punctilious strictness to the elaborate costume of the eighteenth century, was Dr. Henry Revell Reynolds, one of the medical attendants of George III. in his long and melancholy affliction. Dr. Reynolds was "the Brummel of the Faculty." His very grave-clothes were fashionably made, and his epitaph records:

"Here well-dressed Reynolds lies,
As great a beau as ever;
We may perhaps see one as wise,
But sure a smarter never."

We are now introduced to some of the early English physicians, and first to Dr. John Phreas and Dr. Thomas Linacre. The former received from Pope Paul II. "the fatal gift" of an English bishopric, for his translation of 'Diodorus Siculus.' "A disappointed candidate for the same preferment is said to have poisoned him before the day appointed for his consecration." The latter, in conjunction with others, obtained from Henry VIII. the grant of letters patent establishing the College of Physicians. He also had an ecclesiastical tendency, and assumed the sacerdotal garb five years before his death; but he appears to have been marvellously impressed with the contrast between Christianity as propounded in the New Testament and practised in the world. In the generations following, we have successively, Dr. John Kaye (or Key, or Caius), Sir John Mayerne, Harvey, and Bulleyn. The brief inscription "Fui Caius" is upon the tomb of the former in the chapel of the college bearing that name at Cambridge. Mayerne was a terrible administrator of mercury, and gave calomel in scruple doses. He had a famous "gout-powder," whose chief ingredient was "raspings of a human skull unburied." He had an efficacious "balsam of bats," with which he anointed hypochondriacs; and he was a believer in "the efficacy of amulets and charms." The great discoverer of the circulation of that blood which phlebotomists have so lavishly shed, is described by Aubrey as wearing a dagger, and being "very cholerique." "He rode on horseback, with a foot-cloath to visit his patients, his man following on foot, as the fashion then was, was very decent, now quite discontinued." Of the physicians of the Elizabethan era we are most largely familiarized with Dr. Wm. Bulleyn, an "interesting and sagacious" practitioner. Like the famous Dr. Butts (mentioned by Shakspeare in *Henry VIII.*), Caius, and Wm. Butler, he was from the eastern counties. Bulleyn was proud of that littoral which is washed by the German Sea, and of the efficacy of Suffolk and Cambridgeshire herbs he had the highest estimation. Such hops and such peas as there vegetated were not to be found, he said, in any other locality. The parsnips, carrots, and radishes of the London market, as compared with those of Suffolk, were "more plentiful than profytable." This was the age of unlimited faith in herbs, and fruits, and plants. "Figges (says the doctor) be good against melancholy and the falling evil, to be eaten. Figges, nuts, and herb-grace do make a sufficient medicine against poison or the pestilence. Figges make a good gargarism to cleanse the throates." Equally comprehensive virtues were attributed to spinach by Lemery, physician to Louis XIV. It "stops coughing, allays the sharp humours of the breast, and keeps the body open." Bulleyn, like most of the doctors of that day, grew his own herbs in his own garden. "Hence (says Mr. Jeaffreson) we may here see the origin of the old nursery tradition of little babies being brought by the doctor from the parsley-bed." Nor the skill nor the herbs of Dr. Bulleyn protected him from the sting of man's ingratitude.

> "With daisy-tea—or *bellis-tea*—(he records) I, Bulleyn, did recover one Belliser, not onely from a spice of the palsie, but also from the quartan. And afterwards the same Belliser, more unnatural than a viper, sought divers ways to have murthered me, taking part against me with my mortal enemies, accompanied with bloudy ruffins, for that bloudy purpose."

Bulleyn was what may be termed an heroic practitioner, and he inculcated boldness with the sister art of surgery. "Soft chirurgians (he writes) make foul sores." In dressing wounds he advises, on the part of the dresser, "a gladsome countenance," because "the paciente should not be greatly troubled." Some of Dr. Bulleyn's recipes are very mirth-stirring. Here is a marvellous embrocation :

> "*An Embrocation.*—An embrocation is made after this manner: ℞ Of a decoction of mallowes, vyolets, barly, quince seed, lettice leaves, one pint; of barley-meale, two ounces; of oyle of vyolets and roses, of each an ounce and half; of butter, one ounce; and then seeth them all together till they be like a broathe, putting thereto, at the ende, foure yolkes of egges; and the maner of applying them is with pyeces of cloth dipped in the aforesaid decoction, being actually hoate."

We shall not trouble our readers with "A Good Emplaster," nor with an "Electuarium de Gemmis," which "kings and noblemen have used for their comfort. It causeth them to be bold-spirited, the body to smell well, and ingendereth to the face good coloure." Amusing as are these prescriptions, we think they hardly equal a famous recipe of an earlier date (Henry VIII.) which we remember to have somewhere met with :

"A goode medicine for weaknesse or consumption: Take a pig of nine days olde, and slaye him, and quarter him, and put him in a skillat, with a handfull of spearmint and a handfull of red fennell, &c., and nine dates cleaned, and a hanfull of great raisins, and picke out the stones, and two stickes of good cinnamone, bruised in a mortar, and distill it with a softe fire, and put it in a glass, and set it in the sun nine days, and drink nine spoonfulls of it when you list."

Observe the faithful adherence to the number of the Muses. But the best of Bulleyn's receipts is one in which he prescribed "a smal yong mouse, rosted." "Snayles (he affirms also) broken from the shells and sodden in whyte wyne, with oyle and suger, are very holsome." Mr. Jeaffreson adds, that only very recently has a belief in snail-virtue been banished from the Suffolk mind. It is easy to understand how these herbalist physicians would resent the treatment of modern times, and pronounce as heretical those who inculcated such a doctrine as that of Dumoulin, who said, "I leave behind me two great physicians, Regimen and River Water." It was in much the same spirit that Sir Philip Sidney defined health to be "great temperance, open air, easy labour, little care;" and that Lady Wortley Montague (who slapped Pope in the face), has recorded, that "air, exercise, and company are the best medicine; and physic and retirement good for nothing but to break hearts and destroy constitutions." "William Bulleyn died in London, on the 7th of January, 1576. He was buried in the church of St. Giles's, Cripplegate, in the same tomb wherein his brother Richard had been laid thirteen years before, and wherein John Fox, the martyrologist, was interred eleven years later."* (vol i. p. 42.)

It is but little space that Mr. Jeaffreson devotes to that most accomplished physician and writer, the author of the "Religio Medici,' whom Coleridge speaks of as "a fine mixture of humorist, genius, and pedant." We confess to a weakness for Sir Thomas Browne, and his unorthodox (as some would have it) erudition; and we never think of him without calling to mind that beautiful saying of his: "Sleep is Death's younger brother, and so like him that I never dare trust him without my prayers." He, too, settled in the eastern counties, at

* We must give Bulleyn's unique and excellent rules for an apothecary's life and conduct: "THE APOTICARYE. 1.—Must fyrst serve God, foresee the end, be clenly, pity the poore. 2. Must not be suborned for money to hurt mankynde. 3. His place of dwelling and shop to be clenly to please the sences withal. 4. His garden must be at hand, with plenty of herbes, seedes, and rootes. 5. To sow, set, plant, gather, preserve, and kepe them in due tyme. 6. To read Dioscorides, to know the natures of plants and herbes. 7. To invent medicines to choose by colloure, tast, odour, figure, &c. 8. To have his morters, stilles, pottes, filters, glasses, boxes, cleane and sweete. 9. To have charcoles at hand, to make decoctions, syrups, &c. 10. To kepe his cleane ware closse, and cast away the baggage. 11. To have two places in his shop—one most cleane for the phisik, and a baser place for the chirurgie stuff. 12. That he neither increase nor diminish the physician's bill (*i. e.*, prescription), and kepe it for his own discharge. 13. That he neither buy nor sel rotten drugges. 14. That he peruse often his wares, that they corrupt not. 15. That he put not in *quid pro quo* (*i. e., use one ingredient in the place of another when dispensing a physician's prescription*) without advysement. 16. That he may open wel a vein for to helpe pleuresy. 17. That he meddle only in his vocation. 18. That he delyte to reede Nicolaus Myrepus, Valerius Cordus, Johannes Plucaton, the Lubik, &c. 19. That he do remember his office is only to be ye phisician's cooke. 20. That he use true measure and waight. 21. To remember his end, and the judgment of God: and thus I do commend him to God, if he be not covetous or crafty, seeking his own lucre before other men's help, succour, and comforte.

Norwich, marrying a rich and beautiful lady of that county. Dr. Browne was educated at Oxford, and was knighted by Charles II. when he visited Norwich, in 1671. We beg those who call in question Sir Thomas Browne's orthodoxy, to remember that he was an accomplished physician, an elegant scholar, a Christian gentleman, in the truest sense of the term, and that "his suave and unobtrusive manner secured him many friends, and his philosophic moderation of temper saved him from ever making an enemy."

"Had it been my province" (says a Norfolk rector) "to preach his funeral sermon, I would have taken my text from an uncannonical book, I mean that of Syracides, or Jesus, the son of Syrach, commonly called Ecclesiasticus, which, in the 38th ch. and 1st verse, hath these words: 'Honour a physician with the honour due unto him, for the uses which you may have of him, for the Lord hath created him; for of the Most High cometh healing, and he shall receive honour of the king' (as ours did that of knighthood from the present king when he was in this city). 'The skill of the physician shall lift up his head, and in the sight of great men shall he be in admiration;' so was this worthy person by the greatest man of this nation that ever came into this country, by whom also he was frequently and personally visited."*

The "Religio Medici" met with severe but feeble criticism from a cotemporary of Sir Thomas Browne—the pedantic Kenelm Digby—who, not confining himself to the profession of medicine, was before the world as "courtier, cook, lover, warrior, alchemist, political intriguer, and man of letters." Some of his culinary recipes are worthy of Soyer, and his eulogistic epitaph such as Sir Kenelm would have liked to have seen inscribed upon his own tomb:

"Under this tomb the matchless Digby lies—
Digby the great, the valiant, and the wise;
This age's wonder for his noble parts,
Skilled in six tongues, and learned in all the arts.
Born on the day he died—the Eleventh of June—
And that day bravely fought at Scanderoon.
It's rare that one and the same day should be
His day of birth, and death, and victory."

"The lives of three physicians (says our author)—Sydenham, Sir Hans Sloane, and Heberden—completely bridge over the uncertain period between old empiricism and modern science. The son of a wealthy Dorsetshire squire, Sydenham was born in 1624, and received the most important part of his education in the University of Oxford."

He had previously graduated in medicine at Cambridge, and studied at Montpellier. It does not appear that this illustrious individual had unusual confidence in the curative powers of his profession; indeed it may be stated that "he was a man of many doubts," harassed by perplexity and undue deliberation. "It was he who replied to Sir Richard Blakemore's inquiry after the best course of study for a medical student to pursue—'Read 'Don Quixote:' it is a very good book—I read it still.'" Sydenham, who was a martyr to the gout, died at his house in Pall Mall, on the 29th of December, 1689. Dr.

* Minutes for the Life of Sir Thomas Browne, by the Rev. John Whitefoot, M.A., Rector of Heigham.

Forbes Winslow, in a book published by him some twenty years since, and entitled 'Physic and Physicians' (and upon which Mr. Jeaffreson has drawn somewhat largely), has given an interesting picture of this physician's sufferings.

William Heberden was born in 1710, receiving his education and graduating at Cambridge. After practising for thirty years in London with great success, he retired to Windsor, where he died in 1801. It was at his instigation that the 'Transactions of the College of Physicians' was commenced, to the first volume of which he contributed many valuable papers. In 1782 he wrote his 'Commentaries,' which were published after his death. Their elegant Latinity attests the scholarship of this accomplished man.*

Sir Hans Sloane now comes upon the scene. London, Paris, and Montpellier alike contributed their quota to his medical education. He was an intimate friend of Sydenham. In September, 1687, he sailed for the West Indies as physician to the Duke and Duchess of Albemarle. The duke died in two years, and Heberden returned with the widowed duchess to England, settling down to London practice. In the person of Sir Hans Sloane was initiated the honour of a baronetcy in the medical profession. "Upon his death, on the 11th of January, 1753, his museum and library passed into the hands of the nation for a comparatively small sum of money, and became the nucleus of our British Museum."

We next allude to Garth, not because he had anything to recommend him as a physician, but because he was a rollicking, witty bon-vivant, the friend of Pope and Swift, and an unmistakeable politician of the Whig school. To him Dryden owed an honourable interment; for "when the great poet died, Garth caused his body to be conveyed to the College of Physicians (then situate in Warwick-lane), and started a public subscription to defray the expenses of the funeral." "Even in our ashes live our wonted fires," says the poet; a truth verified in the death of Sir Samuel Garth, for he was unable to restrain his characteristic levity in the most solemn season allotted to mortality. "Gentlemen," said he to the crowd of weeping friends who stood around his dying bed, "I wish the ceremony of death was over." Yet Pope wrote of him afterwards: "If ever there was a good Christian, without knowing himself to be so, it was Dr. Garth."

Johan Radcliffe, M.D., hujus Collegii quondam socius (as it is recorded on the stained glass window which he presented to University College), is intimately connected in our minds with the City of Oxford. He is represented by his biographers as "having recommended himself more by ready wit and vivacity than by any extraordinary acquisitions in learning." In 1684 he took a house in Bow-street, Covent-garden, and within twelve months acquired a large and lucrative practice. The *suaviter in modo* does not appear to have been one of the ingredients of his social character; and though attractive to those whom he liked, and who paid court to him, he made many and great enemies by an injudicious and uncalled-for severity to his professional equals

* See Memoir prefixed to Commentaries.

and inferiors.* It is affirmed by Dr. Mead and others that Radcliffe was unusually gifted in the prognosis of disease. To this may be partly attributed his great success; for, like Sir Thomas Browne, he appears to have been somewhat sceptical as to the remedial powers of medicine.

> "When a young practitioner, he is reported to have said (in language which was almost endorsed by a celebrated physician and physiologist who has so recently been removed from us), 'I possessed twenty remedies for every disease; and at the close of my career I find twenty diseases for which I have not one remedy.'"

That virtue, or rather that habit, which is declared to yield precedence but to godliness, together with pure air, and a wholesome diet, were the resources upon which he relied most largely. The very powerlessness of the lower orders to obtain these things in over-populated cities, and under the high-pressure exactions of modern civilization, peoples our hospitals with diseases, and crowds our lazar-houses with death. Radcliffe had a contempt which he could not conceal for every ailment which savoured of the fanciful, the hysterical, or the hypochondriacal. He could not have lived in our Belgravia, or driven his chariot within the precincts of St. James's. The Princess Anne of Denmark, to whom he had been medical adviser for some years, was at last necessitated to supplant him by the introduction of Dr. Gibbons. The royal patient had summoned the disloyal physician. Bacchus suggested delay, poured out a fresh beaker of claret, and bade his now vivacious disciple leave the princess and her "blue devils" till the morning. "Tell her Royal Highness," exclaimed the doctor to the messenger, "that her distemper is nothing but the vapours. She's in as good a state of health as any woman breathing—only she can't make up her mind to believe it." The royal lady did not forgive this imputation of the "vapours;" nor was the physician (who was turned away from the palace on presenting himself the next morning) more charitable towards his successful rival, when he indulged in the most unwarrantable sarcasm, by saying: "Nurse Gibbons has gotten a new nursery—Nurse Gibbons was fit only to look after a woman who merely fancied herself ill." Yet Radcliffe's great success was not in the least diminished by his indulgence in these indecent outbreaks from the conventional restraints of temper and propriety. His fees were very large—large enough to justify his accepting (had he so chosen) the offer of a baronetcy by the king. It can hardly be said such an

* A rather unscrupulous cotemporary of Radcliffe—Dr. Hannes, who at one time had quite the run of the town, and excited the former's jealousy—was incessantly at feud with the Oxford celebrity. "The virulence of their mutual animosity (says Mr. Jeaffreson) may be estimated by the following story. When the poor little Duke of Gloucester was taken ill, Sir Edward Hannes and Blackmore (famous as Sir Richard Blackmore, the poet) were called in to attend him. On the case taking a fatal turn, Radcliffe was sent for; and after roundly charging the two doctors with the grossest mismanagement of a simple attack of *rash*, went on, 'It would have been happy for this nation had you, sir (to Hannes), been bred up a basket-maker (the paternal trade), and you, sir (to Blackmore), had remained a country schoolmaster, rather than have ventured out of your reach, in the practice of an art which you are an utter stranger to, and for your blunders, in which you ought to be whipped with one of your own rods.'" (vol. i. p. 139.)

act of royal condescension was merited, for his Majesty could not but be displeased at the coarseness which led Radcliffe to say, after examining the royal and dropsical ankles, "I would not have your Majesty's legs for your three kingdoms." Radcliffe never married. He was too fond of satirizing the sex. He told ladies in general that there should be an act of parliament to enable nurses to have the sole medical charge of women; and he told one in particular, who had complained to him of a nervous singing in the head, that she had better "curl her hair with a ballad." But anxious to give additional force and illustration to the axiom, "there's no fool so great as an old fool," the doctor, at the age of sixty, essayed in vain, by a new equipage, and the most recent fopperies, to captivate a lady gifted with youth, wealth, and beauty.* The stories related of Radcliffe's rudeness are scarcely less numerous than those of his pecuniary foibles. Like many other wealthy bachelors and married millionaires, he was a strange compound of meanness and liberality. The suffering poor of his immediate day could have told what he might have done and did not do for them; they have told, however, at the same time, of splendid munificence and diviner charity. And the noblest seat of England's learning, and the Society for the Propagation of the Gospel in Foreign Parts, have a perpetual and living record of this physician's goodness. "The evil which men do lives after them; the good is oft interred with their bones." It is not always so. To wit the following:

"You will find (says Radcliffe, in a letter to his married sister, Millicent, found after his decease) by my will that I have taken better care of you than perhaps you might expect from my former treatment of you; for which, with my dying breath, I most heartily ask pardon. I had indeed acted the brother's part much better in making a handsome settlement on you while living, than after my decease; and can plead nothing in excuse, but that the love of money, which I have emphatically known to be the root of all evil, was too predominant over me; though I hope I have made some amends for that odious sin of covetousness in my last dispositions of those worldly goods which it pleased the great Dispenser of Providence to bless me with."

In the same year that Radcliffe settled 50*l.* a year for ever upon the Society above alluded to, he presented, under an assumed name, the sum of 520*l.* to the Bishop of Norwich, for distribution among the poor nonjuring clergy. It is a real pleasure to put such deeds as these before our professional brethren. They soften the heart; they make us feel the link of that brotherhood which should bind us all together, and make the less fortunate one with the more prosperous and well-to-do. Dr. Radcliffe died in 1714 (having been elected M.P. for Buckingham the year previous), at the age of sixty-four. His body lies in the beautiful church of St. Mary, in that city and university, his posthumous benefactions to which are so universally known. It is curious that this physician, though so intimately associated with his Alma Mater, should have been anything but a good scholar. It is true he was ignorant to an extreme of professional literature; but he may have been ignorant of it because he despised it, and not because

* See Steele's charming satire in No. 44 of the "Tatler."

he had no capacity for its study. Yet his cotemporaries derided him, and Dr. Garth remarked with great felicity, that for Radcliffe to leave a library was as if a eunuch should found a seraglio. There is no doubt, indeed, that he who satirized the sex so severely, and dubbed the princess as "vapoury," was extremely deficient in that classical learning for which Oxford physicians ought to be and are almost invariably remarkable. This very ignorance, however, secured him the following exquisite compliment from his immediate successor:

"'Do you read Hippocrates in Greek?' said Radcliffe to Dr. Mead, whom he found reading that writer. 'Yes,' replied Mead, timidly, fearing his scholarship would offend the great man. 'I never read him in my life,' responded Radcliffe, sullenly. 'You, sir,' was the rejoinder, 'have no occasion—you are Hippocrates himself.'"

Mead was on other occasions equally happy in his elegant flatteries, of which he reaped the fruits, for Radcliffe exclaimed in a frenzy of delight, "By Heaven! I'll recommend you to my patients."

Mr. Jeaffreson now indulges us with a few remarks upon the ponderous obesity of certain members of the medical profession. We cannot perceive that our general life and habits are peculiarly favourable to corpulency, and must regard our author's illustrations as quite exceptional. Indeed, we are not at all sure that if we could be individually and collectively guaged by the corporeal standard of some other profession, we should be in that scale of the balance which would "kick the beam." Of course we have our Daniel Lamberts.

"The obesity of Dr. Stafford was celebrated in an epitaph:

'Take heed, O good traveller, and do not tread hard,
For here lies Dr. Stafford *in all this churchyard*.'

Dr. Beddoes was so stout that the Clifton ladies used to call him their 'walking featherbed.' Dr. Flemyng weighed twenty stone and eleven pounds, till he reduced his weight by abstinence from the delicacies of the table, and by taking a quarter of an ounce of common Castile soap every night. Dr. Cheyne's weight was thirty-two stone, till he cured himself by persevering in a temperate diet. Laughing at two unwieldly noblemen whose corpulence was the favourite jest of all the court, Louis XIV. said to one of them, 'I suppose you take little or no exercise.' 'Your Majesty will pardon me,' replied the bulky duke, 'for I generally walk two or three times round my cousin every morning.'"

Doubtless, at one time or other, one or all of these corporeal magnates had practically endorsed the opinion of Dr. Tobias Whitaker as to wine being "the physick that doth not dull," and whose "operation leaveth no venomous contact."

"As for my own experience (he adds), though I have not lived yet so long as to love excesse, yet have I seene such powerful effects, both on myselfe and others, as if I could render no other reason, they were enough to persuade me of its excellencie, seeing extenuate withered bodies by it cause to be faire, fresh, plumpe, and fat, old and infirme, to be young and sound, whereas water or small-beer drinkers looke like apes rather than men."

Do not let us pass over the memorable fact which has an intimate connexion with *bon-vivants*, that a physician, "Nurse Gibbons" (to repeat the sobriquet of Dr. Radcliffe), first introduced mahogany into domestic use, his brother, a West Indian captain, having brought over some as ballast to this country. The Duchess of Buckingham gave it her sanction, and since that time this famous wood has been associated with hospitality and abundance.

We are apt to believe—and not without reason—that the members of no profession are more distinguished for their general courtesy and urbanity than those of the medical. If the anecdotes related of Abernethy and Radcliffe, Sir Richard Jebb, and others, are authentic, it cannot be said that of necessity the *ars medica* "emollit mores, nec sinit esse feros." But the antecedents of the Faculty may have shamed us into propriety, this circumstance being coeval and co-extensive with the general improvement of men and manners. Be that as it may, we are certain that anything approaching *brusquerie* and rudeness is not now tolerated, where those exposed to such visitations are in a position to resent them. And more: we are certain that the general treatment of the poor by the medical officers of the unions and others is highly creditable to the humanity of men tried in no ordinary measure by ingratitude, and most inadequate pecuniary remuneration. It is affirmed by those who knew Abernethy well, that the many relations of his unfeeling conduct have been grossly exaggerated, and that he had really a kind heart under a rough manner, and ever promptly responded to the appeals upon his generosity. It is probable that he has brought tears to many a hysteric eye (what medical man possessed of *firmness* has not done so?), and equally probable that he has comforted many a sorrowing heart. But we never heard of an apologist for the coarseness of Sir Richard Jebb, which formed but too probably a pompous covering for professional ignorance. Sometimes the knight was paid more that he expected in return, and received his *congé* instead of his guinea. "That's my way," said he once to a noble invalid, who was astounded at the physician's rudeness. "Then," replied the sick man, pointing to the door, "I beg you'll make *that* your way."

"To all questions about diet, Jebb would respond tetchily or carelessly. 'Pray, Sir Richard, may I eat a muffin?' asked a lady. 'Yes, madam, 'tis the *best* thing you can take.' 'Oh dear! Sir Richard, I am glad of that. The other day you said it was the worst thing in the world for me.' 'Good, madam, I said so last Tuesday. This isn't a Tuesday, is it?' Sir Richard's best set of dietetic directions consisted of the following negative advice, given to an old gentleman who put the everlasting question, 'What may I eat?' 'My directions, sir, are simple. You must not eat the poker, shovel, or tongs, for they are hard of digestion; nor the bellows (for they will create wind, he might have added); but anything else you please.'" (vol. i. p. 194.)

These injunctions would hardly satisfy the comprehensive gastric physiology of Dr. Prout, and would necessitate, if fully carried out, another edition of 'The Stomach and its Difficulties.'

Let us now endeavour to trace the connexion between medical labours and medical remuneration.

"From the earliest times (says our author) the leech or healer has found in the exercise of his art, not only a pleasant sense of being a public benefactor, but also the means of private advancement. The use churchmen made of their medical position throughout Christendom (both before and after that decree of the Council of Tours, A.D. 1163, which forbade priests and deacons to perform surgical operations in which cauteries and incisions were employed), is attested by the broad acres they acquired for their religious corporations, as much from the gratitude as from the superstition of their patients. And since the Reformation, from which period the vocations of the spiritual and the bodily physician have been almost entirely kept apart, the practitioners of medicine have had cause to bless the powers of sickness."

There is, however, in the present day, an unquestionable tendency to revert (according to Mr. Darwin's theory) to ancestral characteristics. Few so much love to dabble in "chirurgic stuff" and therapeutic agents, not always of the milder sort, as parochial clergymen and their wives. And occasionally some unusually learned curate startles the readers of the *Times* by the discovery of "a new remedy for cancer," which, though his mission is "to do good and to distribute," he does not think it a part of his vocation to make known to the world at large. Dr. William Bulleyn makes mention of many fashionable empirics during the Elizabethan and the three preceding reigns. Amongst others he signalizes Sir Thomas Elliot, Sir Philip Parris, Sir William Gascoyne, Ladies Taylor and Darrel, as also that "goodly, hurtlesse gentleman, Sir Andrew Haveningham, who learned water to kill a canker of his own mother." One of Lord Derby's ancestors was skilled in "chirurgerie and bone-setting;" and mention has been made of the surgical capacity of James IV. of Scotland—"quod vulnera scientissime tractaret." Lindsay says of him, that he was "such a cunning chirurgeon that none of his realm who used that craft but would take his counsel in all their proceedings." There was no occasion to fee such lofty and aristocratic practitioners as these. "Chirurgie" was their amusement; medicine was their pastime. But the educated practitioner who has expended so much upon the acquisition of knowledge must be adequately compensated, or he will have to do as Dr. Arbuthnot did—leave his settlement, and seek a more remunerative locality. It is true that it was not in this particular case the ingratitude or closeness of his patients which necessitated this step on the part of this accomplished physician, the protégé of Queen Anne, and the friend of Pope and Swift. There were no patients at all. Dorchester was too healthy for the growth of medical science; she limited the aspirations of professional philanthropy. The great-grandfather of Mr. Thwaite had been there, and the shadowy predecessors of sanitary commissioners. "Where are you off to?" cried a friend, who met Dr. Arbuthnot riding post towards London. "To leave your confounded place," was the answer, "for a man can neither live nor die there." What, then, is the general nature of pecuniary settlement?—what the experience of the profession as to the material gratitude of its patients? The epitaph of Sir John Ayliffe, sheriff of London, and merchant of Blackwell-hall, records:

"King Edward for his service sake,
Bade him rise up a knight,
A name of praise; and ever since
He Sir John Aylife hight."

There is but little of the substantial about such a reward as this: but as it was customary to leave the remuneration to the individual generosity of the party who had received the advice, a physician could hardly complain publicly at what royalty condescended to mete out to him.* Mr. Jeaffreson seems to imply that the gratitude of patients is in the ratio of their suffering, and that the *cacoethes donandi* is most fully developed at the height of the sufferer's malady. To say that this is a truism is only another way of acknowledging the frailty of human nature. There is no manner of doubt that the donative performances of the active and the living are not commensurate with the grateful promises of those who think they are in danger.

"On entering one morning the chamber of a French marquis whom he had attended through a very dangerous illness, Bonvart was accosted by his noble patient in the following terms:—'Good-day to you, Mr. Bonvart; I feel quite in spirits, and think my fever has left me.' 'I am sure it has,' replied Bonvart, drily. The very first expression you used convinces me of it.' 'Pray explain yourself.' 'Nothing is easier. In the first days of your illness, when your life was in danger, I was your *dearest friend;* as you began to get better, I was your *good Bonvart;* and now I am *Mr. Bonvart.* Depend upon it you are quite recovered.'"

We are afraid there is a well-merited satire conveyed in this anecdote, which renders it desirable that the physician should follow Mr. Jeaffreson's advice, and let his motto be, *accipe dum dolet.* A medical friend of our own is so strongly impressed with the general ingratitude of patients towards their medical attendants, that he declares they are always last paid and worst paid of any creditors. Even the undertakers take precedence of them in enjoying the liquidation of their pecuniary claims. On the other hand, there is much to be said of a generosity and an attachment on the part of patients towards their physician, which has nothing at all analogous in any other profession. These extraordinary anecdotes of the reception of large fees (which almost every physician or surgeon of note can parallel in his own experience), such as the famous nightcap-fee of Sir Astley Cooper, attest rather the foibles of eccentricity than the noble consistency of unfailing gratitude. There is something better than this in the attachment formed by a conscientious gentleman for a conscientious physician: an attachment which shows itself in the daily effort to promote his interests, and uphold the dignity of his character. These stories, therefore, of large fees, though individually acceptable to the lucky practitioner, bear no comparison in our estimation to things which assimilate more closely to the widow's mite.† It is not for us

* Dr. Doran says: "Now there is a religious reason why fees are supposed not to be taken by physicians. Amongst the Christian martyrs are reckoned two Eastern brothers, Damian and Cosmas. They practised as physicians in Cilicia, and they were the first mortal practitioners who refused to take recompence for their work. Hence they were called Anargyri, or "without money." All physicians are pleasantly supposed to follow this example. They never take fees, like Damian and Cosmas; but they meekly receive what they know will be given, out of Christian humility, and with a certain or uncertain reluctance, which is the nearest approach that can he made in these times to the two brothers who were in partnership at Egea in Cilicia."

† "When Dr. Dimsdale, for many years a Hertford physician, and subsequently the parliamentary representative of that borough, went over to Russia and inoculated the Empress and her son, in the year 1768, he was rewarded with a fee of 12,000*l*, a pension

to deny that there are wolves in every sheepfold, and that there is abundant evidence in the daily walk of life of a greediness of gain on the part of physicians, which has brought down upon less unscrupulous practitioners censures deserved only by the few. "Make the most of him," is an axiom which we have known on more than one occasion inculcated by a grasping medical man, respecting each patient that fell to his unlucky lot; but we are thankful to believe that there is abundance of counteracting evidence which redounds largely to our collective professional honour. That there are secret deeds, too, of quiet and unobtrusive goodness, is no more to be doubted than that there are noble deeds on record which live and flourish for our example in the full blaze of day.

"Dr. Gregory, of Edinburgh, was as remarkable for his amiability as for his learning. It was his custom to receive from new pupils at his own house the fees for the privilege of attending his lectures. Whilst thus engaged one day, he left a student in his consulting-room, and went into an adjoining apartment for a fresh supply of admission tickets. In a mirror the doctor saw the student rise from his seat, and sweep into his pocket some guineas from a heap of gold (the fees of other students) that lay on the consulting-room table. Without saying a word at the moment, Dr. Gregory returned, dated the admission ticket, and gave it to the thief. He then politely attended him to the door, and on the threshold said to the young man with deep emotion, 'I saw what you did just now. Keep the money; I know what distress you must be in; but for God's sake never do it again—it can never succeed.' The pupil implored Gregory to take back the money, but the doctor said, 'Your punishment is this—you must keep it now you have taken it.' The reproof had a salutary effect; the youth turned out a good and honest man."

The instances (as we have before intimated) of Abernethy's munificence and generosity are very numerous,* though he has been commonly associated with deeds of a very different stamp. He could not curb his temper when he met with cackling and time-wasting patients, but he could not restrain the impulses of a generous heart when (as frequently happened) he was brought in contact with misery and distress.

In the second volume of Mr. Jeaffreson's work, we are brought at once into the midst of the heterogeneous. We have the "Loves and the Quarrels" of Physicians; Alchemy and Mesmerism; and an account of that impostor, Mr. St. John Long, who turned the heads of half London within the memory of this present generation. There is even a chapter headed, "Make way for the Ladies!" Mrs. Crowe is not brought under our notice (except by one quotation from her "Spiritualism"), doubtless because she yet flourishes in the pages of the "Spiritualist Magazine;" but we have an account of the notorious Joanna Stephens, who gulled the government to the tune of 5000*l.* for

for life of 500*l.* per annum, and the rank of Baron of the Empire." On the other hand, Dr. Glynn, of Cambridge, having gratuitously attended a poor lad during an attack of ague, was interrupted by the boy's mother entering his study to present him with the patient's domestic idol. "We can't get no rest (said the woman, gratefully) for thinking of all the trouble you have had, and so my boy resolved this morning on sending you his favourite magpie."

* See Macilwain's Memoirs of John Abernethy.

the revelation of the most worthless compounds.* "Crazy Sally" is introduced to us, and, dropping her euphonious maiden sobriquet, Mrs. Mapp appears upon the scene in all the attractiveness of matronly modesty. We shall leave to our Transatlantic brethren the full study of this comprehensive chapter, for where Bloomerism and Spiritualism are most rampant, the feminine gender graduates in medicine, and attains other degrees in arts than those suggested and fostered by natural impulses. And we content ourselves with reminding them, and others nearer our own shores, of the witticism of Lacon, "If physic be a trade, it is a trade of all others the most exactly cut out for a rogue." The history of St. John Long may teach this, as also the history of more modern "professors," and even of some who are fattening under our immediate eyes with ill-gotten gains of imposture. Quackery, indeed, is so prevalent amongst us, and nervous and impressional subjects are so obnoxious to its wily insinuations, that a most abundant harvest is reaped by those undeserving creatures who have not sown, and gathered by reptiles who have not strawed. It is meat and raiment to some to combine a knowledge of the weakness of humanity with herbal decoction, spatular pliancy, elaborate pill-boxes, and unblushing effrontery. It is to little purpose to point such persons to Dr. Parr's admirable definition of a quack, for they are conscious, without being ashamed, of their own similitude to the picture. "The term *Quack* is applicable to all who, by pompous pretences, mean insinuations, and indirect promises, endeavour to obtain that confidence to which neither education, merit, nor experience entitle them."

The shameless riotings of St. John Long in the very heart of the best London society only find their solution in that pruriency which is an integral part of certain temperaments, and which is frequently hard to restrain within the limits of conventional propriety. The curiosity of all, the natural enjoyment of many, the occupation which it gave to the indolent and voluptuous, all these things, combined with an interesting *personnel*, and a fascinating and pretentious manner on the part of the operator, gave an unparalleled success to a man whose only

* "My medicines (says the lady herself) are a powder, a decoction, and pills. The powder consists of eggshells and snails—both calcined. The decoction is made by boiling some herbs (together with a ball, which consists of soap, swines'-cresses burnt to a blackness, and honey) in water. The pills consist of snails calcined, wild carrot seeds, burdock seeds, ashen keys, hips and hawes—all burnt to a blackness—soap and honey." For this preposterous recipe the Government actually paid 5000*l.*, after having received from a special commission appointed to investigate the evidence on related cures, a certificate of entire satisfaction with the particulars which had been laid before them. We give our readers this valuable specimen of "senatorial wisdom" upon which the grant was made. "March 5, 1739.—We whose names are underwritten, being the major part of the justices appointed by an Act of Parliament, entitled, '*An Act for providing a reward to Joanna Stephens, upon proper discovery to be made by her, for the use of the publick, of the medicines prepared by her,*' do certify, that the said Joanna Stephens did, with all convenient speed after the passing of the said Act, make a discovery to our satisfaction, for the use of the publick, of the said medicines, and of her method of preparing the same; and that we have examined the said medicines, and of her method of preparing the same, and are convinced, by experiment, of the *utility*, *efficacy*, and *dissolving power* thereof. (Signed)—Jo. Cant.; Hardwicke, C.; Wilmington, P.; Godolphin, C. P. S.; Dorset; Montague; Pembroke; Baltimore; Cornbury; Gloucester; Tho. Oxford; Ste. Poyntz; Stephen Hales; Jo. Gardiner; Sim. Burton; Petershaw; D. Hartley; W. Cheselden; C. Hawkins; Sam. Sharp." We blush for the three last names appended to this certificate.

merit was his consistency, and whose most pardonable fault was his ignorance. Some slight idea of this charlatan's success may be gathered from the following observations by a writer in the 'Gentleman's Magazine' for 1843:

"In England, after Sir Astley, whose superiority of mind or dexterity of hand stood uncontested, another practitioner in that category of the faculty of which it has been said, '*Periculis nostris, et experimenta per mortis agunt medici,*' the once famous St. John Long was, I believe, the most largely requited. I had some previous knowledge of him, and in 1830 he showed me his pass-book with his bankers, Sir Claude Scott and Co., displaying a series of credits from July, 1829, to July, 1830, or a single year's operations, to the extent of 13,400*l.*" "There are yet to be found (says Mr. Jeaffreson, with, we fear, too much truth) in English society, ladies—not silly, frivolous women, but some of those on whom the world of intellect has put the stamp of its approval—who cherish such tender reminiscences of St. John Long that they cannot mention his name without their eyes becoming bright with tears. Of course this proves nothing, save the credulity and fond infatuations of the fair ones who love. The hands of women decked Nero's tomb with flowers."

Within the fencing of narrower limits there is even now a very successful "trade" carried on by educated men, who pander for their own profit to the weakness of those whose monomania is a need of mesmeric and other manipulations. And there is too much reason to suppose that even the most civilizing influences of modern times will have no effect in ameliorating a state of things which is hereditarily acquired, and developed by the social and physiological accidents of our condition.

The transition from Quackery to Quakerism is forced upon us by a euphonious alliteration, and by a desire to compare the heroic treatment of a distinguished member of the Society of Friends (as recorded in the following couplet) with the equally heroic conduct alluded to by Mr. Jeaffreson in his chapter on "The Quarrels of Physicians." The generous and liberal Dr. Lettsom, the pupil of Akenside, and the friend of Boswell, whose professional income reached the high figure of 12,000*l.* per annum, thus gives expression in doggrel English to his own depressing agency, and his subsequent indifference:

"When patients come to I,
I physics, bleeds, and sweats 'em,
Then—if they choose to die,
What's that to I—I lets 'em." (I. Lettsom.)*

Surely this is a narration of systematic "assault and battery" upon confiding patients, which is hardly equalled by the personal encounters

* Lettsom was an intelligent and amusing writer, and, though not an accomplished physician, a generous and considerate friend. He contributed largely to the 'Gentleman's Magazine,' under the signature of "Mottles"—the anagram of his own name. In his 'History of some of the Effects of Hard Drinking,' he gives a thermometrical scale of temperance and intemperance, allotting 70° to each of the two conditions. Against the highest degree of temperance is marked *water*, under which, at distances of 10°, follow *milk-and water*, *small beer*, *cyder and perry*, *wine*, *porter*, *strong beer*. The 10th degree of intemperance is *punch*; the 20th *toddy and frank*; the 30th, *grog and brandy and water*; the 40th, *flip and shrub*; &c. &c. In the same way, vices, diseases, and punishments were tabulated by the illustrious "Mottles."

of Dr. Williams and Dr. Bennet, Dr. Smith and Dr. Jeffries (in America), Dr. Hennis and Sir John Jeffcott (as recently as 1853).*

"John Barrowby was a Censor of the College of Physicians, he was challenged, under very remarkable circumstances, by Matthew Baillie, who afterwards achieved the highest distinction in his profession. Baillie, then a young man, and smarting under the ignominy of being plucked by the college, was resolved on taking vengeance on that learned body by shooting one of their Censors. Barrow had quite enough pluck for a man of his small stature, but he did not see the fun of being riddled by a young Scotchman, simply because he had done his duty; so he replied, 'In point of age, sir, I am only third Censor. When you have killed our President, Sir Hans Sloane, and the two Senior Censors, then I'll meet you.'"

More pleasant than these personal encounters of flesh and blood are the antagonistic records of rival pens. What can be more admirable in its way than the following duel between Dr. Cheyne and Dr. Wynter? The former had been compelled to reduce his own obesity by the most rigid abstemiousness; and having personal experience of its efficacy, he inculcated the same upon all his patients, especially enjoining the substitution of milk in lieu of fermented liquors. The latter took up the cudgels, resented the advice of Dr. Cheyne as a libel upon the habits of the community, and fired at him the following stanzas:

Dr. Wynter to Dr. Cheyne.

"Tell me from whom, fat-headed Scot,
Thou didst thy system learn;
From Hippocrate thou hadst it not,
Nor Celsus, nor Pitcairn.
Suppose we own that milk is good,
And say the same of grass;
The one for babes is only food,
The other for an ass.
Doctor, one new prescription try
(A friend's advice forgive),
Eat grass, reduce thyself and die,
Thy patients then may live."

To which the Scotchman replied with a more brilliant and effective broadside.

Dr. Cheyne to Dr. Wynter.

"My system, Doctor, is my own,
No tutor I pretend;
My blunders hurt myself alone,
But yours your dearest friend.
Were you to milk and straw confined,
Thrice happy might you be;
Perhaps you might regain your mind,
And from your wit be free.
I can't your kind prescription try,
But heartily forgive;
'Tis natural you should wish me die,
That you yourself may live."

We have given a brief space to the "Quarrels of Physicians;" we have nor time nor inclination for their "Loves." The subject, indeed,

* Vide Millingen's History of Duelling.

is too delicate for our rude handling. Mr. Jeaffreson implies that the profession has unusual opportunities for the prosecution of the tender passion, and for the formation of great alliances. It would not appear, however, that they have achieved any very brilliant triumphs. There is only one medical duke in the annals of the peerage.

The literature and art by which the profession has been distinguished, if not more congenial to our taste, is at least more within the scope of our capacity. It is a pleasure to think upon the literary tastes of the many members of the Faculty, to dwell upon the fruition of those tastes, in works which have been for previous, which are now for our own, and will be yet for future generations.

Among those literary names which have lent lustre to the medical profession, we mention John Locke* and Sir James Mackintosh. Locke was for some time an active practitioner, and Sir James was "a practising physician till ambition and poverty made him select a more lucrative vocation, and turn his energies to the Bar." Smollet was an apothecary, who took to literary pursuits because he could not eke out a scanty living from his practice. His works proved unusually attractive, and his success fired him with the ambition of graduating in medicine. To these names may be added Garth, Blackmore, Arbuthnot, Akenside, Armstrong, Mason Good, Millingen, Paris, &c. The father of the celebrated scholar, Dr. Parr, was also a medical man. And the son gives a reminiscence of his youthful occupation in his father's "shop" in the following anecdote:

"At that early age his critical taste and faculty caused him to subject the prescriptions that came under his notice to a more exact scrutiny than the dog-latin of physicians usually undergoes. 'Father,' cried the boy, glancing his eye over a prescription, 'here's another mistake in the grammar!' 'Sam,' answered the irritable sire, 'damn the prescription, make up the medicine.'"

"Wolcot, better known as Peter Pindar, was a medical practitioner, his father and many of his ancestors having followed the same calling in Devonshire and Cornwall." He went out as medical officer to the household of Sir Wm. Trelawney, on his appointment to the Governorship of Jamaica. Wolcot essayed to create for himself a practice in London on his return from the West Indies. But in this attempt he was unsuccessful, and found congenial occupation and profit in political satires and other productions. His improvident habits necessitated the sale of his copyrights, in negotiating which he had a violent outbreak with the trade in Paternoster-row, whom he satirized in the following severe and unjust verses:

"Fired with the love of rhyme, and, let me say,
Of virtue, too, I sound the moral lay;
Much like St. Paul (who solemnly protests
He battled hard at Ephesus with beasts),

* The following letter was addressed by Locke to Sir Hans Sloane: "Dear Sir,—I have a patient here sick of the fever at this season. It seems not violent; but I am told 'tis a sort that is not easily thrown off. I desire to know of you what your fevers in town are, and what method you find most successful in them? I shall be obliged by your favour if you will give me a word or two by to-morrow's post, and direct it to me, to be left at Mr. Harrison's, in the 'Crown,' at Harlow. I am, Sir, your most humble servant,—J. LOCKE."

I've fought with lions, monkeys, bulls, and bears,
And got half Noah's ark about my ears;
Nay, more (which all the courts of justice know),
Fought with the brutes of Paternoster-row."

It is true, indeed, that even now our brutes and our bears are not confined to the Zoological Gardens; but we can at least affirm that they are not so rampant and obstructive in these days in Paternoster-row as to merit the satire of modern Peter Pindars. Walcot was the friend of Opie; indeed, it was through the poet's instrumentality that the young artist was enabled to take his first flight in crayons and colours. That poet "whose name was writ in water," and whose mind was too acutely sensitive to do battle in the literary path which he had chosen for himself, much less in the rugged lanes and bye-ways of professional labour, was initiated into the mysteries of Æsculapius, "serving his time" with an Edmonton surgeon, and subsequently becoming a student at St. Thomas's Hospital. We know the melancholy close to that young ambition; and many of us, doubtless, have stood upon the poet's grave in that wondrous city now garrisoned by foreign troops, and "childless and crownless in her voiceless woe." The poet Crabbe, likewise—"Nature's sternest painter, yet the best"—was formerly in the ranks of medicine. His father was brutal, but happily did not transmit to his eldest son the qualities which merit the above zoological distinction. The son was apprenticed to Mr. Page, a surgeon of Woodbridge, in Suffolk. He tried, at the expiration of his term, to astonish the good people of his native town, Aldborough, by the bright bottles which bespoke the old apothecary, and now the modern chemist. But Crabbe had not walked the London hospitals, and met, therefore, with little success. A medico-military ardour, however, found its realization in the Warwickshire militia, to which regiment he was appointed surgeon. Yet poverty knocked at the young man's door, and drove him away to seek the chances of another profession, where the loaves and fishes are more equally distributed than in our own. The generous Burke took him by the hand. Crabbe was ordained, and, becoming chaplain to the Duke of Rutland, obtained some church preferment which ensured him a quiet and happy existence. Through the poet's marriage, he eventually came into the possession of considerable landed property.

The position of Crabbe as apothecary in a small country town gives us an opportunity of here contrasting the genus apothecary of that day with its modern development—the well-educated general practitioner. No profession has made such rapid educational strides as ours during the present century. It is impossible to over-estimate the demands which are made upon the time, and patience, and skill of thousands of our brethren in populous and remote districts; and equally impossible to speak too highly of that cheerful acquiescence in all the trying incidents of life, that general capacity for every emergency of position, and that considerate kindness towards the poor, which have given a sterling character to those who are necessitated to break through the narrow and exclusive limits of professional specialty, and surrender themselves to the more comprehensive and useful practice of our compound art. Formerly, to any little accident requiring the most ordinary surgical knowledge, the country apothecary was quite unequal. Long distances, in days when travelling was no light matter, had to be encountered, to bring from a neighbouring town the pompous professional big-wig, to give the benefit of his large experience and his sagacious knowledge to the unhappy patient and the bewildered apothecary. In these times, the Union-surgeon, and every general practitioner in the country, are equal and eager to undertake duties for which a sound education has given them the most complete aptitude. We cannot yet go so far as to say that even our most skilful surgeons have acquired the manual dexterity of the photographer, who, in the forcible phraseology of Mr. Punch, proclaims that he is equal to every occasion; and will "take yer 'ed off for sixpence, and yer 'ole body for a shillin." But certainly it is but comparatively rare for the country surgeon to call in the specialist, who finds an abundant sphere of usefulness, and opportunity for pecuniary acquirement, in the morning consultations, and in attendance upon the wealthiest classes. And we will take this opportunity of saying that the mania for special hospitals is becoming so great as to threaten very serious results, as regards the division of professional study and labour. The subject has already engaged the attention of some of the leading members of our fraternity, with a result as yet unappreciable. The overstocking of our ranks, and the imperious requirements of social conventionalities, will suffice in a measure to explain the division which we deplore, but do not see the means of escaping from. My lady must take her hectic daughter to one whose whole time is given (or supposed to be given) to the study of that disease which marches with cowardly but irresistible strides against the weak and lovely. My lord must take his patched and pimpled son to some dermatologist whose experiences are confined to the eruptive vagaries of man's outer covering. The dyspeptic and over-gorged bon-vivant must claim for his alimentary economy the wise supervision of some Hippocrates steeped in the lore of gastric juice and saccharine assimilation. The self-contemplating valetudinarian, freed from the active duties of life, must find his solace under some modern system which plays with his physical and moral weakness, and fans the feebleness of mortality. The hysteric fancies of the other sex, too slothful for the active duty which constitutes health, yet ever solicitous to find health through channels in which it never flows, must equally have some learned and interesting medical adviser, to whom it can pour out its complaints and reveal the secrets of its psycho-physiological life. These are the social verities which map out the division of professional labour, and give an insuperable reality to the development of medical and surgical specialties.

Mr. Jeaffreson observes of the country doctors of the middle and close of the last century:

"They were a rude, vulgar, keen-witted set of men, possessing much the same sort of intelligence, and disfigured by the same kind of ignorance, as a country gentleman expects now to find in his farrier. They had to do battle with the village nurses, at the best on equal terms, often at a disadvantage.

Masculine dignity, and superior medical erudition, were in many districts of less account than the force of old usage and the sense of decorum that supported the lady practitioners. Mrs. Shandy had an express provision in her marriage settlement, securing her from the ignorance of country doctors. Of course, in respect to learning and personal acquirements, the rural practitioners, as a class, varied very much in accordance with the intelligence and culture of the district in which their days were spent, with the class and character of their patients, and with their own connexions and original social condition. On his Yorkshire living, Sterne came in contact with a rough lot. The Whitworth Taylors were captains and leaders of the army in which Dr. Slop was a private. The original of the last-mentioned worthy was so ill-read that he mistook Lithopædii Senonensis Icon for the name of a distinguished surgical authority, and under this erroneous impression, quoted Lithopædus Senonensis with the extreme of gravity." (vol. ii. p. 280-1.)

The kindred story of *Tinctura Ejusdem* must be too well known to render its repetition necessary. But is the story of Dr. Standish, who flourished "when George the Third was king," equally well known? A little ungovernable political scoundrel, he thrashed his wife with a dog-whip. Her subsequent flight led to the following advertisement, which we are surprised has escaped the recording notice of Dr. Andrew Wynter, in his 'Curiosities of Civilization:'

"*Dr. Standish to all whom it may concern.*—Dr. Standish's wife having run away, this is to give notice that he'll be skinned before he pays her debts, and that he wants a housekeeper. Dr. Standish doesn't want good looks in a woman; but she must know how to hold her tongue and cook a plain joint. He gives ten pounds. Mrs. Standish needn't apply—she's too much of a lady."

Surely this advertisement makes a most unreasonable demand. We never heard of a woman who could cook a plain joint satisfactorily and also hold her tongue.

Here is a picture, by a writer of that period, of a country doctor of the last century—one who has earned for himself an imperishable name, and only within the last year been inducted to his seat in Trafalgar-square, under the shadow of the semi-draped "First Gentleman in Europe:"

"When first I saw him, it was on Frampton-green. I was somewhat his junior in years, and had heard so much of him that I had no small curiosity to see him. He was dressed in a blue coat and yellow buttons, buckskins, well-polished jockey-boots, with handsome silver spurs, and he carried a smart whip with a silver handle. His wig, after the fashion, was done up in a club; and he wore a broad-brimmed hat."

Such was Edward Jenner, long the scorn of the ignorant and the prejudiced. No man was ever more misrepresented or abused: no man was ever more confident in the reality of a great discovery, in its prospective acknowledgment by the world, and in the complete reversal of a premature verdict, which awaited him. Truly has Burke said, "Obloquy is a necessary ingredient in the composition of all true glory."

Mr. Jeaffreson gives us, in the last place, a slight sketch of the "medical buildings" of this huge metropolis. We shall not linger with him here, for our readers are undoubtedly acquainted with the histories of those structures which have done, and are still doing, so much to alleviate the physical and mental sufferings of our London population. They know of Thomas Guy, the London stationer, of the hospital which bears his name, and with which is inseparably connected the lustre of so many members of our noble profession. They know of St. Thomas's, and St. Bartholomew's, of St. George's and of King's College Hospitals, &c., and of "Mad Tom" and Bedlam. To the hospital of Bethlem we would, in passing, invite any one who would wish to see what modern science and rational treatment, and the skill of an accomplished physician, are now achieving for the most miserable and cheerless of mankind. It may be sufficient to state that, with the exception of four of the hospitals above named, none in London is in possession of property at all adequate to the demand upon its working and beneficent machinery. "Supported by voluntary contributions" is the appealing motto borne by almost every structure, to indicate as forcibly as may be the precariousness of its capacity for continuous good. What London would be without these noble charities it is impossible to conceive. They cover a multitude of sins in those who suffer not, and minister inestimable comfort to those whose sole inheritance is the inheritance of woe. Amongst the "medical buildings" are included those famous fountains of erudition from which flow the living streams which utilize charity and purge away disease. It would be inconsistent with our expressed opinions of that kindliness which gives birth to our eleemosynary institutions if we again invited our readers to those boards before which they once trembled. They have got vivid recollections of "the College" and "the Hall," and of that graceful structure to which the London physicians migrated from Warwick-lane in 1825. Which of us cannot recur, with some feeling of reverential awe, to Pall Mall East, or Lincoln's-inn-fields, or that locality which flowing numbers thus allude to—

> "Nigh where Fleet ditch descends in sable streams,
> To wash his sooty Naiads in the Thames,
> There stands a structure on a rising hill,
> Where tyros take their freedom out to kill."

This retrospect of the medical profession may lead us, in spite of our many admitted imperfections, to be satisfied, by comparison, with what we are. It may stimulate also to better things—teach us to combine a more generous rivalry with a less spurious brotherhood than that which sometimes embraces us. We have to labour without ceasing in a calling which can ennoble every worthy member of the same, and which every worthy member may dignify with his own character. "The truest measure (says Bishop Jackson) of man's internal or proper excellences is his beneficial use or service in the great universe whereof he is a part." Each individual's "entitive perfection" is the guage of his power to do good to others, both by precept and example. The sufferings and the trials of our profession are many. But "he that hath not suffered (says Madame de Staël), what doth he know?" These things improve and dignify, and give to the bearer of the burden an advantage never acquired in any profession by the few whose lot is cast in pleasanter places, and who enjoy an immunity from the stern but purifying vicissitudes of the many.

REVIEW IX.

Second Annual Report of the General Board of Commissioners in Lunacy for Scotland.—Edinburgh, 1860. pp. 225.

ALTHOUGH this second Report of the Scotch Lunacy Commissioners does not possess the advantages of the preceding volume, in being the first reliable and complete account obtained in this country of the state of lunacy in Scotland, yet in intrinsic interest and in the value of its contents it may claim an equality with it. A similar arrangement of contents is followed in both reports, and we are thus enabled readily to compare the statistical facts and general circumstances detailed in the two.

The first important fact which calls for remark is the increase in the number of the insane who fell under the cognizance of the Commissioners in 1859, as compared with the returns for 1858. On the 1st of January, 1858, there were 5748, and on the same day in 1859, 5991. The actual number, however, at the latter date was 7878, or 1887 more than that just stated, the difference representing the number of "private single cases," as far as at present ascertained, but of which no estimate was attempted in the statistical table presented in the first Report. Treating this, therefore, as a constant quantity in the two years, we have, to make the comparison correct, deducted it from the total of 1859, and thus the increase for this year upon 1858 stands at 243, or 4·25 per cent. A further analysis of the tables shows that this increase is made up of 116 additional inmates in public asylums, of 76 in private asylums, and of 93 in private houses,—a total of 285, from which a reduction of 42 in the number of insane confined in workhouses leaves the sum, as before stated, at 243. The total number of private patients partaking of asylum accommodation is the same in 1859 as in 1858; but their distribution is so changed, that 809 instead of 786, or 3 per cent. more, are placed in public asylums, and 200 instead of 219, or nearly 10 per cent. less, are detained in private asylums. On the other hand, whilst the proportion of pauper insane has augmented in public asylums from 1594 to 1687, or nearly 6 per cent., it has likewise increased in the private asylums from 526 to 621, or nearly 19 per cent. Taking into consideration the fact that the number of pauper insane in public asylums—viz. 1687—is double that of private patients—viz. 809—in the same institutions, it appears from the foregoing calculations that the latter class of insane persons has received equal relief from those asylums with the former during the past year, 1859.

"This shows" (as the Commissioners observe) "the preference given by the friends of private patients to public asylums over private houses, and affords a strong argument in favour of providing accommodation of a superior kind in connexion with the district asylums."

The second clause of this quotation suggests an explanation,—not necessary, it may be, to many of our readers, yet desirable, lest it should be assumed that so many patients supported by private funds have usurped accommodation primarily intended for pauper lunatics—viz., that most of the Scotch public asylums are of a mixed character, having a department, sometimes a separate building, for private cases and another for pauper inmates. However, to proceed: the figures given above indicate a very slow extension of public asylum accommodation, whether for private or pauper insane; and hence it is that the proportion of the latter in licensed houses has so largely augmented,—a circumstance admitted to be less advantageous to their interests as well as more costly to the ratepayers than provision in properly organized county asylums.

Referring again to the table (p. iii.) presenting the distribution of the pauper insane, we observe that more remain unprovided with asylum accommodation than possess it—viz. 2672 distributed in poorhouses and private houses, against 2308 detained in public and private asylums; and although a decrease of 42 can be pointed to in the insane occupants of workhouses, yet in 1859 the number confined in these establishments—viz. 795—is not far short of half that of those for whom a due legal provision is furnished—viz. 1687—in public asylums, leaving out of the question the 1877 pauper lunatics living single in private houses, to whose wretched state, however, and need of asylum care, in not a few instances, both the present and previous Report abundantly testify; additional provision in district asylums is claimed for those 795 living in workhouses, and for the 621 detained in private asylums, or for 1416 additional patients, a number not far short of that for which accommodation was provided at the commencement of 1859; for as regards the desirability of the transfer of pauper lunatics from private to properly appointed public asylums, few objectors will be found; whilst with respect to lunatics in workhouses, the Scotch Commissioners reiterate their previously expressed opinions, which entirely concur with and enforce those of the English Lunacy Board, as well as of all competent persons who have weighed the subject—viz., that workhouses, together with workhouse lunatic wards, are unfit receptacles for the insane, detrimental to their welfare, and in the long run not economical.

On these points the following extracts from the Report before us are worth repeating:

> "It must of necessity be more for the interests of the districts to place their pauper lunatics in establishments under their own management, than to consign them to the custody of the proprietors of private asylums, who must draw their own profits from the payments made for the maintenance of the patients."

With this brief comment, this aspect of the matter from a purely business point of view, the Commissioners advise the withdrawal of all pauper lunatics from licensed houses, but when they would advocate their transfer from poor-houses to district asylums, they recognise the necessity of a much more elaborate defence of that recommendation to meet the many specious arguments against it resting on economical considerations. Yet, as we shall presently see, they are able fully to meet and overthrow such arguments, and to prove asylum accommodation to be not only, as all will admit, the best, but also the cheapest, for the insane. The following remarks apply particularly to poor-house lunatic wards, wherein an attempt is made to meet the wants of lunatics and to diminish or remove the objections which all perceive in treating sane and insane paupers on the same footing and under similar conditions:

"We strongly object to lunatic wards in poor-houses being used for this purpose, chiefly on the ground that the primary object of poor-houses is to afford a test for poverty, and to provide for the poor in the most economical manner. The fundamental principle on which these establishments are conducted is thus antagonistic to that which ought to regulate the treatment of lunatics, and which, briefly stated, is the provision of every comfort which can reasonably be demanded, to lighten the burden of perhaps the greatest calamity which can afflict humanity. We are, however, willing to admit that, notwithstanding the fundamental principle of economy which must necessarily be the guide of parochial boards in determining the treatment of paupers, some of them have attached to their poor-houses lunatic wards of a very creditable character for the accommodation of their insane poor. Nevertheless, the treatment of the patients in these establishments can scarcely in any instance be regarded as in harmony with the humane views of the day. There is generally a great deficiency in cheerfulness and comfort within doors, a prison-like aspect about the airing-grounds, and an almost total absence of the means of employment, both within and out of doors. Yet, notwithstanding the unfavourable contrast which poor-house lunatic wards must, in these respects, bear to asylums, the cost of the maintenance of patients in the former class of establishments, especially where no restriction on admission exists in connexion with the form of the malady with which they are affected, is perhaps as high as that of those placed in the latter. . . . But it must be remembered that there are other poor-houses in which the accommodation provided is exceedingly unsatisfactory, and in which the physical wants of the patients are provided for with but little departure from those economical principles which regulate the treatment of sane paupers. It is undoubtedly the duty of parochial boards to economise the funds of their respective parishes, but we are of opinion that this end would be attained in a much more satisfactory manner, as concerns both the ratepayers and the patients, were lunatic wards in poorhouses to be entirely abolished." (pp. xi. xii.)

A few pages further on the Commissioners resume their arguments against workhouse detention of lunatics, taking their stand on purely economical principles, and handling the pounds, shillings, and pence argument in such a manner as to entirely make good their views. To collect data for their arguments, they analyse the returns of expenditure on account of their lunatics of many parishes in Scotland, large and small, taking care to make allowance for circumstances which may interfere with the correctness of their results—such, for instance, as the facility of obtaining accommodation:

"Assuming that the returns are correct, some curious and unexpected results may be deduced from them, and more especially those made by parishes which adopt what may be considered preferential modes of disposing of their pauper lunatics, and which are, at the same time, of sufficient extent to afford a field for legitimate comparison. There are certain parishes, for example, which send all their lunatics to asylums, with the exception of those exempted as single patients. There are others which place them preferentially in the lunatic wards of poor-houses; and others, again, which divide them between asylums and poor-houses, sending to the former the recent and unmanageable cases, and placing in the latter the chronic and more tractable." (p. 27.)

The results deducible from a comparison of the relative expenditure of each of the three classes of parishes above indicated—

"Are extremely important, as indicating that asylum treatment is really more economical than poor-house treatment. They show that parishes which take the entire charge of their pauper lunatics, and treat those requiring segregation entirely in the lunatic wards of poor-houses, maintain the whole at an average rate of 19*l.* 4*s.* 11$\frac{1}{4}$*d.* per head; that those parishes which place only the more manageable of their patients requiring segregation in poor-houses, and send the rest to asylums, maintain the whole at an average rate of 17*l.* 1*s.* 5$\frac{1}{4}$*d.*; and, lastly, that those parishes which trust entirely to asylums for the care and treatment of such of their lunatics as require to be placed in establishments, maintain the whole at an average rate of 15*l.* 7*s.* 2*d.*" (p. 28.)

Here, then, we have the comparative economy of workhouse and of asylum provision for pauper lunatics set forth by figures which unmistakeably prove the superior cheapness of the latter, and this indeed to so considerable a degree that asylum accommodation turns out to be more than one-fifth per head less costly than the comparatively defective and detrimental lodging of insane patients in poor-houses.

Many thanks are due to the Scottish Lunacy Board for working out this problem in this manner; for may we not now entertain the hope that parochial officers may be disabused of their convictions that poor-house lunatic wards are the cheapest receptacles for the insane by these arithmetical calculations proving the contrary, when all arguments to induce them to prefer asylums as more conducive to the interests and happiness of unfortunate lunatics fail in their philanthropic object?

The above-quoted results are drawn from the returns of a very limited number of parishes; but the copious tables of the expenditure of the several parishes throughout Scotland, contained in Appendix C of this Report, will furnish ample material for further comparative calculations, and for the complete establishment of the conclusions arrived at by the Commissioners.

The embarrassment felt in the carrying out of the English Lunacy Acts, from the loose statutory definitions of lunacy, and particularly from the reference of lunatics to two classes—harmless and dangerous, has been felt alike in Scotland, and has led to the same injurious results—viz., the deprivation of many of the insane of their legal and necessary relief in properly constituted asylums, and the exposure of the public to the destructive propensities of such lunatics at large.

Ever and anon the community is startled by the details of some horrible murder, suicide, or other crime, by individuals of whose mental obliquity evidence is then at once forthcoming, and of whom it further appears, that though not sound in mind, they were left unguarded and without supervision, because this or that friend maintained them to be harmless. The fact is, the unpractised observer, whether medical or not, has a particular conception of a dangerous lunatic; his ideal being is the furious maniac, threatening in word, look, and gesture; and therefore when he meets with an insane person whose conduct is calm, and apparently under his control, whose conversation is probably generally rational, who can divert attention from his besetting delusion, and mislead his examiner respecting its influence over him, the conviction occurs that such an individual, though wrong in the head, has no harm in him and may be let go free. A large proportion of the visitors who go through the wards of our public asylums experience a certain feeling akin to disappointment at the absence of the dramatic incidents which they have associated in their minds with mental disorder; they suspect that they are excluded from some wards where their typical madmen are confined, and they will arrive at the general conclusion, that most or nearly all of the asylum inmates are poor harmless creatures who might be allowed their liberty. Unless instructed in the working of such establishments, and initiated in the phases of the insane state, they cannot appreciate the influence of the surrounding conditions of life, they cannot arrive at a correct conception of what is intended by the "moral treatment" of the insane.

The Scotch lunacy law requires the medical practitioner who is called upon for a certificate of insanity to state whether the individual in question is or is "not a lunatic within the meaning of the Act." This allows much divergence of opinion, according as the term lunatic is interpreted. The Commissioners observe that it is obvious the medical practitioner "thereby incurs a double responsibility, as he must be held to give an opinion, first, in regard to the mental state of the patient; and, secondly, as to the appropriate nature of the circumstances in which he is placed. It is at the same time evident that the certificate to the effect that any one is "not a lunatic in the meaning of the Act," does not necessarily imply that the person is of sane mind. Indeed, it is frequently expressly understood that the certificate is not intended to convey this meaning, but is granted merely as an expression of opinion that the patient is not likely to commit an act dangerous to himself and others. With what latitude medical men will occasionally undertake to determine on the presence or absence of danger, the Commissioners proceed to show by instances, among which is one where "pauper lunatics were withdrawn in wholesale fashion from our jurisdiction, in consequence of the parochial surgeon holding the opinion that they were not lunatics in the meaning of the statute." To remedy this ambiguity, and to obviate such unseemly discussions between themselves and those called upon to give certificates of lunacy, the Commissioners propose the very simple expedient of so amending the statute, "that the definition of lunacy should be so extended as to correspond with the phraseology of the thirty-fifth section, and embrace 'every insane person, idiot, or person of unsound mind.'" (p. 8.)

As may be presumed, there is a reason to be found why parochial authorities discover such a disposition to ignore the existence of lunacy "within the meaning of the Act;" and this reason, as indicated in the ensuing paragraph, is the presumed costliness of asylum provision.

"It has" (write the Commissioners) "on various occasions been asserted that we propose to send every pauper lunatic to an asylum, no matter what may be his mental condition; and there is reason to think that it is this idea which has led inspectors to procure medical certificates to the effect that their insane paupers do not come within the provisions of the Act."

The Commissioners hereupon take pains to disavow any such presumed intentions on their part of a wholesale deportation of insane people of all sorts from their present habitations to asylums, and to substantiate this disavowal refer to "the small proportion of cases (of single patients) in which they have recommended removal." But surely this circumstance cannot be regarded by the Commissioners with satisfaction or as a matter of congratulation, for their own history of the condition of single patients proclaims it as a fact that such patients are for the most part badly, often wretchedly taken care of, and should rightly, and for humanity's sake, receive the advantages of asylum care. We do not indeed pretend to say that all insane persons should be congregated into asylums—on the contrary, we consider that the principle of the aggregation of lunatics into huge asylums has already been pushed too far, and that their segregation, as some have termed it, or their distribution in cottage-homes or small refuges, is the problem awaiting solution at the hands of those concerned in making and superintending the provision for the pauper insane. This question, however, is much too wide for discussion on the present occasion.

We are glad to see that the Scottish Lunacy Commissioners are opposed to large asylums. Thus, after showing the influence of the distance of asylums on the distribution of the insane, and the enunciation of it as a sort of axiom, "that the number of patients sent to asylums diminishes in a ratio corresponding to the distance, and that the number of those which remain at home increases in a similar degree," they observe that—

"These results afford an indication that small asylums in convenient situations will more satisfactorily meet the wants of the country than large central establishments, which must necessarily be remote from considerable portions of the extensive districts which they are designed to accommodate. It is true that the progressive extension of the railway system will gradually afford greater facilities in removing patients to distant asylums; but distance, and the expense it involves, will nevertheless continue to exercise an unfavourable influence on the visits of relatives and on the discharge of patients on trial. It should also be borne in mind that the aggregation of the insane in large masses greatly increases the difficulty of providing for their proper care and treatment." (p. 10.)

The Report deals largely with the condition of those patients, both private and pauper, who are placed singly in private houses; and the Commissioners very rightly remark: "We feel that to secure the proper care of those pauper lunatics who are placed in private houses is one of the most important duties of the Board, and constitutes, perhaps, our greatest responsibility." (p. xxxvii.) And they "are inclined to ascribe great importance to the visitation of single patients, not only for improving the treatment and management of those actually visited, but for elevating the general condition of the insane, whether placed in asylums or in private houses." (p. xl.) Animated by these views, they have been very assiduous in their visitation of such patients, and during their two years' term of office they reported on "4097 lunatics placed as single patients, of whom 1904 were private patients, and 2193 paupers; of both classes, 3527 were seen and reported on, and 570 reported on from information derived from others." The copious appendices contained in the two Reports as yet published testify to their assiduity as well as to the great need for the inquiry, and it is with just pride that they refer to them, as embodying "a kind of information which, so far as known to us, is not within the reach of any other Board of Lunacy."

To facilitate their labours in this direction, to relieve asylums from over-crowding, and on economical grounds, the Commissioners propose that the restriction imposed by statute against receiving more than one lunatic in a house not licensed should be withdrawn, and that it be enacted—

"That any number of patients, not exceeding four, may be received into a private house, without the necessity for a license, provided the Board make previous inquiry into the nature of each case, and grant their sanction according to special forms for the admission of each individual patient. Under some such provision we feel satisfied a system of cottage accommodation would gradually spring up which would not only furnish more fitting accommodation for chronic patients than the lunatic wards of poor-houses, but would also be calculated to prove a valuable adjunct to asylums. . . . . By its adoption economy would ensue, and to the Visiting Commissioners the advantages would also be great. By placing three or four patients together, the number of houses requiring visitation would be greatly lessened, and the labour of inspection would be further diminished were the cottages generally grouped together."

We are happy to find these views expressed by the Commissioners, for they coincide with those we have for some time past entertained. We would willingly prosecute our remarks in this direction, but the length to which this notice has extended intimates the propriety of bringing it to a close; and in doing so, we have to express our thanks, in the name of all those interested in the well-being of the insane, to the Lunacy Commissioners for Scotland, for their very interesting and instructive report, of which we may add (without wishing to cast any slur on the English Board of Commissioners), that in our opinion it may advantageously compare with our similar English Reports, both in the matter and manner of its contents and in its typography and general getting up.

Review X.

1. *Tobacco: its History and Associations; including an Account of the Plant, with its Modes of Use in all Ages and Countries.* By F. W. Fairholt, F.S.A., &c.—*London*, 1859.
2. *Anti-Tobacco Placard, No. I. Opinions of Sir B. C. Brodie, Bart., D.C.L., F.R.S., &c., on the Effects of Tobacco on the Human System.* (From the 'Times' Newspaper, 31st August, 1860.)
3. *Tobacco: its History, Nature, and Effects on the Body and Mind; with the Opinions of Rev. Dr. Nott, &c.* By Joel Shew, M.D., Author of various works on Hydropathy.—*New York*, 1857.
4. *Tobacco: its History, Cultivation, Manufacture, and Adulterations. Its Use considered with Reference to its Influence on the Human Constitution.* By Andrew Steinmetz, Esq., Barrister-at-Law. —*London*, 1857.
5. *Tobacco: its Influences, Physical, Moral, and Religious.* A Lecture. By the Dean of Carlisle. Second Edition.—*London*, 1859.
6. *The Anti-Tobacco Journal.* A Monthly Periodical published by the British Anti-Tobacco Society, instituted April, 1853.
7. *Is Smoking Injurious? The Arguments Pro and Con, Rationally Considered.* By John Skelton, Esq., M.D. Addressed to the Working Classes. (Second Edition of Ten Thousand.)
8. *Juvenile Street-Smoking: Reasons for Seeking its Legislative Prohibition, in a Letter to the Lord Bishop of Manchester.* By the Secretary of the British Anti-Tobacco Society.—*London*, 1856.
9. *A Memento of the Cambridge Tobacco Riot.*
10. *The Substance of a Lecture on the Pernicious Properties and Injurious Effects of Tobacco.* By the same Author as the two preceding.
11. *The Smoker's Handbook; or, a Dissuasive against the Use of Tobacco.* By Anti-Fumo.—*London*, 1854.
12. *Fifty-four Objections to Tobacco.*
13. *Death in the Pipe; or, the Great Smoking Question.* By J. L. Milton, Member of the Royal College of Surgeons.—*London*, 1857.

From the "counterblaste" of Royalty, to the denunciation by the President of the Royal Society—a no inconsiderable interval of time in social history—tobacco has been an unfailing subject of abuse and praise, of commendation and condemnation, and in no measured terms. Considering its literature, the pros and cons for its use and abuse, it might seem a vain task to discourse on it dispassionately; yet this is what we propose to attempt—not, indeed, exhaustively, for that would require volumes, but briefly and partially, restricting our remarks chiefly to its effects from a physiological and pathological point of view.

The striking feature as regards its use is that whilst it is acknowledged to be a violent poison, and the smoking of it on first trial is in most cases productive of distressing symptoms—sufficient, it might be supposed, to produce an everlasting disgust—it has become more and more popular with little interruption, and has so spread from people to people, from country to country, that now it might be difficult to find a region in which it is unknown or a race that does not indulge in it. Such a diffusion, such a general use, must be admitted to imply some peculiar charm belonging to it, some effects, some virtues which make it so followed and beloved, and so far must be received, we think, as a precognition in its favour, and justifying the poet's rhapsody:

> "Sublime tobacco, which from east to west
> Cheers the tar's labour and the Turkman's rest."—BYRON.

And Charles Lamb's more sober reflection:

> "The old world was sure forlorn,
> Wanting thee!"

We need say little respecting the botany of the subject, or its history, the one being so well known, the other so imperfectly. It may suffice to remark that the plant yielding the leaf which in its prepared state constitutes the tobacco of commerce, belongs to the natural family Solanaceæ, a family above all others distinguished for remarkable poisonous qualities; that there are three species or varieties of it most worthy of note—*Nicotiana tabacum*, *N. rustica*, and *N. persica;* and that till the discovery of America, though it might have been known before in China, Hindostan, and Persia, it certainly was not known in Europe. By whom introduced, or from whence, is not altogether certain. Thus much, however, appears to be well established—viz., that the Spaniards and Portuguese first brought it from the New World, and that in Spain and Portugal its use first became popular. From these countries it passed into France and also into Italy. Into England it was brought by the great navigator Drake, and brought into fashion by another great man, Sir Walter Raleigh. Even the derivation of its name—its trivial name—is open to question—some deriving it from one of our West Indian islands, Tobago, others, with more appearance of probability, from the city of Tabasco, in New Spain, in the neighbourhood of which, from an early period, it had been largely and successfully cultivated. Nothing but the sound seems to be in favour of the former, as for nearly a century after tobacco had found its way into Europe, Tobago, the island, was uncultivated and uninhabited. Respecting its specific name, *Nicotiana*, no kind of doubt, however, exists whence derived; it is certain that it was borrowed from that of the individual who introduced it into France—Jean Nicot, Maître des Requêtes, Ambassador of Francis II. to Sebastian, King of Portugal, by whom and from whence we are assured it was brought in 1560, and by him presented to Catherine de Medicis and to the Grand Prieur, both of whose names it for a time bore, they being its great patrons, but only for a time, that of l'herbe à la Reine and l'herbe au Grand Prieur fading away into oblivion before the shorter and apter one of the ambassador. In Italy, where it was received with equal favour, it was honoured with the name of l'erba santa, on account of its supposed virtues, and having, according to report, been introduced by the Cardinal Santa Croce, on his return from Spain and Portugal in 1589, giving rise to another rhapsody:

> "Herb of immortal fame!
> Which hither first with *Santa Croce* came,
> When he his time of nunciature expired,
> Back from the court of Portugal retired;
> Even as his predecessors, great and good,
> Brought home the cross."

In saying that the history of the plant was obscure, we had in mind its early, its first period—that of its introduction into Europe. Of its later history there is ample information, and of a very curious and instructive kind, which we shall barely glance at, showing how little authority can control taste, the use of tobacco having been prohibited by imperial mandates, subjecting the offence, as it was then considered, to the severest punishment;*—how discordant are the opinions of learned doctors, extreme and opposite views having from time to time almost, if not quite up to the present, been taken of its powers, in whatever form employed;—altogether strongly illustrating the difficulty there is in the attainment of truth where human feeling interferes, and the impossibility of acquiring precise knowledge, except by the methods of the exact sciences, of which one only has lent its aid to enlighten us on tobacco—viz., chemistry—physiology, we regret to think (the next best aid), hardly yet coming under the same category.

From chemistry we learn that tobacco, the pure leaf in its fresh state, is very compounded, that it consists of at least ten different ingredients, and these, per 10,000, in the following proportion: 8828 water, 287 bitter extractive, 174 gum, 27 resin, 26 albumen, 105 amylaceous gluten, 133 organic and inorganic salts, 497 ligneous fibre, 6 a peculiar oily-like substance named nicotine, and 1 a camphor-like, volatile oil, called nicotianin. Of all these ingredients, nicotine is the most important, being admitted to be the chief active principle of tobacco—that on which all, or mostly all, the peculiar qualities of the leaf depend. This, one of the vegetable alkaloids, combines with acids, forming neutral crystallizable salts; liquid at ordinary temperatures, it solidifies at 22° Fahr.; is heavier than water; volatile at 284°, boils at 472°; slightly soluble in water, it is more so in alcohol; its odour is that of tobacco in its greatest intensity, its taste acrid in the extreme. The process for obtaining it is not difficult, though somewhat complicated; we need not describe it, it may be found detailed in every work on organic chemistry. From the researches of M. Schloesing, it would appear that this alkaloid varies in proportion in the different

* The Grand-Duke Michael Federoits prohibited the importation and use of tobacco into Russia by the penalty of the bastinado for the first offence, of the loss of the nose for the second, and of life for the third. The Sultan Amurath IV. made smoking a capital crime, as also did a Shah of Persia, Shah Abbas; the former from the belief that it was a check to the increase of population—a charge more than once since repeated, and with some probability as regards its abuse.

kinds of tobacco. The following are his results per cent.—that is, of nicotine, from the thoroughly-dried leaf:

Lot	7·96
Lot Garonne	7·34
Nord	6·58
Ile de Vilaine	6·29
Pas de Calais	4·94
Alsatia	3·21
Virginia	6·87
Kentucky	6·09
Maryland	2·29
Havana, less than	2·00*

According to the analogy of other active principles of plants, nicotine may be viewed as a secretion, and, like them generally, not diffused, but isolated and retained in cells. These cells are distinct under the microscope in the fresh leaf. Owing to this circumstance, the fresh leaf has none of the odour of the prepared article.

We shall now pass on to our main subject—its effects on the animal system; noticing, first, the experiments which have been made to elucidate its action, commencing with those of Sir Benjamin Brodie, published in the 'Transactions of the Royal Society,' as being most exact and reliable. They were made with a strong infusion of tobacco, and with its empyreumatic oil, the effects of which we shall see were remarkably different. In his first experiment

"Four ounces of infusion of tobacco were injected into the rectum of a dog. Four minutes afterwards he retched, but did not vomit; he then became faint and lay motionless on one side; at the end of nine minutes from the time of the injection the heart could not be felt, he gasped for breath at long intervals, and in another minute there was no appearance whatever of life."

It is added,

"I immediately laid open the cavities of the thorax and abdomen. The heart was much distended, and had entirely ceased to contract; there was no peristaltic motion of the intestines."

Three other experiments are detailed, affording the same results. His first experiment, using the empyreumatic oil, he describes as follows:

"Less than a drop of the oil was applied to the tongue of a young cat. Instantly violent convulsions took place in all the muscles, and the respiration became very frequent. In five minutes after its application she lay on one side insensible, with slight spasmodic actions of the muscles. At the end of eleven minutes she retched, but did not vomit. In a quarter of an hour she appeared to be recovering. I repeated the application of the poison, and she was again seized with violent convulsions, and became insensible, breathing at long intervals; and in two minutes from the second application respiration had entirely ceased, and she was apparently dead. On opening the thorax I found the heart acting with regularity and strength, circulating dark-coloured blood. On dissection nothing remarkable was found in the appearance of the tongue or brain."

* Comptes Rendus, Dec. 1846.

Two more experiments are detailed, in which the oil, suspended in a little water, was injected into the rectum of a dog and of a cat, with a very like result. From them—those with the infusion and those with the oil—the author concluded that the former injected into the intestine has the power of rendering the heart insensible to the stimulus of the blood, thus arresting the circulation, occasioning death by syncope, taking effect through the medium of the nervous system, having found in an experiment on a decapitated dog that after the injection the heart continued to act. That the latter—the oil—acts simply by "destroying the functions of the brain," death resulting "because respiration is under the influence of the brain, and ceases when its functions are destroyed."

Orfila's researches on the same poison were made somewhat later, and are given in the third volume of his 'Toxicologie Générale,' published in 1815. He employed in his experiments tobacco in substance and the infusion—the former, to which we shall restrict our notice, introduced into the stomach, and applied to the surface of a wound. The results of one of the trials were the following:

"At two o'clock sixteen grains moistened with water were applied to the cellular tissue on the inside of the thigh of a robust little dog [carlin]. Ten minutes after the animal vomited twice. After six hours it became slightly vertigenous, and its hind legs had a tremulous motion. It died in the night. The lungs were found of a deep red, with livid spots, their tissue rather denser than natural, the alimentary canal unchanged, the part operated upon a little inflamed."

To his own experiments Orfila adds an account of some made by Professor Macartney, of Dublin, on the brain and nerves. The following is one of them:

"The superior portion of the cranium of a rabbit having been removed, and a part also of the membranes of the brain, as soon as the flow of blood had ceased, some drops of the empyreumatic oil were applied to the surface of the encephalon. After half an hour the animal exhibited no remarkable symptom. The death was effected by pouring two drops on the tongue."

In another experiment, half a scruple of the oil was introduced into the hemispheres of the brain, but, after thirty minutes, without any apparent effect. Three drops applied to the tongue of this rabbit occasioned immediate death. The same want of effect was witnessed when the oil was brought in contact with the sciatic nerve, provided it was isolated; and also without effect even when the extremities of this nerve, after a transverse division, were immersed in the oil and left for an hour. Of the conclusions arrived at by M. Orfila, the following are the chief:—1st. That "the empyreumatic oil does not act directly on the brain or on the nervous trunks, but through the nervous system in a manner not yet well determined." 2ndly. That the infusion of the leaf acts specially on the nervous system, its poisonous principle being absorbed and carried into the circulation. He considers "a general tremor, rarely witnessed from other poisons, an almost constant accompaniment of its action."

These conclusions we are inclined to receive as most warranted by the results of the experiments made, including some which we ourselves have instituted, using a preparation supplied as nicotine, but which we believe to have been a mixture of nicotine and of the volatile nicotianin. The subjects of the experiments were mice. We shall notice briefly a small number of them, and, first, those which indicated the mixed nature of the poison.

1. A vigorous mouse was placed under a bell glass, a common "finger glass," with 28 grains of the so-called nicotine, in an open wide-mouthed vial. The animal immediately became affected, dull and drowsy; during the first three or four hours it now and then ate a little bread; after thirteen hours it seemed comatose, being motionless with the exception of its head, which was tremulous. This was late at night; the following morning it was found dead and rigid. The fluid was reduced by evaporation to the amount of 6·3 grains.

2. When the like trial was made with the fluid in equal quantity, after exposure for some time to the air, a mouse confined with it so that it could not touch it, well supplied with air, as in the former instance, seemed to suffer no inconvenience, its activity and appetite continuing unimpaired even when thus confined two whole days. The effects of the less volatile portion of the fluid as applied by contact were very strongly marked.

3. A mouse confined under a bell glass with a portion of this fluid, inadvertently in its efforts to escape touched its surface with its lips. In two or three seconds it was convulsed, in as many more fell on its side and was motionless. Immediately opened, the heart was found contracting, and it continued to act for some time, even after fifty-five minutes a slight movement was seen of its auricles. The fluid, which had before been weighed, had by the contact sustained a loss of ·4 grain. In other trials death was produced as rapidly by the contact of as minute a quantity to the same part, and in all but one instance was preceded by convulsions; in two instances the heart continued to act feebly for a minute or two, in one its action ceased immediately and was not excited by puncture.

4. The last experiment we shall mention was on the heart itself. An active mouse was rendered unconscious by ether. In this state the chest and abdomen were laid open. A drop of the fluid was instantly applied to the pulsating heart; its action, gradually becoming feebler, continued for about three minutes; the peristaltic motion of the intestines for a shorter time.

In whatever manner inferentially all these results may be received, they must be admitted, we think, as decisive as regards the virulency of the poison of tobacco on brute animals; and that its effects are analogous on man there seems to be little room for doubt. Whoever has witnessed or read of the operation of the infusion of the leaf given in cases of strangulated hernia must feel persuaded of the truth of this. Orfila quotes several instances in proof of the same:—1st, of three children, after the application of a liniment made of powdered tobacco and butter to the head for tinea, being seized with vomiting, vertigo, and fainting, and for twenty-four hours walking as if drunk.

2nd, of a decoction applied to parts affected with itch occasioning violent vomitings and convulsions. 3rd, of a person falling into a state of somnolency and dying apoplectic after the excessive use of snuff; the words in the original are—" pour avoir pris par le nez une trop grande quantité de poudre de tabac." 4th. The instance of the celebrated Santeuil, who, after having drank a glass of wine into which some tobacco had been put, was attacked with vomiting and most severe pains, under which he expired. It would not be difficult to add to these examples many others, some well authenticated, others more than doubtful. We shall give only one of each, leaving the reader to decide to which class each belongs. " Borrhi, in a letter to Bartholine, mentions a person who, through excess of smoking, had dried his brain to that degree, that after his death there was nothing found in his skull but a little black lump, consisting of mere membranes." This is a quotation from 'A New and Complete Dictionary of the Arts and Sciences,' published in 1755 by a society of gentlemen. Père Labat, in the sixth volume of his 'Nouveau Voyage aux Iles de l'Amérique,' of the date of 1742, amongst other particulars which he gives in his very ample and amusing account of tobacco, relates, as from his own knowledge, how the enjoyment of a great entertainment was interrupted from the direful effects of tobacco on the guests who had partaken of a turkey into the stuffing of which tobacco had been introduced by a negro cook of the family in a fit of jealousy, in consequence of another cook of superior skill having been employed to dress the dinner. The turkey, by those who were helped of it, was pronounced excellent; "but one after another they were taken ill, those who had largely ate of it were seized with vomiting and purging, others suffered cruel pains—in brief, the fête was broken up, on crut tout le monde empoisonné." The surgeon of the family was hurriedly sent for, and all his *confrères* from the surrounding country. They "anatomised" the different meats which had been served at table, and at length found what cleared up the mystery—the offending tobacco. Aid was immediately administered; those who were merely griped were relieved by lavements, those reduced by excessive evacuations were comforted by cordials, the least ill were on their legs in from twelve to fifteen hours, others kept their beds for two days.

We shall give another anecdote, premising that the thesis we shall refer to was not maintained in the schools of Oxford or Cambridge of the present day, or before the College of Physicians or Surgeons of our time and country, but in the schools of medicine of Paris long ago—viz., on the 26th of March 1699. The question was "An tabaci usu frequenti vitæ summa brevior?" M. Claude Berger, Parisian, M.B., having to sustain the act, proved in the most demonstrative manner that the frequent use of the leaf shortens life—"Ergo en frequenti tabaci usu vitæ summa brevior." Père Labat, who relates the disputation and the solemn decision arrived at, remarks that the takers of tobacco had their fears in some measure quieted by one circumstance, that of the president, who admitted the force of the argument, and supported it with reasons of his own, exhibiting a disagreement between his tongue and his nose, having had during the whole of the act his snuff-box in his hand—"Et ne cessa pas un moment de prendre du tabac;" the author adding—"Etoit-il bien convaincu de ce qu'il vouloit persuader aux autres? Je le laisse à penser à mes lectures."

Though more than a century and a half have passed since this discussion was formally held, is not the subject still an open question? We apprehend we must answer in the affirmative; we apprehend were this very question—"An en tabaci usu frequenti vitæ summa brevior?" submitted to the free vote of the profession, that whilst there would be some unanimity in the decision that tobacco in very great excess is injurious and likely to shorten life, there would be a division of the suffrage regarding its moderate use and an occasional excess. No longer than 1857 proof was afforded of such a difference of opinion. In the first volume of the 'Lancet' for that year the matter is largely discussed, arising out of the authoritative assertion of a public lecturer that the vice of smoking is one of the causes of paralysis and of its increase at the present time. There are in that single volume referred to as many as twenty-six different communications on the subject, some holding one doctrine, some another, with titles in accordance—such as, "The Salutary Effects of Smoking;" "Smoking not a Cause of Insanity;" "Is Smoking a Blessing?" "Tobacco and Crime;" "Can Tobacco be Deprived of its Injurious Qualities?" "Temperance and Justice, the Handmaids of Truth in the Tobacco Question."

The author of the paper with the last-mentioned title, as that title would indicate, belongs to neither extreme. He says:

"He can as little repeat the words of Molière—'Qui vit sans tabac n'est pas digne de vivre, il rejouít et purge les cerveaux humains et il instruit les âmes à la vertu,' as he can share the fears of the alarmist, lest the Gibbon of our Rome discover reason for eloquent mourning on the 'weed' as the cause of our national decay."

He, viewing the subject dispassionately and after much experience in the public service, arrives at certain conclusions of a moderate kind, which we think are not far from the truth. It is due to him to mention his name—it is Mr. Gamgee, late surgeon of the first class, principal medical officer of the British Italian Legion, and who, he declares, has never taken a particle of tobacco in any form, from his dislike to it. Like him, we have the same dislike to the weed, in all its prepared states; like him, we have, in various parts of the world, been amongst those who have used it freely, though we cannot declare, like him, we have never tried it. A cigar we once smoked with horrid effect, to us conclusive of its poisonous qualities. That was at an early period of life; later in life, when among habitual smokers, and when to decline the pipe offered in courtesy would have been considered a breach of good manners, we have used it most temperately, and we must confess not without an agreeable soothing effect, very much needed in transacting business with an ill-disposed Turkish authority. In the interval, within the tropics, in the East, as well as the West, never smoking, we have fortunately escaped the prevailing diseases of the countries, and that when they have been rife around us, and smokers have fallen victims to yellow fever in the one, to remittent fever, dysentery, and cholera in the other. On one occasion, the abstainer was the only European in a district scourged with fever who escaped free. These personalities we mention to show that we are not one of the sworn friends of tobacco. Of its uses and abuses we shall now speak, and with the moderation and modesty becoming so important a subject, one on which there is so much difference of opinion amongst inquirers of high respectability. We shall propose a few questions on some of the more important points of inquiry, and answer them according to the best of our knowledge.

1. Has tobacco, especially smoking, the power of preventing contagion or infection, or of counteracting malaria? We think not, never, as already remarked, having witnessed the exemption of those who used it most from noxious influences of any kind, such as are productive of disease. We are more disposed to the inference, that if used in any excess it may favour the operation of those obscure causes; and for this reason, that whatever impairs the vigour of the constitution appears to predispose the system to, or render it more susceptible of, morbid action. In the 'Lancet,' under the head of "The Effects of Tobacco in Ague," it is stated by a medical officer of Crimean experience, that its use interfered with recovery from ague, and that till discontinued relapses were frequent.

2. Is the stomach capable of digesting tobacco? We think it may, after having become habituated to it; at least, in certain instances. There are direct and analogous facts in favour of this inference. The analogous facts are the most familiar ones. It is well known how the carnivorous animal can be brought gradually to subsist on vegetable food, and *vice versâ*. In the 'Lancet,' the volume already referred to, an instance is given of an individual, a man, sixty-four years of age, who "for many years had been in the constant practice of eating a quarter of a pound of the strongest negro-head tobacco every five days," and who, the contributor, Dr. David Arrott, adds, would willingly eat more could he get it free of expense. Singular as this must seem, we can adduce an instance even more remarkable. When at Constantinople, some twenty years ago, we were told of a man, a Turk, employed in the arsenal, who was in the habit of taking daily, and had been for fifteen years, two drachms of opium, mixed with one of corrosive sublimate, in the form of pills, which he swallowed at intervals in the course of the day. His health had suffered and his faculties had become impaired, but not to such a degree as to prevent his acting as janitor to the naval school.* In these two cases is it not reasonable to suppose that both the tobacco and the opium were digested, and became subservient, like food,

* Doubting that what was called corrosive sublimate was really such, the officer (an Englishman) superintending the arsenal procured for us a portion from the individual of the substance in question; we found it to be veritable corrosive sublimate, and nearly pure. From a physician long resident in Pera we learnt that the Turks often take corrosive sublimate with opium, under the persuasion that it aids the effects of the latter, and often in large doses.

to supply waste of material? And is it not reasonable to infer that in cases of privation of food, when the sense of hunger is appeased by the use of tobacco, it may act in the same way? The goat which will eat hemlock with impunity, we are assured, has been known to have had a fondness for tobacco. The leaf of the laurel, the exhalation from which when bruised is fatal to insects, is the favourite food of the slug. The leaf of the yew is poisonous to cattle if ate alone, but if mixed with food, it is said to be not injurious. This we state as a current belief amongst farmers. Don, in the 'Gardener's Dictionary,' says that the flowers of *Lathyrus sativus* mixed with wheat flour in half the quantity, makes very good bread, but alone produces surprising rigidity of the limbs in those who use it for a continuance. Other examples of the like kind might be given; even the leaf of the tobacco-plant itself, we find, is attacked by the slug.

3. We know how poisonous are the effects, often fatal, of alcohol taken in excess, and how gently cheering and refreshing are the lighter alcoholic drinks, used in moderation: may not some similarity be traced between strong tobacco in its operation and alcohol, and between light wines in their action and mild tobacco, rendered more mild by admixture, as used where smoking has been longest practised? Such an analogy, we think, exists, not indeed as to the specific effects compared together, but as to the injury or benefit received. The Turk, an habitual smoker, and who, as regards health, appears to indulge in that habitual smoking with impunity, uses the mildest tobacco (*N. rustica*), and inhales its vapour (probably more that of nicotianin than of the more powerful nicotine) through water, or the long cherry-stick pipe, kept scrupulously clean. The North-American Indian, another great smoker, mixes a small portion of tobacco with a large proportion of some other plant, such as the uva ursi. The Hindoo smokes even a more compound mixture, of which tobacco constitutes but a small portion, and through water, using the hookah with its tube of many coils. How great is the contrast between these diluting methods and the use of the cigar or of the short black pipe, saturated, or nearly so, with the empyreumatic oil derived from strong and unmixed tobacco! When we reflect on the physiological effects of the alkaloid, can we be surprised that the latter, if used in excess, should be injurious and productive of all the ailments that the poison is capable of effecting?

4. What are these effects? Can we with any confidence describe them? We cannot say that we can with confidence, because they seem to vary in different individuals, according to idiosyncrasy and other conditions, such as modes of living, exercise in the open air, &c., conditions difficult to appreciate. The worst and most common effects, especially in the case of the studious and the young and the in-door tradesman, from excess, appear to be impaired appetite, a relaxed muscular fibre, a pallid complexion, diminished energy. The mere circumstance that those in training at Oxford and Cambridge for the athletic boat-race, needing as much vigour as skill for its success, are prohibited the use of the pipe, is a sufficient proof that it is believed

there to have an enervating, relaxing effect. Whether it actually produces positive disease, such as

> "Coughs, asthmas, apoplexies, fevers, rhume,
> All that kill dead, or lingeringly consume,"

we are nowise prepared to say, nor to express an opinion as to the question whether, since tobacco has come into such general use, the term of man's life has been shortened or lengthened. The fact that our population is increasing, and the belief that the value of life measured by length of years is increased, are circumstances which would induce one to eliminate tobacco from the problem. Man is a hardy animal, capable of enduring much and of adapting himself wonderfully to circumstances. By special pleading it would be easy to prove, making a selection of cases, that tobacco is capable of producing paralysis, or any other disease that flesh is heir to; and by special pleading and a selection of cases, it would be easy to prove the contrary, that it is a preservative of health and a promoter of old age. Hobbes, that masculine thinker, the greatest of smokers, lived in the full possession of his faculties beyond the ordinary term of human existence. He was temperate in other things. How many are the profligate smokers—the profligate in other things—who have died prematurely!

5. Does it conduce to mental disease, to insanity? We think not; we consider the charge brought against it on this account as unfounded, and it has been repelled by physicians of ample experience in the treatment of lunatics. We may state in confirmation that amongst the Turks, who by their religion are bound to abstain from intoxicating drinks, but not latterly from tobacco, there is a remarkable exemption from insanity. We are inclined even to think that the use of the pipe, as used by them, by its quieting influence, may be one of the causes of the exemption. Also we are inclined to believe that were the common sailor and common soldier deprived of its use altogether, we should have more instances of suicide in the army and navy,—an act this commonly characteristic of a diseased mind, and commonly occurring in about the same proportion; so that if insanity is unduly high in the census of any people, we shall find suicide also to be so, and *vice versâ*. It has been asserted that the Turk has become degenerate, and that the degeneracy of the existing race is owing to the inordinate use of tobacco. We cannot adopt the assertion. Comparing his past history with his present, we believe the change from strength to weakness, not owing to the people, but to the government; the great majority of the people, not the few wealthy, still leading a simple, frugal, temperate, monogamous life. Happy, we think, it would be for us, and vastly to the diminution of crime, poverty, and insanity, were we to follow their example in one or two things, especially the use of coffee; making it a substitute for ardent spirits, the abuse in which, amongst other evil effects, appears to be so productive of the last-mentioned malady.

6. Has tobacco any marked influence on the generative organs? We doubt very much that it has any such influence, excepting perhaps

when used in great excess, and then, it may be, by impairing the general health and bodily vigour. These organs, the generative, being so very peculiar, all *à priori* reasoning on their functions is little to be relied on, as is also all analogical reasoning. So much are we convinced of this, that we have thought it right to qualify the preceding inference with a *perhaps;* and are we not justified in such caution by the well-known fact that one of the most debilitating and fatal diseases, pulmonary consumption, is no check on fertility. Other debilitating diseases might be mentioned, which, as well as we can judge, are equally innocuous in relation to the procreative power. Were this the proper place, we could relate some remarkable instances of the possession of the faculty in persons labouring under maladies eventually proving fatal, and by whom it was exercised, as indicated by the result, with unimpaired effect. Had tobacco a marked influence on the organs in question, ought it not to be demonstrated with some degree of certainty in a diminishing population in those countries where the weed is most used, such as the United States and Canada, and the North of Europe generally? But in these countries, on the contrary, the ratio of increase of the people is remarkably high. Further, had the weed such an injurious influence, ought not the proportional inaptitude for generation in the two sexes to be reversed? ought it not to be witnessed (which it is not) more frequently in the male, so much more addicted to smoking, than in the female, so little addicted? And, we would ask, does not the same argument apply in the question of its influence as a cause of insanity? women, the non-smokers, being subject in a higher ratio than men to the mental malady.

7. Is there any material difference of effect connected with the part to which it is applied, as, for instance, from chewing and snuffing, compared with smoking tobacco? If, as there is reason to believe, the principal effect is owing to the absorption of the narcotic principle or principles into the blood-current, we can hardly suppose that the difference of effect from either can be more than in degree, except, indeed, the primary local influence;—that of snuff immediately affecting the lining membrane of the nostrils, that of the quid the lining membrane of the mouth, and if the juice be swallowed, the lining membrane of the alimentary canal more or less generally. The difference, at least, we do not consider sufficiently important to need at present much consideration, especially as snuff now is so little abused, and the quid is so little used except by those hard workers in the open air who cannot indulge in the more expensive and less convenient luxuries of the cigar and pipe. Judging *à priori*, one would infer that snuffing is the least hazardous mode of indulging in tobacco, smoking next in degree, and chewing the most perilous. The one (the first), owing to the small surface exposed to its action; the other (the last) for the opposite reason. The primary effect of both is stimulating, as indicated by an increase of the secretion of the part, that increased flow tending to check the absorption of the poison, and so far corrective and beneficial. What injury is sustained by the expenditure of saliva it is not easy to determine. We wish it could be shown to be serious,

and that the dyspeptic ailments of our energetic transatlantic brethren are owing in part to this waste of a fluid which physiology teaches us is concerned in digestion, and the ejection of which—the squirting of it from the mouth—is so offensive to delicacy and so incompatible with cleanliness. If anywhere, in the United States we should witness the bad effects of tobacco, where its consumption is so extravagant. According to official accounts furnished to Congress in 1842, it would appear that the annual consumption of tobacco in the several States then amounted to 100,000,000 lbs., giving seven pounds to each man, woman, and child of the population, at a cost to the people of between four and five millions sterling.* That smoking tobacco is not attended with the same risk as chewing, we are inclined to infer rather from *à priori* consideration than from actual experience; the consideration that nicotine is very inflammable, and must be consumed in great part in the act of thus using the weed, giving rise to new products, the kinds and qualities of which in great measure remain to be ascertained; but, be they what they may, with the exception of hydrocyanic acid, said to be one of these in extremely minute proportion, we may be pretty sure that they are greatly less active than nicotine.

8. What are its good effects when used in moderation? And if any, are we more certain of these than of its injurious, when used in excess? In a qualified manner we are disposed to answer both these questions in the affirmative. That the smoking of tobacco has a soothing, tranquillizing influence on those accustomed to it, is universally admitted, especially of its milder kinds, *N. persica* and *rustica*, and this we hold to be its happiest effect, and that which gives it its greatest charm, and makes it so fascinating. It was this, we cannot doubt, that recommended it to Milton, who, we are told, before retiring to rest indulged in one pipe and a glass of fair water after a light supper. If Newton smoked, which is a common belief, we may be sure that he experienced the same benefit from it. We can imagine him quietly inhaling the gentle anodyne in tranquil meditative repose in his orchard at Woolsthorpe, when the first idea of gravitation flashed across his mind. How many are the great and worthy men who might be named who, alike thus tempted, alike indulged in moderation in the luxury. It is not improbable that Lord Bacon was one of these, for he speaks of tobacco as from his own experience.

"Experientia testatur, usum tabaci abigere lassitudinem. Ratio, quia refocillat corroboratque spiritus, partes contusas aut compressas aperiat, et præcipue quia opiatæ virtutis beneficio spiritus reficit, atque sic lassitudinem aufert; ut in somno quoque evenire videmus."†

In 'The Contemplative Man's Recreation' we find Isaac Walton and his friend Charles Cotton, after a day's pleasant angling in Dove Dale, ending it with a draught of ale and a single pipe. In a note to the same book, we learn how a martyr — Kemble by name — for conscience' sake, in the time of Queen Mary, went to execution, com-

* Were the price and the duty there the same as in England, the expenditure for this one article would amount to 896,000,000*l.*!

† Sylv. Sylvar. Cent. viii.

posedly smoking his pipe, amidst a crowd of weeping friends and neighbours; an incident which, in the county where it occurred, gave rise to the last pipe smoked being called a Kemble pipe. To the hard-working navvie, to the sailor and soldier on service, tobacco is undoubtedly a comfort, and in moderation we think may conduce to health. To them, especially amidst privations, it appears to be a comfort in a high degree. It is related by the master of a French whaler, who in the South Sea had the happiness of rescuing eight English sailors who had been shipwrecked on a desert island, and there for eighteen months had lived on penguins and such crustacea as they could pick up, that, taken on board, when they seemed beside themselves for joy at their delivery, and when whatever was on board was tendered them, they did not ask, we are assured, for bread or for spirits, but for tobacco, saying had they but had tobacco in their almost hopeless position, they could have patiently borne their lot—it would have soothed them in their misery. Besides a soothing influence, it seems to have on some persons other influences, such as the promoting of regularity of bowels, and in some cases rendering breathing more easy, and in some conducing to sleep, especially after unwonted mental excitement or exertion before going to rest, that excitement or exertion hindering sleep.

Yet, after all that can be said in its favour, were the balance to be struck of the good and evil resulting from the use of tobacco, we apprehend the result would be against it, and that to a no inconsiderable amount, even when doing no positive harm to the constitution, taking into account the time idly wasted, the money uselessly expended in the indulgence. The time lost in taking snuff we have seen recently calculated—it was no inconsiderable fractional portion of life; that lost in smoking must be greater.

> "The pipe with solemn interposing puff
> Makes half a sentence at a time enough;
> The dozing sages drop the drowsy strain,
> Then pause and puff, and speak, and pause again."

Thus sang the moralist in his poem 'Conversation,' when he satirizes the

> "Pernicious weed! whose scent the fair annoys,
> Unfriendly to society's chief joys."

The vast expenditure—and may we not say the waste of money?—on the luxury, may be imagined from the revenue accruing from the duty on tobacco, amounting in the United Kingdom to more than five millions sterling.* In Europe generally the expenditure is equally remarkable, and we believe increasing. In France, since 1815, the consumption of tobacco (a Government monopoly) has more than tripled. The gross receipts from it are set down in the Budget for the present year at 183,000,000 francs, or 7,625,000*l.* Whether the Government of that country has taken alarm out of regard for the health of the people, and on that account, as reported, raised the duty,

* In 1858 the duty on tobacco and snuff was 5,272,471*l.*; the expenditure on both annually about 8,000,000*l.*

much, as we learn, to the dissatisfaction of the said people, we cannot pretend to say. Whatever the motive may be, we see with satisfaction that the Minister of Public Instruction has further issued a circular, addressed to the directors of colleges and schools throughout the empire, forbidding the use of tobacco and cigars to students, on the asserted ground "that the physical as well as the intellectual development of many youths has been checked by the immoderate use of tobacco." Whether this be proved or not (and we are not satisfied that it has been fully proved), we hope, on many accounts, that the like prohibition will be enforced in our great public schools, where we fear there is too much addiction to the weed, without a shadow of apology, such as the hard-working navvie might urge, or the soldier or sailor amidst their privations, or the martyr on his way to execution. Temperance in youth is the best security for a vigorous manhood. Bad, indeed, and harassing must be the discipline of the school, or low and poor the disposition of the boy who feels the want of any drug, such as opium or tobacco, or of any stimulus, such as spirits or wine. Sure we are that there will be no hankering after these, if "the diet be plain, healthful, and moderate," and the plan of youthful training be even in degree in accordance with the noble idea of education inculcated by the writer* whose words we have just quoted—that "generous education which fits a man to perform justly, skilfully, and magnanimously all the offices, both private and public, of peace and war."

To conclude: reflecting on its history and the kind of infatuation which it discloses, how its use has gradually spread through all countries and amongst all classes of society, and knowing that whatever has a like influence, whether it be wine, spirits, opium, tea, coffee, haschich, or banng, these the favourites of millions, and altogether regardless of consequences, we can hardly expect that any statement of the physician and man of science respecting the evil effects of tobacco on the health will put a stop to its use, or will materially diminish its estimation with the thoughtless world; we fear all expostulations against it will be met in the spirit of the burlesque lines—

> "Quoi qu'en disce Aristotle et sa docte cabale,
> La tabac est devin et n'a rien qui l'égale."

Therefore, should not the main object of the profession be not to attack it indiscriminately, but fairly and with judgment, so as to endeavour to check at least the evils of its abuse, pointing out at the same time the cases in which it might be used in moderation with a prospect of benefit, and how in some cases—these not infrequent—even when used in moderation continuously, it may do mischief? It is an easy matter to propound cases of each kind—how, for instance, a single pipe of the mildest tobacco might soothe an irritable temper and promote domestic happiness—how, when there is a delicate stomach, even a single pipe might aggravate dyspepsia. Had John Hunter inhaled the tranquillizing vapour just before his last visit to St. George's

* Milton, in his 'Tractate of Education.'

Hospital, he might have escaped that spasm of the heart in discussion in the board-room which so instantaneously proved fatal. Were there no smoking-room attached to the House of Commons, angry as the debates sometimes are, would they not be more angry? A return of the smokers amongst the honourable members, and of the non-smokers, would be an interesting document, and we hope to see it moved for by some philanthropist.

On the works on tobacco placed as a heading to what we have written, we must be very brief in our comments. The ample title of the one which takes the lead denotes well its contents. Should it fall in the way of any of our readers idly disposed, it may amuse them for the while. It is full in its details respecting all things concerning the plant, excepting that which is most important and most difficult to estimate—viz., its effects; it is especially rich in anecdote and the literature of the subject, both in verse and prose. The following lines, which we extract as a specimen, so sensuous and sentimental, betoken, some will think, their inspiration:

> "Tobacco, charmer of my mind,
> When, like the meteor's transient gleam,
> Thy substance gone to air I find,
> I think, alas, my life's the same!
>
> What else but lighted dust am I?
> Thou show'st me what my fate will be;
> And when thy sinking ashes die,
> I learn that I must end like thee."

The next in succession, the letter of Sir Benjamin Brodie, is, as might be expected, of a very different character, relating mainly to the effects of tobacco. We have designated it a denunciation against the use of the weed; such it is, not, indeed, in the style and terms of the counterblast of King James, or in the not less unmeasured abuse bestowed on it by a poet of a later reign,* but in a qualified manner, directed chiefly against its habitual use and in excess. He says, and we hold the words of so high an authority to deserve quoting—

"I am not prepared to subscribe to the opinion of those who hold that under all circumstances, and to however moderate an extent it be practised, the smoking of tobacco is prejudicial. The first effect of it is to soothe and tranquillize the nervous system. It allays the pain of hunger, and relieves the

* Charles Cotton: his poem on Tobacco† is a capital example of the "infected muse," and, if not written in irony, we cannot but think it was written after suffering from the effects of the first pipe he smoked. The following few lines are mild in comparison with the others:

> "Pernicious *weed* (should not my muse offend,
> To say Heav'n made aught for a cruel end),
> I should proclaim that thou created wert,
> To ruin man's high and immortal part.
> Thy stygyan damp obscures our reason's eye,
> Debauches wit, and makes invention dry;
> Destroys the memory, confounds our care;
> We know not what we do, or what we are:
> Renders our faculties and members lame
> To every office of our country's claim.

† Poems on Several Occasions, written by Charles Cotton, Esq. London, 1689.

uneasy feelings produced by mental and bodily exhaustion. To the soldier who has passed the night in the trenches before a beleaguered town, with only a distant prospect of breakfast when the morning has arrived; to the sailor, contending with the elements in a storm; to the labourer, after a hard day's work; to the traveller in an uncultivated region, with an insufficient supply of food, the use of the cigar or a tobacco-pipe may be not only a grateful indulgence, but really beneficial."

His remarks on habitual, on constant smoking, that which he deprecates, we shall not comment on. In many particulars we can give them our assent, but not in all, remembering the motto of that learned society, "nullius in verba," over which he so ably presides. As a judicious warning against an increasing evil, we hope his letter will continue to have a wide circulation.

Regarding the other publications prefixed, so many and so different, we hardly know what to say. We find in them much repetition, much assertion, scanty facts well authenticated, extremely few new facts. They display, we cannot but think, more of special pleading than of fair inductive reasoning. Their authors too often seem drawn to their conclusions under the influence rather of a refined sentiment than of a severe logic. We have recommended one work as likely to afford amusement in an idle hour; most of the others, should they fall in the way of our readers, may perhaps be looked into with some profit, as showing, where there is a want of exact knowledge, how there may be the greatest discord of opinion, and how extreme views are apt to be taken and supported with all the confidence belonging to unlimited faith. We are glad to think, however, that great as is the disagreement of the contending writers, at least on two points they are of one mind—viz., that in excess tobacco is hurtful, and that the young ought to be prohibited its use, as a bad habit, whether it stunts growth or not.

One of these publications requires particular mention; it is the last on our list, and which we did not see till we had nearly finished our task. The author of 'Death in the Pipe, or the Great Smoking Question,' has treated the subject, as we think, logically and fairly, and has discoursed on it even eloquently. His pamphlet, written chiefly in refutation of the statements and assertions of a favourite author of the agitators against the use of tobacco, is not undeserving of the attention of the members of the Anti-Tobacco Society, and of the readers of the Anti-Tobacco Journal. Of that society, one germane to the Total Abstinence Society, we would not wish to write disparagingly, the intentions of both being good; at the same time we cannot express approval of them or of their philippics, showing too often a forgetfulness of the old adage, "that you diminish all that you exaggerate." We are the advocates for the uses of things, for temperance in all things, in meats as well as in drinks. Even the virtues it was thought of old, in excess, might acquire a tincture of vice. Pledges, whether made at the hustings or at table, are too often a snare; they are in the same danger of being broken by the honourable member and the humble artificer, and are hardly worthy of the free man. For much the same reason we should have no satisfaction in learning that the society in question, agitating against tobacco, were successful in raising the already high duty on this substance, or in obtaining an Act prohibiting even "juvenile street-smoking." If we read history aright, it is not by laws that manners can be improved, nor by high duties that excesses can be prevented.

REVIEW XI.

The Principles and Practice of Surgery. By WILLIAM PIRRIE, F.R.S.E., Professor of Surgery in the University of Aberdeen, Surgeon to the Royal Infirmary, &c. &c. Second Edition. Illustrated by numerous engravings on wood.—*London*, 1860. 8vo, pp. 878.

THIS book is very carefully written, and deserves to be spoken of with the highest respect. Professor Pirrie has published it chiefly as a compendium of his lectures, and his objects are stated to have been "to combine simplicity of arrangement, and conciseness and clearness of description, with the elucidation of sound principles and practice, as well as to give a faithful account of the present state of surgical opinion on the various subjects treated in the work." The present is the second edition of the book, and the opportunity has been taken advantage of to enlarge and considerably to improve it. It is impossible to deny that everything aimed at has been fairly and well accomplished, and we cannot hesitate to state our belief that as a compendium of surgery, a sort of enlarged 'Druitt's Vade Mecum,' it will be difficult to surpass this work. It gives a clear and concise account of nearly every important improvement that has been made in surgery down to the present moment. The laryngoscope, iridectomy, Wood's operation for the radical cure of hernia, Allarton's operation for stone, and many other novelties, all find their appropriate places, and the author, moreover, has a hearty and generous mode of speaking of his cotemporaries that is peculiarly attractive, and it must be confessed by no means common in the case of professional writings proceeding from the land of cakes. The work is distinguished by sound practical judgment as well as by competent knowledge, and we think that the Aberdeen Professor shows that he is not only alive to everything that is going on, but also that he is gifted with the art of condensation, exercised with more than ordinary critical acuteness.

Nevertheless we must acknowledge that Professor Pirrie's 'Principles and Practice of Surgery' does not supply that which we consider to be one of the great wants of the day—namely, a thoroughly readable course of surgical lectures.

All the works of which it is not unfair to say that Dr. Druitt's well-known manual may be taken as the type, are rather adapted to the requirements of advanced students, especially when preparing for their examinations, or to the wants of the busy practitioner who refers to them with the view of keeping "*en courant*" with his profession, than to the necessities of a surgical tyro, whose mind has to be educated as well as informed. Mr. Lawrence's masterly lectures, which, however, have never been separately published, and are to be met with only in the pages of the 'Lancet,' or of the 'Medical Gazette' of former days, supplies perhaps the only series of surgical lectures at all worthy to take their place beside the classic volumes of Dr. Watson on the kindred subject of physic. But Mr. Lawrence's lectures were written more than thirty years ago, since which period surgery has undergone a complete revolution, and therefore, however excellent in their day, they can hardly be considered to represent the author's principles or practice at the present moment. Still they are models of style; large in view, flowing in diction, philosophical in their conception, they keep the student's mind in a state of pleasing expectation, and assist to form his taste at the same time that they instruct and elevate his judgment. Can this be affirmed of many of that prolific series of surgical works which of late years has emanated from the professional press? Is the student to look for nothing higher than a manual whose every page suggests that it is to be used for the purposes of grinding for examination? or, on the other hand, is he entitled to expect that he shall be saved all the trouble of thinking, and required to exercise little more than his memory? Does not this system of instruction tend to degrade a science into a mere art? Is it not the efficient cause of much of that routine practice under which so many of the Queen's subjects are fated still to suffer? Do not many practitioners use these manuals much as our mothers used to employ Buchan's Domestic Medicine, and do they not care but little for any work whose chief object is the establishment of great principles, albeit of universal application? This subject deserves and requires careful consideration, involving as it does the question of the methods of teaching surgery which are now in vogue. These may be divided into the theoretical and the practical departments. To the former belongs the two courses of systematic surgical lectures, rendered compulsory by the regulations of the College of Surgeons, and the usual reading and grinding. In the latter are included hospital attendance, dressing, and clinical lectures.

Now, it requires no demonstration to show that the efficiency of the future practitioner, at the outset of his career, will mainly depend upon the use he has made of the practical element in his education. Much, very much, may be learned even in the course of the three short years devoted to hospital practice, if only opportunities are diligently used; and consequently it is universally observed that the student who has been an habitual attendant in the wards, starts in life with an amount of well grounded confidence and self-reliance, of which the student who has merely diligently studied his manual and faithfully attended and recorded his notes of every lecture, is too often wholly destitute. We put the question broadly—Which is the more desirable attendant, the man, for example, who has taken charge of cases of stricture in the out-patient department of his hospital, and fairly watched and handled them to their termination, or he who, knowing by heart all that his manual and his lecture-notes tell him on the subject, has, notwithstanding, never honestly watched a single case from its commencement to its termination?

Which man's knowledge is worth having? that of the prize student who glibly describes the theoretical nature and most approved methods of treating fractures in all their varieties, Bayntor's plan of strapping ulcers, Scott's plan of treating joints, and a thousand other such matters; or that of the man who has actually performed all these operations over and over again, and who goes to his work with ready disciplined hands and eyes? Yet we appeal to the experience of every hospital surgeon, whether for one of the latter class of students he will not find ten of the former. The regulations of our examining bodies actually discourage practical study by the undue importance which they attach to theoretical professional teaching. Their surgical examinations, for the ordinary licence, are entirely theoretical, just as until very recently was the case with the anatomical examinations also. Their estimation of a man's proficiency is decided by the regularity of his attendance on lectures, not on the progress he may happen to have made in his studies. A student's hospital note-book of cases observed and recorded by himself is useless to him at the College of Surgeons. He has no opportunity of displaying it; it counts for nothing in his favour; it will atone for no slip of memory in his enumeration of the varieties of skin diseases, of tumours, of cancers. Hospital attendance is indeed compulsory; but does hospital attendance imply anything more than that a man has duly paid his fees, and shown himself sufficiently often in the wards to familiarize the surgeon with his features, and thus furnish him with an excuse for signing his certificate, which both parties are too apt to regard as a mere form. All who are familiar with hospital arrangements know full well how the great body of students gather round the bed of a patient whose case is considered as peculiarly exciting and interesting, and having satisfied their curiosity, care nothing at all about its further progress or its termination. We are convinced that it is comparatively rare for a student with honest labour to watch even a single severe case through from first to last.

Then, again, so far as the College of Surgeons is concerned, a student need never have performed the practical duties of the office of dresser at all. In some hospitals high fees are required for the privilege of dressing, and good men are excluded owing to the shallowness of their purses; in other hospitals the dresserships are made the subjects of competition, manifestly, however, a great improvement on the first system; but in very few hospitals indeed, are all students without exception required to take these duties upon them, and systematically to discharge them. Yet it is surely the bounden duty of our surgical authorities to take care that their licence is not conferred on any one who has not given proof that he possesses a fair amount of practical skill as well as of book knowledge. The consequence of all this seems to us to be that an attempt is made to teach in surgical lectures, that which should rather be learned by attendance in the wards and by personal manipulations. Thus surgical lectures lose that elevated and philosophical character which they ought to possess, and assume more or less the characters of a handbook.

The inefficiency of the existing regulations as regards lectures and hospital study cannot be more convincingly stated than they were in a memorial which, several years ago, was addressed to the College of Surgeons by the teachers at one of the metropolitan schools of medicine, and from which we make the following extracts:

"Under the present system students are required to attend lectures and hospital practice at a recognised school for a given time. So far as concerns their education at the school, the certificate of attendance is all that is needed to qualify them to present themselves for examination before your Court. The certificate is to the effect that the student has attended a certain course of lectures, or a certain amount of hospital practice. If he have complied with the requirements of the school, and have been present with sufficient regularity in the class-room, or in the wards, the certificate, referring as it does simply to attendance, must be granted to him. He has established a claim to it which his teacher has no right to resist. It is true that the lecturer may require attendance at the class examination. But though the student exhibit at them a complete ignorance of the subjects which have been taught, he is not thereby the less entitled to the certificate of attendance. In short, he is not required in any way to give evidence of having studied the subjects on which he is compelled to attend lectures. With regard to the most important branch of medical education, clinical study, it is notorious that the large majority of pupils, although they may walk through the wards, never attempt to make any use of the opportunities presented to them; and yet, according to the present regulations, they are entitled to their certificates of attendance on hospital practice.

"It is a fact well known to your memorialists, and equally so, they believe, to the lecturers of all the medical schools in the country, that the examination for the diploma of your college is no criterion of the diligence or idleness of a student during his attendance at his school. The pupil who has been thoroughly idle, and whose time for two years has been spent in dissipation, is yet frequently enabled, by means of a six months' working with a 'grinder,' during which time he sees neither a subject nor a surgical case, to pass his examination and obtain his licence to practise. The pupil, on his first coming to London, soon learns this, and although he may have begun his studies with the best intentions, which the necessity for steady and continuous work would have fostered, he is often led by his knowledge of the absence of that necessity to relax in, and eventually to abandon study altogether. The confessions of the students themselves, and their regrets that they were not from the first obliged to work for their certificates, have satisfied your memorialists that this view of the case, which in the present state of things is natural, is also practically true."

After referring to other evils attending the college system, the memorial proceeds thus:

"It appears to your memorialists that a reasonable and easy remedy might be found in a slight alteration in the certificates, which would give to the lecturers a power they ought to possess, if entrusted at all with medical education. A statement might be added to the certificate of mere attendance on lectures and hospital practice to the effect that the student has really and *bonâ fide*, to the satisfaction of his teachers, profited by the instruction furnished to him. Were such authority conceded, each school or each lecturer might determine the manner in which the students' knowledge should be tested. The more stringent the regulations (within due limits), the better for the students and for the character of the school. The great advantages which appear to your memorialists likely to result from such a plan, are that each student would be forced to work year by year, or period by period; that on subjects in which he is required to attend more than one course of lectures, he might be obliged so to work as to ensure regular and progressive improvement; and that in the wards he might be obliged to observe for himself, so as to be able to pass a clinical examination. An opportunity would also be given for altering the existing prize system, which is avowedly open to many and great objections. Instead of the present very limited competition for prizes, the teachers would have power to institute a comparison of the relative merits of all the students in the school. The plan of frequent compulsory and practical examination, it is true, is not adopted in the case of students of other professions. It may be remarked, however, that many of the colleges in the Universities of Oxford and Cambridge, finding that the University examinations are insufficient to ensure regular study, even under the system of discipline and tutorial instruction which characterize an university education, now insist on sessional or annual examination. And they deem that such a test of real solid acquirement may be more readily dispensed with in other professions, since the fitness of those engaged in them is more open to general criticism, and ignorance is more readily detected and exposed."

Our readers will probably join in our regret that these admirable suggestions have met with but little response from the examining body to which they were addressed. No doubt frequent compulsory examinations throw great additional labour upon the teacher, and it is very probable that such tests would be enforced with much less strictness in some schools than in others; but this is an evil which in the long run would cure itself. That school which is really conscientiously conducted, and in which pains are taken to ensure the lasting progress of the student, will certainly secure the higher class of men, and reap its own reward; nor must it be forgotten that however laxly these examinations might in some cases be conducted, they would still constitute an improvement on the present system, which enforces no such examination at all. During the past year the College of Surgeons has so far improved upon its old ways, as to have discarded the ridiculous plan of attempting to ascertain whether a student is qualified to practise surgery, by testing his knowledge in a *vivâ voce* examination of one hour's duration, divided between the well-remembered four tables. The division of the examination into two portions—an anatomical and a surgical examination—to be undergone at considerable intervals of time from each other, and the adoption of written as well *vivâ voce* examinations, are matters of great moment and importance, but they do not go to the root of the evil; nay, in some respects they are even attended with injurious consequences. It has been noticed that the student's attendance in the wards falls off very considerably for some months before he has to present himself at the College, owing to his being engaged in grinding for his anatomical or surgical examination; and if the habit of clinical study has once been broken, it is with great difficulty renewed. This evil, however, will probably correct itself next year, when the four years' course of study comes into operation. No plan of surgical education can, in our opinion, prove satisfactory which does not provide both for an extended system of clinical instruction and examination as well as for theoretical teaching. Our own persuasion is that the kindred sciences of surgery and anatomy should equally be taught chiefly by practical demonstrations, and that these should be supplemented towards the termination of the student's career by a single course of theoretical lectures of a higher and more philosophical character than those which, if we may judge from the treatises which every now and then come before us, are commonly delivered in our schools. The study of such a work as Dr. Williams's 'Principles of Medicine,' and of such principles as applied to surgery, would prove of incalculable advantage to the student at the close of his career; and now that the period of medical education is about to be lengthened, we can see no sufficient reason why it should not be enforced.

We have made the foregoing remarks because they seem to us germane to the matter, but we have no desire to cast any imputation on Mr. Pirrie's work; indeed, taking it for what it professes to be, we have already expressed our high appreciation of its merits. It would be unfair to estimate it according to an ideal standard which it does not seek to attain; and we think it eminently creditable to the surgical teaching of the University of Aberdeen. Works of this class do not admit of anything like a systematic review, but we have marked a few passages for quotation and reference. As a general rule the illustrations are remarkably graphic and clear, but from this commendation we must except the attempt at p. 30 to represent a weak ulcer by means of a woodcut. We have seldom seen anything more ridiculous; and as the task, without the aid of a coloured drawing, is probably an impossible one, we think it would be better to omit both this and the similar illustrations of other varieties of ulcers in any future edition. In his observations on the treatment of chronic abscesses, the author advocates the plan of opening them by valvular incision, but he gives a very proper caution as to the danger of interfering with them when they are connected with incurable disease of the bones or joints. Such abscesses, however, may, according to our experience, especially when connected with caries of the vertebræ, in many instances be safely and usefully evacuated by the employment of drainage tubes. The chief danger to be apprehended in these cases arises from the liability of the impacted pus to become putrid, but drainage here, like drainage of the soil, keeps the parts thoroughly dry, and thus obviates a great objection to what would otherwise be a desirable operation.

Drainage, however, does not yet appear to be sufficiently employed in British surgery, but an extended experience enables us to pronounce it one of the most important improvements in practical surgery that of late has emanated even from the French school.

In the treatment of erysipelas, Mr. Pirrie has made trial of the muriated tincture of iron, and he speaks highly of its value. Curiously enough, he appears to have found it most signally useful in erysipelas of the head and trunk, which are precisely the classes of cases in which some physicians profess to have been disappointed in its effects. The fact however is, that our medical colleagues seldom administer it in sufficiently large doses. Its specific mode of action is not well understood, but, according to our belief it becomes effective by arresting the catalysis of the blood; and it might probably be advantageously superseded by the perchloride of iron, one of the most powerful disinfectants known.

In the following extract we learn the author's views respecting the prognosis and treatment of several varieties of chronic diseases of the joints, and we can only say that we fully endorse all that is here stated:

"I have been at great pains, and have spent a considerable sum of money, for the purpose of arriving at a correct conclusion as to the curability of this disease of the joints (scrofulous chronic synovitis), and the firm belief at which I have arrived is, that in the event of the disease not being so far advanced as to have given rise to disorganization of the joint, almost every case is perfectly curable, on the expenditure of a sufficient amount of money in procuring the use of proper remedies. I have taken a great interest in this class of cases for many years, and have been long in the habit of treating some at my own expense among the poor, and have not a stronger belief in almost any surgical subject than in this, that unless total or very extensive disorganization of the joint has taken place, most cases, however unpromising, are curable. The treatment from the use of which I have seen such gratifying results consists in residence in the country, in a dry, bracing atmosphere, with much exposure in the open air to the light of the sun; in maintaining the atmosphere in the patient's room perfectly pure by day and by night; in sleeping in a room in which there is free admission of the light of the sun during a reasonable portion of each day; in guarding against anything which could cause any impurity of the atmosphere; in sleeping in a bed so arranged as to allow free circulation of air around the patient, and as much exposure to the light as possible; in the daily use of animal food, cream, and cod-liver oil, along with some of the preparations of iron; in maintaining a healthy condition of the skin, by the means proper for that purpose; in watching the condition of the digestive organs, and maintaining them in a vigorous state; and, if necessary, giving some alkaline preparation to correct the acidity of stomach, which is so injurious in the scrofulous diathesis, and so certain, in the way explained in the chapter on tubercle, to keep up the error in the constitution of the blood, which constitutes so great a part of the scrofulous diathesis. These means, along with cheerfulness, mental occupation, encouragement, and suitable local treatment, certainly lead in most unpromising cases, with very few exceptions, to the best results that can be desired. It is exceedingly important to encourage the patient, and to produce a firm conviction that benefits will result from treatment. I was much struck with the manner in which this was expressed to me by a patient some years ago. The patient was a female, who had a bad knee; she had made up her mind to allow it to be removed, and as it was a very unpromising case I thought it an excellent one for testing the usefulness of the above-mentioned treatment. I got the treatment instituted to the utmost extent I could desire; the knee got perfectly well, and when the woman, who I have no doubt was a sincere Christian, called on me to thank me for the great interest I had taken in her case, she remarked:—'I find medicine, like the Gospel, must be received in perfect faith to get the full benefit of it; I had such comfort and pleasure in following all your directions, even at my worst, because I felt sure they would do me good, as you were always confident that I should get better.' The reason why so many poor people lose their limbs from this disease is, that their poverty renders them unable to place themselves in circumstances to obtain the remedies essential for cure. I have often thought it a matter of the deepest regret that persons who leave much for benevolent purposes, seem not aware how beneficially funds might be appropriated if left for maintaining and treating in the country, in healthy situations, hundreds of the poor of our towns, who are constantly dying or losing limbs from the effects of scrofula." (pp. 420–21.)

Professor Pirrie will be glad to know that to some extent at least in this country his wishes are already carried out. The Sea-Bathing Infirmaries at Southport, in Lancashire; at Harrowgate, in Yorkshire; and at Margate for the south-western counties, and we believe several more on a minor scale at other sea-side localities, are admirable institutions which, in the main, are devoted to the reception of cases of scrofulous disease.

Another extract, on the subject of compression in aneurism, bears testimony to the author's generous feeling towards his contemporaries, and to the pains which he takes to ensure his obtaining the best information.

"During the sixteen years 'the treatment of aneurism by compression has been put upon its trial,' I have taken the greatest interest in this subject, and in October, 1859, went to Dublin, for no other purpose than to satisfy myself, by personal observation, of the advantages of 'the bloodless cure of aneurism.' By the much-valued kindness of Mr. Tuffnell, I had the advantage of learning, and of seeing, all I possibly could desire to know and see regarding this mode of treatment. Mr. Tuffnell showed me cases under treatment in hospital; made a journey of nearly one hundred miles into the country to show me a large popliteal aneurism the consolidation of which he had just effected by means of pressure; and demonstrated in the museums the extremely instructive preparations of vessels taken from the bodies of those who had died of other diseases after having been cured of aneurism—preparations of which I had previously read, and which it was extremely interesting to me to examine, and to hear the histories from Mr. Tuffnell. The result to my mind was, the firm belief that the treatment by compression is a safe, simple, successful, and almost painless mode of curing aneurism. The surgeons of Dublin can have no possible motive for preferring compression to operation, except the conviction that it is the preferable mode of treatment. In operative surgery 'whatever men dare, they can do' as well as hands can do; but in suitable cases they prefer compression to operation, because they believe it the safer mode of treatment. It would be presumptuous in me to praise men so distinguished; but I may be permitted to say, that my instructive and delightful visit filled my mind with the highest admiration of the talents and skill of the surgeons of Dublin, and gave me a strong impression of the advanced state both of the science and art of surgery in that city.' (pp. 529–30.)

We trust that these remarks will not be thrown away on other persons in Scotland and elsewhere, who decline to give up the practice of performing cutting operations for the cure of aneurism, simply because they imagine that they are more skilful than their neighbours. With one more extract from Professor Pirrie's work, we must conclude this notice; it is on that common affection, "ingrowing of the nail."

"Persons who confine their feet in tightly-made boots, frequently suffer from an ingrowing of the nail of the great toe. As the soft parts on each side are, from confinement, kept constantly pressed against the sharp edges of the nail, the result is, that the nail penetrates the skin, a crop of flabby granulations springs up, from which a certain amount of fœtid pus is discharged, and the party affected speedily loses the comfort and use of his foot.

"The proper remedy for this painful affection is removal of the nail, and afterwards treating the ulcer according to the usual rules of practice. To remove the nail, the surgeon firmly grasps the toe with his left hand, passes one blade of a strong-pointed pair of scissors beneath the nail up to its roots, then cuts it through its entire length, and twists off first one half and then the other." (p. 815.)

We have much satisfaction in stating our firm persuasion that this barbarous proceeding, the very recital of which makes the blood run cold, and which before the introduction of anæsthetics was perhaps the most painful operation in surgery, is wholly unnecessary, and can always be avoided if proper care and patience be bestowed on the treatment of the case by the surgeon. The chief cause of the ingrowing toe-nail is not primarily from wearing tight boots, but from cutting the nails down at the edges, or from what is very common in the young, tearing them. The torn nail does not grow so fast as the thickened integument at the sides of the digits, and consequently instead of the nail reposing on and covering the edges of the toe, it grows into the obstructing integument, and speedily establishes a little raw ulcerating surface, from which exquisitely tender granulations spring up.

The remedy is to be found in insinuating something between the nail and the painful flesh, such as the most trifling shred of cotton wool, or a bit of paper or card, which protects the surface until the nail has attained its proper dimensions and position. In commencing this treatment the surgeon must exercise the greatest gentleness and patience, and at first be content with doing very little, but the moment the nail has grown long enough at the edge to permit of its retaining the protecting body underneath it, the cure of the case is placed in our hands. The nature of the affection should be explained to the patient, and when the nail has recovered its proper length, he should be instructed always to cut it perfectly square, and on no account to remove the edges of the nail, as is commonly done. The operation of tearing out the offending nail will be banished from the mind of every one who patiently and intelligently follows out these directions, which we have practised in a vast number of instances.

Review XII.

1. *Observations on the Limbs of Vertebrate Animals: the Plan of their Construction, their Homology, and the Comparison of the Fore and Hind Limbs.* By George Murray Humphry, M.D., F.R.S., Surgeon to Addenbrooke's Hospital, and Lecturer on Human Anatomy and Physiology in the Cambridge University Medical School.—*Cambridge*, 1860. Plates. 4to, pp. 40.

2. *Nouvelle Comparaison des Membres Pelviens et Thoraciques chez l'homme et chez les Mammifères, déduite de la torsion de l'Humerus.* Par Charles Martins, Professeur d'Histoire Naturelle Médicale à la Faculté de Médecine de Montpellier.—*Montpellier*, 1857. Plates. 4to, pp. 540.

New Comparison of the Pelvic and Thoracic Members of Man and Mammals, deduced from the torsion of the Humerus. By Charles Martins, &c., &c.

The first of the above essays may be regarded under two points of view: firstly, as a summary, by an able anatomist, of some of the more important details of the mechanism of the limbs; and secondly, as a contribution to controversial osteology, especially criticising some of the views elaborated by Professor Owen as to the nature of limbs.

It is perhaps to be regretted that, in the present state of anatomy, no strict separation of these two methods of dealing with such a subject is possible; to be regretted, we may indeed say, on practical grounds, for few topics in the range of comparative anatomy could be more instructive than a simple comparison of the bones of some half-dozen of the larger mammals, in their obvious relations to locomotion and muscular movement. The skeletons of a tiger, an elephant, and a horse, for example, might be made the basis of teachings of this kind; in which, without any allusions to pleurapophyses or homotypes, such real information as to the actions of muscles might be impressed upon the student as would no longer oblige him to trust to the long (and often incorrect) tables which he has to learn by rote from the end of some anatomical manual, or to accept from the oral instructions of his grinder, as the traditional standard of anatomy and physiology by which he will be measured in Lincoln's-inn or Blackfriars. If such anatomists as Humphry and Struthers—to risk no jealousy by mentioning metropolitan names—were the examiners in their own science—itself, we fear, more imperfectly taught and studied every year among the throng of subjects on which the student is so painfully be-lectured—it is probable that, in addition to the simpler and truer tests of knowledge which a master of any science always knows best how to apply, and the cessation of those Examination-room blunders which somewhat lower the scientific position of the profession in the eyes of the neophyte, we should gain something as regards the writings of anatomists themselves; who now, writing for an exceedingly small and learned group, and habitually teaching a large one, unconsciously move "as in a strange diagonal" between the two, pleasing neither, though partially instructing both.

But the increasing divergence of Biology from Physic, illustrated and even exaggerated as it undoubtedly is by the constitution of our Examining bodies, disarms criticism of one-half its functions. We may regret that an eminent provincial surgeon should have to address a Philosophical Society—"fit audience, though few"—rather than his own profession and his own pupils. But doing so, the author may claim to be judged on the ground he has chosen to take, rather than on that which his casual critic may (however rightly) think would have afforded better scope for such abilities.

Beginning with the general purpose of limbs, as developed, in the main, proportionally to the locomotive function of the animal, the author traces their division into three chief segments, the components of which progressively increase in number as we pass from the proximal (e.g., the humerus) to the middle (radius, ulna) and terminal segments; in which latter, again, the wrist and the digits form three series of three, four, and five bones successively. This rule holds good through a large proportion of animals, and is indeed confirmed by the study of those deviations from it which, as is no wonder, are sometimes detected.

The fore-limb is then considered in its several segments. The pisiform is regarded as not belonging to the corpus, but to the flexor tendon, which it certainly ministers to. The second phalanx of the thumb is identified as the missing segment of these three which are present in the other digits. And the range of pronation and supronation in different animals are also noticed:—though with a very imperfect recognition, as it seems to us, of the large share virtually taken in this act, where it reaches its climax in Man, by the flexion of the fore-arm and rotation of the humerus, which together convert an angle something less than 180° into about 330°. For such an omission, however, the brevity of this monograph is possibly an excuse.

The hind limb receives a similar treatment. The author then compares the fore and hind limbs, suggesting some points of great interest. He deduces that the two limbs are antagonistic in action as well as in construction: an antagonism which, as it is established during their development, so, examined in various stages, it permits us to recognise in the transitional positions of these limbs in the higher animals, their arrested and permanent positions in the lower. The arguments by which he develops these important propositions are couched in terse and intelligible language, which is almost a model of anatomical writing. Some remarks are added with reference to the patella, which the author regards, not as a sesamoïd bone, but as the correspondent of the olecranon.

Here, however, we may demur both to the alternative character of the conclusion and to one of the chief arguments on which it is based. The patella might, for example, correspond to the olecranon, and yet equally justify us in regarding this latter process as a fused sesamoïd. And our author himself sets the example, in the case of the pisiform bone, of doubting the conclusiveness of development from true fœtal cartilage instead of fibro-cartilage. And conversely, without precipitating a decision which will probably be soon taken as to the exact import of what is, after all, only one aspect of the growth of bone, we might fairly decline to exclude any bone from the true skeleton on grounds which (*mutatis mutandis*) would excise a great part of the skull from the whole animal series, leaving the unhappy creatures with their brains bare. The development of bones like the parietal from connective tissue, is a fact so pertinent to the question of the development of other bones from another form of this tissue, that the import of a bone could scarcely be decided upon such grounds.

The scapular and pelvic arches are next treated of; with the result that the author arrives at conclusions very different from those deduced by Owen in his well-known monograph. Of these differences, perhaps the most important are those which refer both the limbs, as well as their sustaining arches, to the vertebræ near which they are placed; and thus allot the fore-limb to the hinder cervical or thoracic vertebræ, and not to the occipital series, which, in birds, is thirty vertebræ distant. Further, the arches are regarded as hæmal, not pleural elements; and as probably formed of two mesial bones (clavicle and coracoïd, ischium and pubes), articulating with the representatives of two lateral hæmal bones fused into one, as the scapula and ilium respectively.

Such considerations naturally lead the author to criticise the generalization which Professor Owen has based on the views our author controverts. An elaborate tabulated comparison of the upper and lower limbs in the human subject, successively noticing their bones, ligaments, muscles, arteries, and nerves, concludes the Essay, which is illustrated by plates containing some sixty well executed osteological outlines, chiefly from the Cambridge Museum.

The careful, though brief summary we have just given may indicate the value of Mr. Humphry's essay, scantily as we can do justice to its details. Its drift deserves a final remark, especially in contrast with the large and attractive views it seeks to modify, or even displace.

The gist of Professor Owen's argument was—we see in the limbs of animals a tendency to uniformity, utterly unlike anything which the teleology of a human artificer can instance, and sometimes unexplained by the function of the parts themselves. Our swords, sails, hammers, spades, boats, balloons, constructed solely with reference to their use as instruments of homicide, navigation, smithery, digging, &c., respectively, are so utterly dissimilar in construction, as to show what mere teleology might do, ungoverned or unmodified by a common plan or type. Furthermore, in the instruments with which nature furnishes the Vertebrate animal, she seems to disclose the presence of a typical idea, regulating form, in some instances, apart from function; complicating, by division, production and the like, implements in which a simpler arrangement would have the same office; so that, for instance, the paddle of a Cetacean retains a kind of elaboration useless to it as a mere instrument, though effective in its congeners.

To this argument the Cambridge anatomist demurs, in words which may revoke in some minds the lingering echoes of the great Cambridge (and English) poet of our century—

> "'So careful of the type,' but, no,
> From scarped cliff and quarried stone,
> She cries, 'a thousand types are gone;
> I care for nothing, all shall go.'"

"Throughout the vertebrate series the limbs present a marked similarity in their general construction, yet are subject to varieties of form as numerous as the various modes on which their functions have to be performed. This grafting of an endless variety of detail upon one plan, in conformity with the endless variety of wants, feelings, and offices of the several animals—this combination of variety with uniformity, in which the deviations of each form from the others are no more than are absolutely necessary to attain the required end—constitutes one of the most striking features in creation. We cannot doubt that the working out of variety in detail in the several parts of an animal's frame, so as to bring each part into harmony with the others, and with the sensational and volitional qualities of the creature, is effected under the influence of some high controlling law; that the modifications of the limbs, for instance, in accordance with the peculiarities of the brain, and the attendant wants and desires of the animal, take place in obedience to some definite principle. But the nature of that relation, and the manner in which the influences of the formative processes in different parts are mutually operative, are subjects which lie too deep in the mysteries of nutrition for us even to venture, at present, to speculate upon.

"The similarity which pervades the general plan of the construction of the limbs evidently has relation to, and perhaps may be said to depend upon, a similarity in the plan of their development. The further we trace the fore and the hind limbs, and the limbs of various animals, back to their early embryonic forms, the closer do we find that the resemblance between them becomes, one peculiarity after another disappearing till the several distinctive features are lost in a oneness of simplicity; just as in tracing the different animals back along the paths of development we find them all reduced to one simple germ-form. Each germ is, of course, potentially different from the others, inasmuch as it is endued with the qualities requisite for the manifestation of its distinctive peculiarities; but they all start from one point, and the development of all proceeds in the same manner as far as is compatible with the ends to be attained; and they diverge from one another in different ways and in different degrees, to evolve the several varieties of form which are observed in the several members of the animal kingdom. Thus the cause of their similarity lies at the commencement of their career, where the different species start from a common centre of resemblance; and the several causes of dissimilarity operate with various, but on the whole with increasing, influence as the work of development proceeds.

"Thus much we can perceive; but if we attempt to speculate beyond this we incur the risk of error. We can see that nature works from a simple form, and builds upon a uniform plan, as far as possible; but there seems no good reason to assume that she works up to, or with reference to, any ideal or archetypal pattern. One cannot, therefore, but entertain a doubt whether the introduction of the notion of an archetype into the minds of anatomists, and the consequent endeavour to make out that archetype, and to trace the approximations to it and the divergences from it in the various classes of animals, do not rather tend to lead the student from the true path of nature, and fetter rather than assist him in the comprehension and investigation of her works.

"We venture upon this hint with some timidity, and not without much reflection, knowing that it is in opposition to the views of one whose labours in this field exceed those of any living anatomist, and whose authority is deserving of proportionately great weight. We feel, too, an unwillingness to cast a doubt upon a great and important generalization; for generalizations give a charm and an impetus to science, and it is vexing and disappointing to find them questioned when we have nurtured them, and enjoyed them, and looked to them as our helps to further progress. But, after all, it is by questioning and discussing, no less than by assenting, that science is to be advanced, and a theory, if true, will thereby shine forth brighter and clearer, and better fitted to light men on their way.

"This feeling renders me unwilling to assent to the expression that 'the same type has governed the formation of the two limbs;' I would rather say that they are products from one simple structure, their similarities not being the result of a subjection to one pattern, but of an emanation from one point under the influence of similar nutritive forces. So, likewise, it seems scarcely correct to speak of the 'fading away of the pattern,' unless we have stronger ground for belief in the existence of a pattern. Again, the statement that 'the archetype is progressively departed from as the organization is more and more modified in adaptation to higher and more varied powers and actions,' would seem to me more correct if the term 'simple primary form' were substituted for the word 'archetype.'"

To this it might well be answered that, difficult as many would find it to appreciate the distinction between an "archetype" and a "simple primary form," few could find in the whole of Professor Owen's views an assertion of the existence of an archetype in anything more than a subjective sense: in other words, of a plan which seems deducible from a comparison of different animals, but is not, therefore, necessarily evinced by any substantive and independent existence, any concrete and perfect living example. Doubtless the assertion of such a view involves great risk of misconstruction. But, on the whole, the language of its promulgator has been singularly little open to misinterpretation, and nowhere more accurate than where it has been most eloquent and aspiring. Indeed, not even the passages above quoted are incompatible with such an opinion. Justice obliges us to add, that the essay before us is so far contrasted with Professor Owen's as that it includes various passages in which the words "type," "homologue," "correspondence" are scarcely used with that strict and logical exactness, the want of which, unless rigidly guarded against, would soon reduce the study of transcendental anatomy to the condition of some of the discussions in the no less difficult and sublime science of comparative philology; the key to which has been malignantly alleged to consist in the application of two great principles in the comparison of languages and words—namely, that "consonants are interchangeable, and vowels don't count."

The relation above indicated by the author between that part of the history of development which is common to various animals and their similar plan of construction is, perhaps, one of the most philosophical and important of the many topics he has brought forward; and one which, indeed, courts a further inquiry. Nor is it possible to deny that, to the strong arguments thus adduced, his views as to the unknown extent in which teleological laws, the details of which are at present concealed from us, may operate, form a fit appendix and complement. That the common plan of the Vertebrate series is related to the common development of its members, and to a life whose functions in general are, to a great extent, analogous in even the most unlike of its groups; such is the conclusion which, in the absence of an archetype invertebrate, not to say the impossibility of finding less than one such for each of many invertebrate orders, many anatomists would probably concur with our author as being both most accurate and more fruitful of future inquiry, than the stately edifice constructed by Professor Owen.

The other essay which heads this article mainly deserves notice for the able summary it gives of the views of various anatomists on the correspondent parts of the limbs. The chief novelty propounded by its author (M. Martins), which refers the antagonistic arrangement of the upper limb as contrasted with the lower to the torsion of the humerus on its axis, is one which, resting as it does upon an insufficient basis of fact, is disposed of by the statement of Mr. Humphry—that the twist is apparent and not real, being associated with no such torsion of the osseous tissue, and not even corresponding to that change of direction which this segment of the limb undergoes during the progress of fœtal development.

REVIEW XIII.

On the Medical History and Treatment of Diseases of the Teeth, and the adjacent Structures. Lectures delivered before the Members of the College of Dentists of England in the Session 1858–9. By BENJAMIN W. RICHARDSON, M.D., Senior Physician to the Royal Infirmary for Diseases of the Chest, and Lecturer on Physiology at the Grosvenor-place School of Medicine.—*London,* 1860. pp. 274.

THESE lectures were intended, as their author states, "to hold up to those who are engaged in dentistry proper the medical aspects of their science;" and they appear to us well calculated to carry out the object with which they were delivered. Without plunging into the troublous sea of dental politics, or considering the distinctive educational and other differences which separate the so-called College of Dentists and the Odontological Society, we would observe in passing, that it is not a little remarkable that the former body, which, as we are given to understand, entirely repudiates the necessity or advisability of dentistry proper being established upon the broad basis of surgery, should have so wisely sought to strengthen their position and secure their advancement by seeking instruction in general principles *ab extra.* However the determination to improve themselves in this manner originated, such lectures as these delivered to a body of dentists could not fail, if diligently studied, to do an infinity of good and to assist in the removal of such evils as of necessity wait upon the practice of dentistry which is not most fully and systematically associated with the breadth-giving studies of surgery, physiology, and pathology. The scheme upon which the plan for these lectures was originally drawn out has been so completely filled up that it appears to have been elaborated to an extent even greater than their immediate object required. Although professedly addressed to those of whom many had not the opportunities of a general professional education, these lectures deal with subjects to be only comprehended by those who have had the advantages of such an education.

Amongst the headings of the various lectures will be found the following,—viz.: The Constitutional and General Causes of some Diseases of the Teeth and adjacent Structures—The Hæmorrhagic Diathesis, Neuralgia, Hysteria, Dyspepsia, Rheumatism, and Gout, in their Relation to Dental Affections—The Diseases of Infancy and Childhood as dependent on and influencing Dentition—Purulent Affections of the Gums, &c.—Diseases of the Teeth and Maxillary Bones arising from the Effects of Mercury, Phosphorus, Lead, and other Poisons—Diet and Modes of Life in their Influence on the Organs of Mastication; and also a Chapter on Chloroform and the Narcotic Remedial Series.

In considering the above subjects, the author has in several places given expression to views which possess great originality, and which are manifestly the result of long-continued personal consideration; but the chief merit of the lectures consists in the clearness with which general principles are adapted as a repository from which the specialist may enrich himself. Moreover, we find that the subjects handled are most aptly and ingeniously illustrated in many cases, and the general language is forcible and racy, having here and there such a touch of quaintness as to amount unfortunately even to grotesqueness.

The following passage, treating of the effects of malaria, in the introductory lecture, will show how the author brings his own experience to bear on his subject. Speaking of the various poisons which implicate the teeth, he goes on to say,

"That these poisons—or it were perhaps nearer the truth to say *this* poison, the evidence being in favour of a unit poison—acts only through the system, is clear from the fact that it produces general systemic symptoms of an intermittent kind, of which the tooth symptom is often but one, though it may be for the time the only symptom. How this poison finds its way into the system is a point of great obscurity. There is strong evidence that it is sometimes carried by impure water and is absorbed by the stomach. In its intensity of action it is confined to particular districts of the country; to marshy districts of the country of all others, where it lights up endemic disease. But in lesser intensity the poison lurks everywhere, setting up neuralgic affections and tooth neuralgia, perhaps most of all. Often when least expected, this poison is doing its silent work, and teeth are not infrequently sacrificed to its fury, which in structure are as sound as unbroken enamel or dentine can make them. I remember some years ago being peculiarly impressed with this fact. In a village in which my professional labours were once exercised, there was a small plot of ground covered with houses and little gardens. During one season I was constantly having under treatment some of the inhabitants of this locality for the toothache; I am afraid that under pressure of solicitation and in ignorance of the cause at work, I drew more sound teeth than diseased ones for the people of this place. In the ordinary course of professional practice, the origin of the malady did not at first engage attention. At length an adult male inhabitant of the place came to me, not with toothache, but with brow ague. He remained under treatment some time, and though relieved by quinine, did not recover. He went to one of the London hospitals, and was very properly advised to try change of air. He took the change for a week, and had no sign of his disease. He returned home, and the disease at once returned to him. The same process was again repeated with the same results. On his after report of these facts to me, I was led to attribute this disease and the attacks of others in the locality to some central and local cause. The cause once looked for was soon found in the shape of a common, foul, and untrapped drain, the mouth of which was the open cloaca for the district. The drain emptied and cleansed, all the cases of neuralgic pain disappeared."

Again, the following quotation will serve to indicate well the author's method of illustration. Speaking of the difficulty frequently met with, where the hæmorrhagic diathesis obtains, of arresting the bleeding from leech-bites, after alluding to the fact that the blood sucked by the leech from a healthy person loses its coagulating power, he proceeds to state that—

"A certain definite period is required after the removal of the leech before the blood which continues to flow through the wound possesses the power of coagulation. Thus the blood which is caught directly after the leech is withdrawn will not coagulate at all; blood received five minutes later requires twenty-five minutes to coagulate; blood collected ten minutes later still requires eight minutes to coagulate; and a full half-hour is required before blood regains its ordinary period of coagulation. The explanation of the continued flow from the leech-bite is, I believe, supplied in the fact that the leech, while drawing, secretes from its body and impregnates the wound with a fluid which has the property of arresting blood."

The whole of the chapter on the Hæmorrhagic Diathesis, from which the above is extracted, is of considerable interest. We cannot forbear also quoting the following curious observations (bearing also on a now much-mooted subject) made *à propos* of the treatment of hæmorrhage:

"In the anæmic condition upon which, as we have seen, the tendency to hæmorrhage sometimes rests, the anæmia will be found ordinarily to depend upon impure air rather than improper food. A person would become anæmic upon the best cheer ever set on Royal table, if he ate in a cellar and lived in his dining-room. Fresh air, sunlight, and exercise are the remedies here. If the patient tells you he has all these necessaries, then you may look for some other cause, still, however, connected with the air respired. I will tell you of one cause—that is, excessive smoking. I do not think moderate smoking does harm, but immoderate smoking does every harm, and especially in producing a disordered state of the blood. I once submitted the blood of a confirmed smoker to examination, generally and microscopically. In the morning, when the fumes were *off*, the blood drawn from his hand coagulated with moderate firmness, was of a red colour, and the red corpuscles were clearly defined; at mid-day, when the fumes were *on*, these conditions were all modified; at midnight, when the fumes of twenty-five pipes had done their worst, the blood drawn was of dirty hue and uncoagulable, while the corpuscles, floating in a liquor of unnatural density, assumed every variety of outline. The cause of this change is easily given: with the smoke of the weed there ascends an alkaline principle, an ammonia. Blow the smoke of tobacco in one whiff over a glass plate moistened with hydrochloric acid, and on drying your plate, it is frosted with sal-ammoniac."

The above experiment is a remarkable one, but it appears to us necessary, before attributing these changes in the blood entirely to the smoking of tobacco, carefully to exclude all other causes which might have assisted in producing them, such as the use of alcoholic drinks, a very ordinary accompaniment, as we know, of this immoderate use of the narcotic weed. Of such careful exclusion we look in vain for any evidence in the details recited.

We might occupy more space in extracting such further passages from these lectures as would give our readers that interest in them which they merit, but the extended scope necessary is not at our command. We would briefly draw attention to the lecture on the influence of Diet and Modes of Life upon the organs of mastication, to that concerning Chloroform and the Narcotic remedial series, and to that concerning the effects of certain Poisons on the teeth, as containing new and suggestive material, and as being of much practical value. And as the subject has been more specially treated of by Mr. Salter since the lectures were delivered,* with a different result from that obtained by Dr. Richardson, we cannot forbear particularizing the views laid down by the former gentleman, as respects the effects of the eruptive diseases in producing caries, atrophy of the teeth, and disease of adjoining structure, &c. It is no doubt an ancient and a prevailing opinion that eruptive diseases do bring with them very frequently danger to the teeth and neighbouring parts. This view is held by Mr. Salter, who in the place alluded to above states that he considers that in the eruptive diseases the "*materies morbi* affect the teeth by virtue of their being members of the dermal or tegumentary system, the system upon which the poisons of the eruptive fevers spend their force, and that, blighted and irretrievably destroyed, they light up in the surrounding periosteum an inflammation which, whilst it is destructive or curative, &c. &c."

Not so thinks our author, Dr. Richardson. His opinion is emphatically given, that, excepting small-pox, there is no proof that any of

* Transactions of the Pathological Society, vol. xi. p. 209.

the eruptive fevers produce the said affections of the teeth and adjoining parts alluded to; but that these are rather to be attributed either to pre-existing syphilis, or to a "mercurial treatment pursued for the cure of the disease." "Certain it is (he says) that the whole of the infantile diseases, small-pox excepted, may occur during the formation of the permanent teeth, and yet a set of permanent teeth may follow as perfect as may be desired." This question can only be settled by rigid observations carried on with direct reference to the question.

To bring our remarks on this work to a conclusion, we must say that in certain parts of these lectures we find some expressions of opinion to which we can yield no assent whatever—as, for example, when the effects of malarious poisons are spoken of (p. 7) as "being limited to the period during which the body is subjected to them," or where (as at pp. 12 and 14) reference is made to syphilis as being "epidemic," and epilepsy spoken of as not probably being at any time the result of dentition (p. 141). Again, we should not be exercising a truthful criticism did we omit to state that these lectures, as a whole, appear to have been composed hastily. Indeed the author, perhaps, confesses this when he refers to the "suddenness" with which he was called on to deliver them. Certainly many indications of this are merely typographical, but yet, seeing that the volume is a reprint of what appeared originally in the 'Dental Review,' they are to be answered for by the author. Thus, we have the word "odontotrophia," instead of "odont*a*trophia" (p. 11), giving a totally unintended meaning; and in three consecutive pages (87, 88, 89) the word "odontologia" occurs instead of "odontalgia." Similar obscurity is produced by the occasional substitution of certain words for others having a directly opposite signification, as when "temporary" (p. 144) is printed in lieu of "permanent," and "permitted" (p. 156) in place of "prevented." These defects are capable of easy rectification in any future edition; and "having delivered our mind" by remarking upon them, we are at liberty to sum up by stating our anticipation that these lectures will be extensively and profitably resorted to, not only by the mere dentist, but also by the surgeon-dentist, and, indeed, find deserved favour with all sections of the profession.

Review XIV.

1. *The Surgical Diseases of Children.* By J. Cooper Forster.—*London*, 1860.
2. *Lectures on the Surgery of Childhood.* Delivered at the Hospital for Sick Children. By Athol A. W. Johnson. Reprinted from the 'British Medical Journal.'—*London*, 1860.

A book on the treatment of children's diseases has long been a desideratum in surgical literature. Nothing of the sort, so far as we know, exists in any language. Of the two books which stand at the head of our page, the one does not profess to supply this deficiency, being simply a report of three lectures delivered last year at the Children's

Hospital. Nor does the other, though a much larger book, and bearing a more ambitious title, enter much more deeply into the subject. We confess to considerable disappointment on perusing Mr. Cooper Forster's volume; we had so long hoped that the surgeons of this metropolis might have had the credit of producing some work on the surgical affections of early life which might be worthy to be classed with Dr. West's comprehensive volume on those which come under the care of the physician, that we were grievously mortified to find that Mr. Forster in his preface renounces all idea of making his work a complete one, and therefore, as it seems to us, deprives himself of all reason for publishing a book of this size and pretension. It may be very well for Mr. Athol Johnson to give us, in a small pamphlet, the cream and marrow of the subject, and so to prove how well he could treat the whole matter when leisure and opportunity may serve; and we welcome this little paper as an earnest and promise of what we hope is to come; but to what end, we would ask Mr. Forster, should we lighten our purse and load our book-shelf by the purchase of a large octavo volume, if we are not to have the subject accurately and completely handled? Mr. Forster introduces his volume in these terms: "Almost every case referred to has passed under my own eye. Accordingly, the reader must not anticipate a complete systematic treatise upon the surgical diseases of childhood. Where my own observation does not afford any data, I have been silent."—(*Pref.*)

Now this we venture to characterize as an error in principle. Observation and experience are so extended in these days, that if a man affect to stand exclusively on his own practice, and to write only on what has fallen under his own eye, he will produce only a very fragmentary and imperfect book; one which has no right to appear as a substantial treatise, or to lay claim to the rank of an authority. Accordingly, this work of Mr. Forster, though evidently the production of a good and experienced surgeon, should not have aspired to any higher honours than to appear in the columns of some journal of reputation, where the publication of the clinical experience of one who has enjoyed Mr. Forster's ample opportunities would have been very useful.

If we were to search for the omissions, Mr. Forster would beg us to recollect that the work is not a systematic treatise, nor could we avoid admitting the plea for a book in which diseases of the skin are treated in four pages, and infantile syphilis in the four succeeding; in which rickets is not even mentioned, except in a small paragraph, which, to say the truth, is an insult to a large subject; where all the other diseases of the bones are comprised in two pages interrupted by a woodcut, which appears designed to impress on the reader's mind the rather familiar fact, that disease of the bones of the foot is accompanied by thickening and the presence of sinuses; where no mention whatever is made of the important surgical questions connected with the presence of effusion of pus or other fluid in the pleura; and where the question of surgical operation in croup is coolly handed over to "the physician!" Against this latter doctrine we beg leave most earnestly to protest.

In croup, as in every other disease, no surgeon has any right whatever to perform an operation which involves the question of life and death without a very clear and intelligible reason for doing so; and it is the part of an author who professes to have had large experience in such cases to guide his readers to the formation of such an opinion. The theory which would make the surgeon a mere scalpel-holder in the hands of the physician is surely antiquated; and we cannot, therefore, help regretting that Mr. Forster, who tells us that he has "performed the operation in various periods of the disease, and with varied success," can find nothing better to say about it than that he "doubts much whether when a patient is moribund be not the most advisable period for the proceeding." (p. 72.) Surely, before accepting so astounding a doctrine as this, we may fairly ask our teacher to assist our judgment by some data of his own experience (or even other persons', if he would condescend to make use of the labours of his contemporaries), and by some reasoning based upon cases and post-mortem examinations. The point is one of no little importance, and if touched upon at all, should not be quitted in ten lines.

Nor can we admit Mr. Forster's assertion, that "the entire matter of this work is original."—(*Preface*). For instance, on one very important disease—nævus, it appears to us, on a perusal of the chapter referring to it, that not one word is said which is not most familiar to every person who has ever read of, or treated, the complaint, while all the more important part of it is avowedly borrowed from the writings of Mr. Birkett. In the chapter on stone in the bladder, again, the greater part of the matter is certainly as well-known as 'Quain's Anatomy,' though we allow certain original statements are introduced, which might reasonably be demurred to by the believers in that most orthodox work. We would mention especially the opinion which Mr. Forster enounces (p. 179), that the artery of the bulb is always wounded in lithotomy, and that he "scarcely sees how this is to be avoided." Mr. Forster advises that—

"The knife should be plunged into the perinæum just below the scrotum, and about a quarter of an inch, or rather less, to the left of the mesian line. The point of the knife should be directed towards the groove of the staff, and may be steadily thrust directly backwards, if possible, into the urethra."

If the authors of the celebrated work above mentioned are to be believed, Mr. Forster will find that the artery of the bulb may generally be avoided by making the incision less forward, less deep at the anterior end, and (if we understand our author rightly) less far from the raphé of the perinæum. If Mr. Forster's meaning was, that *in children* the artery of the bulb is always wounded, he ought to have made it clear that he does so limit his statement; which, however, even so limited, we cannot believe, as we have had personal experience of numerous operations in children where there was no sign of any deep artery whatever having been wounded. We believe, on the contrary, that the reason why Mr. Forster always wounds the artery is because he makes his incision so much further forward than usual.

At the same time, we allow that in children division of the bulb of the urethra or its artery is of less consequence than in older patients.

Another subject on which we would remonstrate with Mr. Forster is the costliness of the work, induced by the introduction of uninstructive illustrations. What useful purpose is served by elaborate coloured drawings of rare forms of nævus, and of the ravages committed by cancrum oris, and such-like matters? They do not assist any one in the diagnosis or treatment of these affections, and they make the book unnecessarily expensive. If this volume were meant for an "édition de luxe" of some work on pathological anatomy, we should have pleasure in praising the accuracy and beauty of the pictures; but if we had paid money for a book to assist us in treating children's diseases, we should want more useful matter for our shillings. Others of the illustrations are simply superfluous; thus, Mr. Forster, after saying that in sounding for stone he prefers having the child held in the lithotomy-position on the nurse's lap, thinks it necessary to give us a picture of this elegant group; and in treating of gastrotomy on account of injury to and cicatrization of the œsophagus, he gives us a drawing of the body of a patient on whom he had operated, to show, as he says, the position of the wound and the extent of the emaciation; of which the second particular might surely have been left to the reader's imagination, and the first is quite accurately and unmistakeably described in the text. The subject hardly belongs more to the surgery of childhood than of any other period of life; yet Mr. Forster has given to this single rare case occurring in his own practice, as much space as he allots to the whole subject of injuries and diseases of the bones.

The numerous imperfections, then, of which some samples have been given above, show that Mr. Forster's book cannot take a very high place in our surgical literature, much less fill up the special void to which we referred in the commencement of this article; yet we should be very sorry to speak of it as if it was destitute of merit, or was the compilation of an ignorant or inexperienced person. On the contrary, in several parts of the book subjects are met with which have been treated in a manner worthy of the reputation of the author and of the hospital from which a large part of his experience is derived. We would instance especially the chapters on foreign bodies in the air-passages, and on hip-joint disease. Also, all through the book the opinions emitted are usually sound, and the surgery and pathology, as far as they go, are good. The mischief is that Mr. Forster has touched so many large subjects and exhausted so few.

Mr. Johnson's pamphlet is merely a report of three lectures delivered at the Hospital for Sick Children last winter, and reprinted from one of the weekly journals. They were intended to awaken the interest of students in the subject by pointing out its extent and the main topics which it embraces, with such observations upon them as time admitted of. They answer their purpose most admirably, and will well repay the perusal of all who are interested in the subject; but of course the space is too limited to allow of more than a brief mention of many most important particulars. Some, indeed, are hardly mentioned at all, but are possibly reserved for future courses of lectures. We refer especially to diseases of the bones and joints. The section on malformations is more full, and conveys in a very short space a great deal of valuable information; wanting, indeed, only some amplification and a few illustrative cases to form a complete treatise on the subject, excluding its orthopœdic branch, which Mr. Johnson has been obliged to omit, as Mr. Forster also has done. Still, the two books even put together will not make anything like a complete treatise; and we would exhort those of our surgeons who have the experience and opportunity not to let it escape them, but to endeavour to supply our literature with what it has long wanted—a complete and accurate account of the special diseases of early life which come under the care of the surgeon, and a careful and well-reasoned discussion of the methods of treatment which they require.

Review XV.

Introductory Lectures at the Re-opening of the various Medical Schools for the Winter Session 1860–61:

1. *Medical Times and Gazette.* Oct. 6th, 1860.
2. *The Lancet.* Oct. 6th and 13th, 1860.
3. *British Medical Journal.* Oct. 6th, 1860.
4. *Edinburgh Medical Journal.* Oct. 1860.

It will be difficult to give anything like a fair and detailed report of the lectures introductory of our current session at the various medical schools, without appearing to add another to the list. By most of those who profess an opinion in the matter of these inaugural addresses, it will be considered that on the afternoon of October 1st, 1860, there was a full sufficiency of prologue to the drama which has now, in mid-winter, reached its climax of interest, and is still in course of enactment. Certainly, the great bulk of the audience then assembled before the curtain would not care to be reminded of the preliminary part of the performance, at any interval of time short of the next sessional anniversary. Introductory lectures meet with small favour. No one owns to liking them. They "draw" well, they read and act well, "go off" well; but they don't work well. Nothing seems to come of them, and the lecturer generally registers a vow that the one delivered shall be his last. If not conceited and ill-conditioned, he is pretty sure to suffer a reaction from his laboured upward flights of thought and sentiment, in an uneasy sense of having had too much taken out of him in the time, in a semi-consciousness of having "got up" more than he has learned for the occasion, with a misgiving of having been "found out" in it. Especially is he alarmed at having kept all the talk to himself on matters which, if not altogether trite and conventional, those who listen are least inclined to accept on another's bidding. The boy-student, at the close of his audience of introduction, is left in a state of awe and hopeless bewilderment. His elder assessors rise from their seats, depressed by hearing how much more of attainment is expected from the rising generation after a three years' curriculum, than they have been able to lay up for themselves in any given number of lustra,—ashamed of their manifold deficiencies, aghast at the humiliation which they may expect to suffer in the consultations of their declining years.

It was not always so. Before the days of journalism and criticism, when medical essays as yet were not, the teacher—lecturers then were teachers—addressed himself to his introductory lecture with an inward well-earned satisfaction which he did not care to conceal. He had composed it carefully, had transcribed and punctuated it, as if promised to the press. He delivered it with a suitable gravity of tone and earnestness of manner, and having thus proved, he never changed it. We have now under our eye a MS. introductory lecture of this kind, traced by the hand most revered by us, which year after year in the early part of this century, never failed to fill the old Hunterian Theatre of Windmill-street with willing and attentive listeners. There, ever mindful of the great October Tryst, long before the hour of lecture, on the floor and in the galleries of the stately beautiful museum, the profession, in its large sense of class and numbers, held its annual rendezvous from the several districts of the West-End. Town-bred pupils of both the Hunters, of Hewson, Cruikshank, and Baillie were there; and there to meet them, in fealty to their common school, were provincial celebrities from the far west and north, their sons and apprentices, as yet undistinguished, on duty at their side. Retired physicians from the colonies, soldier surgeons on hurried leave, or in charge of wounded from the Peninsula, sailor surgeons on a night run from Portsmouth and the Downs, farmer surgeons, rosy and booted from the well-tilled east; lawyers, literary men, and artists not a few, used to exchange their friendly greetings and gather round the lecturer with expressions of congratulation and good-will. Summoned from the gallery, punctually on the stroke of two, by the dissecting-room steward and quasi-anatomist, old John Howison, to "take their places," this phalanx of well-proved active talent was drilled to precede the lecturer to his chair in the well-appointed theatre where, ranged on the benches nearest to him, they never failed to emulate the boy-students above in the cordiality of their welcome. There are those still living and famous in this Kingdom City of London, first among the very first as surgeons, chemists, and natural philosophers, who would not refuse their assent to the truth of this old and somewhat faded picture. We present it merely for illustration, by contrast, of our present less formal professional gatherings on the same time-honoured anniversary of introductory lectures, addresses, and discourses. The lecture to which we have alluded, the stock and staple "introductory" of many successive generations of students, was simple in its arrangement, concise and clear in its statement, kindly in its counsel. It gave a general view of the organized animal structure; it theorized briefly with John Hunter on life and its principle; it set before the assembled clansmen of this model school a succinct and very interesting history of the difficulties which William Hunter had encountered in establishing it. It recounted with honest pride the achievements of the several teachers who had followed that distinguished man in a succession of usefulness; and concluded with a few plain words of exhortation to those whose attendance implied their readiness to support the dynasty, and to do as had been done before them. Between the conventional sameness of the old-fashioned introductory lecture and the laboured novelties of the modern inaugural address, is there no likelihood of likeness, no promise of compromise, in the years to come? How this question has been met by the introductory lectures of the present year is within our purpose to inquire. Our opinion will be the more readily and agreeably recorded, as it is that of the profession at large, exchanged without reserve among themselves, and reported with full and graceful comment in their public journals. There is a great deal to gather, and not a little to store from these October addresses. Leaves of an autumn day, there is in them much of form and glow, with cunning, well-packed fibre, that we would not willingly let die. Choice words of our picked men, talking their best on matters which they best understand, they deserve at least an intelligent curiosity.

From the increasing number and growing sameness for some years past of these Inaugural Discourses, it has been suggested that the classes of the several London Schools should be assembled in some convenient central place of meeting, and that one orator in chief should do duty for the many. It is better as it is. Were the annual October harangue thus simplified and centralized, it would, on too many occasions, be fearfully formalized; and we should forego our present contingent advantage of approaching the objects of our interest under competing guidance and from different points of view. The general tone of the addresses before us is frank, cheerful, self-reliant, entirely free from acrimony, and calm even to content. Among the various sounds which find utterance from the different performers, there is no hint of discord, no trumpet blast of anger and defiance, no low wailing note of grievance unredressed. The movement of the many orchestras is one of measured, even time, and, as in the several passages, so in the rests, there is a consent, which in medical music is no less pleasing than remarkable. It is of happy augury and interest, that in the many earnest, animated harangues addressed to the classes of the rival London schools on the 1st of October, there is no breath of personal ill-will, no trace, by hint or imputation, of sectional jealousy and disparagement. Each vied with the other in a declaration of love and respect for the objects of our common profession, with an expressed determination to maintain its dignity, and to pursue its inquiries in the spirit of industry and truth. While in all of these addresses there was a zealous advocacy of the claims of their respective schools, there was no one of the many which, with the exception of certain local and obituary notices, might not have been delivered with full and equal propriety in the theatre of one school as in that of another. For many years past there has been no such evidence of tranquillity in the social relations of our medical republic as that which now presents itself. The lull is, as it would seem, complete. Is it to last? Beneath this calm hushed level of our surface-life is every germ at rest? Is nothing coming up? Are we, indeed, as much pleased with ourselves, with each other, and with our common patient public, as we are willing to appear? Have we pleaded our last issue with the Government? To our collective metropolitan interests, to our councils, colleges, and lecture apparatus we may find occasion to revert; but what of our Unionist brethren, under the law, in the far country? Have the farmer vestrymen ceased to bully, the tradesfolk and petty squires to domineer? Are the soldier-surgeons at home and at ease in their mess? Are they duly scaled and balanced with other "officers and gentlemen" in full, fair counterpoise of ration, forage, and pay? Our sailor-doctors, are they snug at last in their cabins, so long and so grudgingly withheld? Possibly to no one of the October orators did any one of these questions occur, as affording topic for their address. Yet enjoying this our present opportunity, we cannot forbear from suggesting them as fit subjects for discussion and active sympathy wherever medical men are gathered together in the common interests of their profession. That no comment was made on the preliminary examination of the medical student, as enjoined by the medical council, that no interest was expressed in the Oxford middle-class examinations by any of our educational Coryphæi, as bearing on the great work which they inaugurated, has to us been matter of regretful surprise. We may give a stronger expression to these feelings on noticing the omission of all record of our late advances in the different departments of medicine, surgery, and pathology. Were the ophthalmoscope and the laryngoscope unworthy of a passing word? Should we not have been again reminded of the world's great wonder, the subjection of pain to its antagonist, Æther, by a review of the grave experimental reasoning which in many hospitals of Germany, France, and America have caused it to supersede the chloroform, as in 1848 chloroform superseded it? Æther, the anæsthetic!—the crowning mercy vouchsafed to man in his agony, gracious in excess of the most passionate aspirations of humanity, marvellous in its effects beyond the wildest dream of imagination, scarcely yet to be realized—incalculable in its prevention of horror and misery, actual or recollected, to the world! Æther! akin in science to the steam-blast, to lightning, and the sunbeam, working with them on man's mind and senses up to the very climax of awe and grateful veneration! Was it by oversight, or failure of apprehension, that Æther, the anæsthetic, escaped all notice by the lecturer of St. George's, in his enumeration of the wonders of these latter days? Can it have been on his part a denial of what words are too weak to affirm? Was no one ready among the surgeons to denounce the new-born prejudice, real, stupid, or affected, that rejects the master remedy of bloodletting in all its contingencies, forms, and degrees? Was no champion willing to lift the ban of utter proscription from the lancet, and to re-assert the necessity, in many junctures of disease, for its timely and sufficient use? Would that some physician of mature experience had opened the academical year by a grave, unsparing exposure of the practices now in vogue of poisoning the sick and the feeble with food which in quantity they vainly strive to digest, of spoiling blood that is healthy, of killing that which is disordered, of gorging the liver, of inflaming the lungs, of maddening the brain by wine, beer, and brandy without stint—thus quenching the intellect in its last expiring rays, forestalling the unconsciousness of death, and dismissing the patient drunken from the world! This is but a *reaction*, we are told, from the opposite extreme of ten, thirty, forty years ago—an equivalént of slaughter in compensation for the countless thousands who then were bled, purged, and starved to death. In this balance of destruction the result is one of small value to the statistician. To the physician it is a double shame.

Of medicine and surgery in their present daily practice, the lecturers tell us nothing. Volunteers on the march, they decline to report progress, and will not even indulge us with its semblance, by marking time on the halt. We have already expressed our admiration of the harmonious movement, so remarkable in its combination, at our monster anniversary concert. We now commend to the pleased attention of our readers the excellences in detail of the individual exponents of the great common orchestral theme. Wound up, as it would seem, by order, they go well together and in part. The fault at which we hint in their performance is one which, it may be, we are ourselves in fault to find. It is not that their discourse wants music, but that they do not help us as clocks. They do not tell us the time of day.

But here we must confess to an oversight, a strange one, to which we are only just awake. Absorbed in the old system of teaching, and listening exclusively to the lecturer, we had forgotten the Press. By the essayist, by the journalist, by the monthly, weekly, quarterly, half-yearly reporter, the October analyst is now of necessity superseded in the history, social and intellectual, of our profession. How thorough the revolution which is here implied! Time was, a yesterday to many, in which the influence of the periodical press on the state and progress of medicine was so fitful and equivocal as to be hardly worth the count. The mighty, never-resting engine of man's most enduring labour that is now directed in its movement by many the first among our master craftsmen, that finds employment for every willing hand, was used among us until lately for the laying up in store of huge bulky reams, which were only in occasional demand; scarcely at all for the production of articles of daily necessity and hourly use. As a toy, or as a mill, it was manageable only by a few. Its directors were seldom in repute as practical working men—never, assuredly, as men in practice. Public opinion was not then its overseer. If not in a degree discreditable, it was never less than damaging to any surgeon or physician aiming at practice, that he should be known especially in connexion with the medical press. The lecturer, he of the upper order, refused caste with the journalist and the publicist. In their present relation how great the difference! How much of trouble before the change! Much of the journalist's present rank he holds by, through, or in defiance of, the lecturer. What the lecturer claims by patent in monopoly, the journalist transferred and sold as his own. There was bitter, unseemly recrimination. There was "law." There was the "reign of terror." Let us forget it. Soon it was found that as the journalist rose by the lecturer, so was the lecturer made famous and enriched by the journalist. The union was complete, for it was one of mutual, great, and unalloyed advantage. Such it continues. Who now of practitioners is too "rare and delicate a monster" for exhibition by the press? Who is ashamed of an essay that sells a number, and restores to its author an equivalent for his time? Who refuses as unclean the wealth and honour earned by honest publication? What is he who never reads, and ignores the journals, but the most audacious of idlers or the vainest of simpletons! Is any lecturer so nice on the point of honour that he refuses the advertisement, by print, of any lecture that is worth the type? What if the work of medical journalism were suddenly arrested by strike, or suspended by imperial decree! How would such interruption affect the general business and interests of the profession? Would it tell in any way on those of the individual surgeon or physician? Physicking and bone-setting, we apprehend, would go on much as usual in the various fields of practice. Every patient would be mated with his doctor. Yet in our corporate functions assuredly we should languish. The life of our house would have left it. As a community we should collapse. No drilling by any number of courses, no extension of curriculum, could again make a corps of us. We could recover only by recovery of the air and life-blood which we had lost, through the free spirit and ministrations of the press.

How if every lecture-room were at once and finally closed upon its class? How if lectures ceased to be — journals, essays, and case-reporting being suffered to remain? Would any harm come of it? Would learning and instruction cease? Would there be one teacher, one pupil the less? In our firm, grave belief, there would be little, if any, prejudice to the collective interests of our medical republic; and none whatever to those of the public at large. The talent excluded from the lecture-room would be more ingeniously and usefully developed by the opportunities and exigencies of the press. To the student there would be an economy, large and direct, in the matter of finance, with a proportionate increase of means for the purchase of books, of instruments, chemical apparatus, microscope, and other appliances. In the economy of time, *always supposing him well inclined and well directed,* he would also be largely a gainer. Here is matter for close, grave, yet most delicate inquiry, upon which, soon or late, we all shall have to enter.

There is a fine quality of negative excellence in our October Annuals of the present year. Their bloom is not gaudy; the flower oes not get the better of the plant. The range of thought is lofty, and though often soaring, is never out of sight. There is no history, no prophecy. There are few polemics. There never was less of the moral sublime.

And so, in our critical capacity, we are signally comforted, for in composition the sublime can only with exceeding and increasing difficulty be distinguished from its vulgar synonyms of clap-trap and twaddle. Of religious cant, the basest of rhetorical artifices, we who reverence Heaven may thank it, there is but little to offend. The boy-students were indulged, as usual, with their full allowance of trite maxims and mild metaphysics, but not with very much more than was good for them. Bacon, that unfailing piece of resistance in ethical repasts, was served out to them with wholesome moderation. Let us hope, from year to year, that less and less of the first hour of the student's winter-term of study will be wasted on these trivial platitudes. The lecturer who first meets and greets them in their class, if he be true to his calling and position, will never fail to secure their reverence for what of task and purpose he sets before them, even though it be not declared by injunction or formalized in phrases, which, at their best, are but the weak translation of home-taught rules and prayers. In truth, very, very few medical men engaged in the rude struggles of their profession, very few men competing in any busy section of public life, could find in their own attainments or antecedents a sufficient excuse (apart from egotism and vanity) for haranguing one or two hundred educated men on their moral and social duties. There is, indeed, a contingency of mischief in these gratuitous ethical episodes if they be coaxing, wheedling, flattering in their tone, sophistical in their framing, or in any the least degree equivocal in their tendency. Thus, a stereotyped assurance, endorsed by the lecturer, is forced upon the student from year to year, that, if he consents to work, success, in the limited professional sense of this much-abused yet most treacherous word, will be the unfailing result on his part of the bargain. Taking honour, charity, and religious obligation into the account, this dogma of the lecture-room is not, cannot, never will be true. It denies all earthly experience; it flouts a revealed hope; it unteaches truth. Many a brave lad of sound abilities and untiring industry has missed his opportunity of early or late advancement from want of means; from mischance over which he had no control; from the slander, jealousies, cabals, and disparagements of rivals; from the prejudice, the arrogance, the dull offended vanity of superiors; from his own nobleness of character; from his frankness, his humanity, his instinctive horror of flattery, subserviency, and intrigue, his respect for his opponents; in brief, from his self-respect. And this is true of the man, as of his father the boy. Many a man of clear head, of cold temperament, of capacity unbounded for labour, and using these advantages with discretion and to the full, has risen, in common phrase, to the top of his profession, not from or through his industry, however close and well directed, but by means and through acts made available in aid of his industry, which in his days of ease and triumph he finds it convenient to forget, and which are forgotten accordingly. By the well taught, well-trained English student, who knows from his earliest home-lessons that envy and malice are positive in this work-a-day world, that through man's own vices and infirmities no meed of service can ever be assured from man to man; by him who has been feelingly instructed in his rough-and-tumble schoolboy days under the law of collision with his fellows, that the best man is not always the best off; by nine-tenths of the student class these ethics introductory are rightly understood and taken at their current value. But there are some among the young who, presented with this apparently simple "labour and success" standard of merit by those in authority over them, would be sorely perplexed and cruelly depressed on finding that by it in their own case they were condemned, even when their merit was greatest, and by others most approved. Success, they were assured, would be the certain reward of their labour. They have not succeeded, therefore they must have been wanting in industry, or working in the wrong direction. Such would be their heresy of self-reproof! Rather than this, let them hear the truth, the rough entire truth from the beginning—that in no profession, certainly not in ours, is success the assured reward of merit, even when of a higher kind than that of industry alone—that men may work as Harvey worked, and be hooted and put under ban for the very excellence of their work—living, it may be, as Harvey lived, into the glory of their work; dying, as Harvey might have died, under their work, and the ruin which it had wrought. Disparaged, slandered, beggared, starved, still let the boy-student be told that he must work; work that he may live; work that he may live to work; for what and where is life's equivalent but in its work?

"Ein unmüss Leben ist ein früher Tod."

Tell him that, without his full store of labour he has no chance, or next to none (cheat him in nothing), of success; that, stocked with industry, he is ever ready for success;—that there is yet another law, the which neglected, greatly to succeed is miserably and in truth to fail.

As a further bonus upon honest industry, the slow division of our gentlemen cadets are flattered into the notion that by constant attendance on lectures and assiduous registration of hospital cases, they can become inspired physiologists and surgeons, each with his attendant genius. "Genius is Labour," "Inspiration is Industry," we are ininformed on the authority of Buffon, Carlyle, and others, by the lecturers in two different schools. This is a sophism, and the boys know it. They refuse the converse of the term, which it was calculated they would accept. Labour is not genius; industry and inspiration are not convertible terms. A dunce of a genius, a genius of a dunce, an inspired idiot, if their teacher wills it so, but never a dunce who is a genius. Among themselves they know where the wit lies, and only look for it in certain directions. Wise in the extreme east, Dr. Barnes seems among his compeers best to understand the "First Year Student." In the few sensible words we transcribe, it seems that we learn to know them both, each through the other.

"There is perhaps no profession in which a man need depend so little for success upon extraneous aid. This you will say is the sunny side of the picture. It has no doubt a reverse one—a side not without asperities and angularities. Upon this, however, I think it unmanly and unbecoming to dwell. Success rarely attends the querulous man. No doubt you will all have your difficulties, your disappointments, your days of expectation, and your hours of despondency. But unalloyed prosperity is not the lot of man, nor is it good for man. The true use of present adversity is to chasten and strengthen the mind; to teach us to look hopefully into the future, not alone of this life, but of the life beyond."

Work, work, work! In every school this year, as time out of mind, there was the usual crack of the whip. For a year to come, it will not again be heard. The lecturer has talked his best. The student has been set to work; who is to keep him at it?

As a profession, now in 1860, what is our state and condition, social, ethical, and scientific? Are we true to the tone of our calling? are we sound in the heart of our mystery? are we with right minds on the right path? are we in good working condition? It is in the spirit of these inquiries, and in full remembrance of the ever-present Press, that at a given place, on a given day, we consent to meet the herald-augurs of our tribe. And in the same spirit they, as best they can, are bound under tacit compact to meet us in our needful wishes. The sessional year last expired has not been barren of result, has not closed without its promise. How were its facts and inferences placed before the assembled profession; and what report was offered of its reform, now an accomplished fact, its councils, its registration, its poll-tax, all under the Act; in brief, of its medical politics?

"Hush'd is the stormy main, cold is Cadwallo's tongue."

Agitation there is none. Reform is without sense or motion; paralysed in its hour of triumph; extinguished by the blaze of its own bonfires. We registered, we signed and delivered, and all was still. Labelled and receipted as forty-shilling freeholders, we cared no more for the franchise. It had become expensive. Reformers paid, reform did not. The Greeks had been too liberal of their gifts. Yet no one owned to having realized his two pounds' worth of registration. It is scratched off as a bad debt. There is not an allusion by any of the interpreters of the professional *lingua Franca* to the sittings, sayings, and doings of the council which is at once our constitution and our Parliament. One notice only, an oblique one, is bestowed on the register of twenty thousand medical patronymics and forty thousand sovereign coins. Dr. Wilks, the representative of Guy's, speaks thus:

"He considered the register of little use while gentlemen whose names were found within it were guilty of practices which were only worthy of the advertising quack. The distinction founded on titles was trivial, and to a certain extent artificial, whereas he (Dr. Wilks) would rejoice to perceive a substantial difference; that those gentlemen who were legitimately registered were in no way imitating the charlatans who were prosecuted by their Registration Societies."

These are true words, and good in their truth. In the heart and limbs of our living, working, professional body there is no substantia difference. In 1860 we are what we were in the days of ante-registration. There is now among us, registered or unregistered, just as much false, pretentious physic, with surgery to match, regular or irregular, as when our volunteer battalions had not yet passed their Government muster, and the 40,000*l.* poll-tax was still in our pockets. Let us listen further to the voice from the Borough:

"Dr. Wilks considered that the great hope of the regeneration of the profession lay in the proposed union of general practitioners with the College of Physicians. He considered that the disconnexion of the profession from Apothecaries' Hall, and its re-erection on the basis of the College of Physicians, would be of immense benefit to the public, for they would thus be taught no longer to associate in their minds the art of medicine with drugs."

The profession to be born again, and built up again on a basis, wide, deep, and stable as that of the college in Pall-Mall East! A secession, lasting and complete, of medical plebs from the City side of Temple Bar! All physicians and no apothecaries! The result a benefit to the public, as to itself, immense! Certainly it is better for the general practitioner that he should no longer be an apothecary; for the apothecary, like the Turk, is sick and past cure, and with no will but that which is to be administered by his haughty rivals. Here is a painfully curious question. It will obtrude itself. How at last will the apothecary die? Will it be by a sudden onslaught of the legislature; by a revocation of the glass and bottle edict of 1815; by an act of suicidal frenzy; or, let it so be hoped, by a slow and painless process of extinction? Virtually he is no longer with us, and therefore let no man henceforth hope to be an apothecary. But let no man, least of all a physician, fling out his heels at the dying rhinoceros. The apothecary did his duty—his full duty—and, alas! in excess of zeal, a good deal more than his duty—to the physic-loving public, when the physician fell very far short of his. He examined and took security from nine out of ten of those who handle and prescribe drugs in their fearful variety, when the physician declined so to soil his hand. In respect of that wonderful trait in our national character now in process of rapid change—the passion for physic, with its reckless indulgence, the hardest thing that can be said of our old time-honoured, time-fitted apothecary, is that he was a true Englishman. Let the saddle be placed where of right it belongs. In the palmy days of trading physic—during the Regency and the two following reigns, the British public was always insisting on giving the doctor a mount. Sick or well, it would be ridden, and moreover neighed incessantly for its drench. Nothing loth, physician and apothecary—such was then the usage—rode double in front hard and bold for many a good year, until, liking the pace, the surgeon—one at least too many—got up behind. Three pairs of spurs, and each spur a lancet!! Then at last the patient, overweighted, lay down with his burden, declined all conventional grooming, and kicked at every dose that was not water in the bucket, or a globule, infinite, of nothing at all.

It was but short, this interregnum of the drugger dynasty. To many it brought wealth and consideration; yet through it how many suffered, and how many died? Thirty years ago, during the convalescence of a severe illness, we strove to busy ourselves by looking over huge reams of recipes that had been filed for a long series of years by the great apothecary firm of the West End. It was with something like humiliation (we were then young, ardent, and honest) that we traced the fast ascending movement of prescription from the occasional "haustus bis in die sumendus" of Baillie, Pitcairn, the Warrens, and the Heberdens, to the "quartâ and sextâ quâque horâ post pilulam præscriptam" of the physicians then edging or rushing into upper practice. In those days the physician who would not operate largely in the drug trade threw away his chance. He might be a great anatomist, a profound nosologist, but if not of acknowledged profligacy in prescription, he was "sceptical," had "little confidence in remedies," and so was set aside. Baillie, it will be said, was an exception. But Baillie was a pathologist, and pathology was then a novelty that was in itself a prescription. One is vexed and sorry, in these days of pure non-trading physic in its chastity, that the large majority of medical men were at one time dependent on the sale of drugs for their livelihood. But let it not be forgotten that such trading necessity was the custom of the time; and in useful necessary trade, who in this country will assert that there is degradation or dishonour? To the Company, as merchants trading in an article which they proved by daily use, as chemists working in a laboratory of which such men as William Brande and Warington have directed the powers and resources; to the apothecaries of the London Hall, what but respect is due from those who remember retail pharmacy in its days of scandal, when Jacob Bell, its hero president of the future, had not yet summoned his compeers to their long and successful task.

"A gain immense to the public," we are further told by the Lecturer of Guy's, when there shall be no apothecary among us, save and excepting the apothecary-regenerate and on a basis, the location of such basis to be provided by the College of Physicians. The public, we apprehend, will find no balance in their favour such as they would most care to find at the close of the year, in their relations with their apothecary, or, as they may prefer to call him, their family doctor. Off his basis, he will be to them in name and position just what he always has been. On his basis in Pall-Mall East, they will know nothing about him; and there he must not stay, or he will starve. Apothecaries, irrespective of the name, as things in truth, and as personal realities, we take pleasure in assuring Dr. Wilks, will never cease from the land. In this country, the medical man dispensing his own medicines cannot be dispensed with. Never, assuredly, was the apothecary element developed in a greater variety of form than at this present time. There is the old genuine trading apothecary, master, warden, or licentiate of the City Hall, and nothing besides; there is the Scotch or foreign graduate, practising as an apothecary in London or elsewhere; the emeritus and recanting apothecary, with or without a degree, lately admitted into membership of the London College of Physicians; and lastly, the proposed apothecary licentiate of the Royal College, providing medicines for his patient, but forbidden to call himself physician. Honour and respect to them all while faithful to their common trust. Renunciation of the Hall and its doings, adhesion to the College and its dignities, are the two recipes on which the Guy's orator insists for the regeneration of the profession. Will he, can any surgeon or physician maintain, that from the fees and abjurations required during the last two years by the authorities of Pall-Mall East from the many recanting graduate apothecaries, any the smallest result by drop or scintillation has been as yet, or will be secured, for the renewal in its flame and current of our professional life? To us it has seemed that in this monster proselytism for present interest there was a dead loss of dignity. Of this unseemly scramble for proffered honours, no hint was given, not a word was uttered by any one of the many who at our great October gathering had full and exclusive liberty of speech. Yet among them, renouncing his opportunity—oh! rare faculty of silence—stood he, registrar of the regenerated, who had assisted at the new birth of our full-grown brothers from the Hall. How 'scaped we telling from him of what he best could tell—the deluge of the doctorate;—the wild buffalo rush from wholesome feeding-places to stalled enclosures and prairies without pasture! He had stood at the gate, aside from the wild scamper, and had scored off the ingoings of the herd. Have we indeed seen the last of it? Is the act over—the curtain down? We would gladly compound for "claudite" if "plaudite" be impossible. The amiable registrar, it is true, eliminated a joke from a question which he did not care to answer, of "Who is a doctor?" It was a miss. The riddle was too easy. "Who is NOT a doctor?" would have been the harder guess.

We linger still with Dr. Wilks, finding him very good company for the work we have in hand. He examines without fear, and tells of what he sees. The great speciality question (and this is remarkable) was encountered by him alone of those prominent at our last October meeting. With characteristic vigour he assailed the specials along the entire line of their position, massing his principal attack on their hospitals in the centre, and harassing them on both their flanks of practice and publication. All that could on one day be done against the enemy was effected by Dr. Wilks. But the battle is far from over: the lines are still untaken, and by no living assailant will the end be seen. If the specialists surrender, it will not be to any force acting on their front. No fire of protest and denunciations, however well served by committees in battery, will drive them from their ground. They may be broken up (with many it will be so) by a failure of supplies. Large sectional divisions of regular professional troops, with general hospitals for their head-quarters, have sometimes been in jeopardy from the same economical difficulties. It seems that our brother practitioner, Oliver Goldsmith, was a volunteer in this civil war of the profession, some eighty or ninety years ago. When can we hope that it will end? The darling of our English literature is so invariably welcome wherever we meet him, that we repeat without hesitation the passage of which Dr. Wilks has made such happy use:

"In other countries the physician pretends to cure diseases in the lump. The same doctor who combats the gout in the toe shall pretend to prescribe for a pain in the head, and he who at one time cures a consumption shall at another give drugs for a dropsy. How absurd and ridiculous! This is being a mere Jack-of-all-trades. Is the animal machine less complicated than a brass pin? Not less than ten different hands are required to make a brass pin, and shall the body be set right by one single operation? The English are sensible of this force of reasoning. They have one doctor for the eyes, another for the toes; they have their sciatic doctors, and inoculating doctors; they have one doctor who is modestly content with securing them from bug-bites, and five hundred who prescribe for the bite of mad-dogs."

For our own parts, thankful for all that is said on both sides, we are inclined to wash our hands of this quarrel. Its argument is too wide for our limits. The facts are few, and before every one. There has been an abuse of a principle, wise or simple, faulty or the contrary, as it may be; still a principle and its abuse. A cause with a fair outward seeming has been caricatured by the extravagance of its advocates. To the abuse and to the absurdity a check, through the press, has been already given. The cause is still on plea. In the meantime the high-toned remonstrance signed by the few with names, and the "many others" without, now circulating widely through the profession, will be made to serve as an advertisement for certain special hospitals and institutions, the least reputable of those which are summarily and in the mass condemned. The prevailing tone of our pathology, limited, partial, structural as it is, has much to do with the speciality of our hospitals as of our book-writing. The manly denunciation by Dr. Wilks of this inveterate perversion of our one great purpose, will be read with thoughtful interest, even by those who recover in his words the echo of their own, repeated with fruitless iteration through the space of long and distant years.

"He considered that the body was so fearfully and wonderfully made that it was impossible to treat one organ separately; and as regarded any additional knowledge of disease which was said to be obtained by the attention being directed to any particular form of malady, he much doubted if this was the case, since diseased conditions were so commingled that they could not be isolated; and taking cancer as an example, the selection of cases of this affection, as it attacks the external part of the body only, and isolating it from other morbid conditions from which it has no actual line of separation, appeared to him (Dr. Wilks) to be the very best method for remaining utterly ignorant of its nature. He was sorry to think that special hospitals were often established from interested motives; and he spoke of the degraded position in which the medical man placed himself who was always begging for his charity, and advertising his cures in the newspapers."

There is another question of great interest now before the profession, which in its discussion may be found to have a close practical relation with special surgery, specific remedies, and their attendant advertising hospitals. Has it ever occurred to Dr. Wilks, has it never occurred to the remonstrants, self-proclaiming and anonymous, that were the assistant-physicians and surgeons of our great general hospitals more satisfied with their position and its contingencies at home, they would be less likely to seek for special advantages abroad? that much of the eccentric movement of our younger professional aspirants would be balanced by a more even method of compensation, and might be centralized under a law of progress which, if not uniformly rapid, would be less irregularly slow. On this most likely and promising topic there was, on October 1st, a strange unanimity of silence. Whether from failure in moral courage, or from excess of moral delicacy, who shall venture to pronounce? We leave it undisturbed. The question just opened is in many ways perplexed. The grievance which it implies is palpable, but slow to ripen for its own relief. For a lustrum, as we fear, of sessional years to come, Surgeon Sisyphus and Graduate Tantalus of our hospital wards,

"Ever climbing, never up: always sipping, not to sup,"

must submit to their traditional destiny; content, in modern phrase and presentment, to be distinguished as the slow men of a fast age.

Within the easy memory of man, the address or lecture introductory to a course of medical study wanted what was essential in the opinion of its audience, if it failed to assert, on behalf of the lecturer and his profession, a full belief in the truths of revelation, with an angry horror of materialism and infidelity. At the beginning, at the end of his address, generally in the climax of its peroration, always, somehow and somewhere, the orator contrived to introduce to his expectant listeners a shadowy, uncomfortable personage, designated for the nonce collectively as the clergy, or individualized as an anxious mother, whose cue it was to denounce physiology as synonymous with paganism, and the study of the human body as an employment leading somewhere lower than the grave. Thereupon the champion professor put in an emphatic, alas! too often a lengthy, vapid counter-plea of religious orthodoxy. And so, with approving cheers, the obtruding puppet theologian was dismissed; the secular religionist was silenced, and no more creeds or confessions were imposed on the class until October 1st in the following year. Yet be it not supposed that our disclaimer was self-accusing in its own excuse. These outrages on the moral and religious character of our profession were notoriously frequent; moreover, they were undoubtedly abetted by certain sects of the clergy, and this at a time when the clergy, in their collective character, had least right to denounce any other section of society for shortcomings in the doctrines or practical duties of the religion which by example they professed to teach. And so it went on, a false passage of arms, for the better part of a century; Religion deprecating the assault, Science ashamed of the defence; each combatant tilting annually with his opponent's shadow, and shouting defiance, at impossible distances, from coign of vantage on ground of his own choosing. It was time that this volunteer sham fight of the divinity doctors with the doctors of physic, unholy were it not childish, fratricidal if not a sham, should pass with things of the past; that it should miss its anniversary, and fail to come off.

Vertuntur tabulæ—medicine became humane; theology more usefully busy. There was a common ground on which to meet, a common cause to defend. In matters of faith for some years past we have been left very much to ourselves. These unworthy bickerings are at an end. The censorious professional divines who used to molest us have been of late years too much occupied with each other to waste time or breath upon us. And now, after long truce, in 1860, one of our chosen spokesmen, chief, without prejudice, of his year, has thought fit to close his address to the "gentlemen, ladies, and gentlemen students" assembled in the theatre of St. Thomas, by warning his audience against certain rationalistic and irreligious clergy engaged in the higher branches of education.

"He specially cautioned his hearers against those monstrous perversions of science which had in these days been exhibited, and painful to add, by clergymen of the Established Church; nay, worse, by professors in the very universities to which young men about to become ministers of the Gospel resorted for theological instruction. From the productions of these so-called Rationalists, it would seem as if the redemption of a fallen and sinful world was not to be proclaimed by the supernatural interposition of the Deity, but in another revelation, that of science: so that the humble Christian, yearning for the salvation of his soul, was to look for his assured hope to physiology and physic, especially to the principles of animal development which played so large a part in the new theology. If they were so far submissive to this rationalistic school, the immortal beings of whom God had graciously said, 'Let us make man in our image after our likeness,' might even attain to the dignified conclusion that they were the direct and lineal descendants of a baboon, of the hideous being which they might any day see in the national collection, the Troglodytes Gorilla, the highest of the animal creation, but yet a brute that perishes, and as widely separated from the divine form of man as the heavens were from the earth."

This is sad. It is a spoiling of lively hopes; the renunciation of a glorious promise. Never has there been more show of dawn in the twilight shades of physiology than that to which we now advance. The science of life in its complicated agencies is no close monopoly of master craftsmen and their apprentice students in the arts of surgery and medicine. It is no longer an affair of guild. Now first since our profession held and kept its own, theologians, acute, charitable, and profound, heart and head worshippers of the one God, ubiquitous, in spirit, and incarnate, are pressing to our schools, asking for our books, craving for our guidance in paths to them as yet unknown; and because forsooth a few of such, exceptional minds, adopt a line of thought along which we may not follow them without indignation, and attempt to render physical science subservient to their experimental views, are we to turn and reject their proffered association in the pursuit of natural knowledge? Surely, remembering that natural and revealed truth have but one source, and how on all sides and in a multitude of ways apparent discrepancies between them have gradually but certainly been eliminated by patient, trustful, and strictly philosophical investigations, there can at this time of day be no real cause for fear as to the issue, or any anxiety lest our profession should suffer by contact with "idealism." Again, considering the nature of the publication in question, its abstruseness, and the little likelihood there is of its finding its way into the hands of the medical student unsought for, we object to the raising of the question of infidelity in connexion with the book, inasmuch as this is but to go out of the way to sow doubts in the minds of those who would have escaped them, and uselessly to excite a curiosity which, according to Mr. Grainger's showing, would be dangerous. Men of letters, smitten with the love of our sacred lore, are renouncing history and romance for the study of themselves in their vital relations as organized sentient beings. Shall we repel these men in the matter of natural science, because we agree not as to the matter of theology? And he who links by analogy all things that live with each other and with himself, is he, the poet-philosopher, without his place in our modern school of vital science? Poet-philosopher, what words are these? How gracious, how fit for reverence, each without the other! How majestic, how divine in their combination! Love, wisdom, creative power! Assuredly in the study of our compound life such attributes are not of light account. Poet-philosophers! Aristotle was poetical; Goethe and Shakspeare were philosophers. Both were playwrights—one was a play actor. From the great German we first learned the structural homologies of the cranium with the vertebral column. In the doctrine of colours he is moreover an authority most ingenious and suggestive. Of Shakspeare do we not all know by our own inner life that he teems with physiology?

"There are many sensations felt by thrill and shock, instantaneously and simultaneously through the system, which begin and end with the blood in its general mass and current. This indeed seems to be the way in which passion and strong emotion work upon the human frame. Their immediate effect is felt in those parts of the body in which the blood is collected in the greatest abundance: they are further made known by the flushed or pallid cheek, by the bloodless lip, by the glaring brightness or dim suffusion of the eye. These physical expressions of human feeling have been best noted by the two men to whom in their respective ages man's compound nature was practically and thoroughly known, by Horace and by Shakspeare."

More than one mind of this poet-philosopher class is now at work in this country with us and for us, on the great problem which our profession has to solve. And already, by analogies of widest suggestion and research, the most varied and delicate, they have helped and animated us in our task of duty. Let us welcome and thank them as they deserve; using them as they wish to be used. It is a sorry jesting and self-damaging partisanship which banters them. Refusing the mystification of the "nervous system" (as the term is often vaguely made use of), shrinking from the too frequent cruelties of experimental vivisection, does the student of life thereby cease to be a physiologist? And here we cannot forbear stating our fears that operations on the living lower animals have of late become much more numerous than is necessary for the elucidation of physiological difficulties. True it is that, conducted circumspectly and by a good observer, they may in many cases be rendered highly instrumental in furthering the curative art, and thus subservient to the wants of man; and when undertaken with this view, and with such due precautions to prevent unnecessary pain as every humane mind would devise, we cannot but think such operations allowable and required. This is, however, very different from the lavish and reckless cruelty which we have reason to believe oftentimes characterizes these procedures when publicly conducted, partly to establish some scientific point, but not infrequently paraded for the purpose of eliciting the savage applause and admiration of a wondering audience for sleight-of-hand. It is but a single step from the revolting exhibitions of the amphitheatre for "Experimental Pathology and Operative Physiology," and under the fascination of cruelty, the student finds himself, without horror, in the torture chambers of Alfort. All honour to Professor Spooner, who has already denounced this scandal of the age in his introductory lecture at the Veterinary College of London. The accounts lately published in the newspapers of the Alfort Pandemonium are true to the letter. When residing in Paris, some thirty-five years ago, the writer consented, on the urgent request of the late Sir David Barry, to witness an experiment instituted by him at the Veterinary College of Alfort on the mechanical effects of inspiration as influencing the circulation in the jugular veins of a horse. In one of the yards through which we passed to where some distinguished French physiologists were assembled to meet us, we came upon a group of students engaged in dissecting different parts of the same living horse. The unutterable cruelties which were then being perpetrated by the boy operators seated on the flanks of the mutilated horse; the coarse jesting on the agonies of the slowly-tortured animal; the brutal battering blows by which any inconvenient movement of its head was resented, return to us still from time to time, in dreams which from their sickening horror we dread the most.

To this level, unless care be taken, may the student be made to descend under the example of his teachers and by the usage of the schools. Think of one's own son or younger brother presented with a prize decorated—*couronné* in French phrase—for dissecting a horse alive! "After all, it is but a very limited knowledge that is gained from experiments on living animals," was the opinion expressed within our hearing, a quarter of a century ago, in a large company of medical men, by a very distinguished member of our profession, whose early reputation as a physiologist had been established in great measure by researches of this kind.

> "Your Highness
> Shall from this practice but make hard your heart,"

says Dr. Cornelius to Cymbeline's Queen, when she presses him for

> ". . . . those most poisonous compounds
> Which are the movers of a languishing death,
> But though slow, deadly,"

of which she wills

> "To try the force on such creatures as
> We count not worth the hanging."

A woman, we are led to hope, will take this present horror from the time that suits not with it, nor with any time to come. Mary Somerville, once a Fairfax, has, we are told, addressed a memorial on this abomination, from the far side of the Alps, to the chief horseman of

France. May the Imperial sympathy of her who sits beside him support our generous countrywoman in her double self-imposed task of rescuing the noblest of animals from torture, and his boy-tormentor from the shame of inflicting it.

The *mêlée* of polemical warfare is amusingly illustrated by the first public result of Mr. Grainger's attack on the "Rationalistic" essayists. Within ten days from its delivery, this introductory lecture was denounced as materialist and rationalistic by a journalist who had been scandalized by the lecturer's nervine apparatus for the production of thought, and his chemical re-agencies for the development of the sentient functions, as set forth in the following passages:

"Hence we conclude that no action whatever takes place in the living body without causing an equivalent change in the organic matter implicated,—that, therefore, in this state of existence, there is no pure manifestation of life apart from matter: no motion, for example, takes place without a corresponding change, which is called waste, in the ultimate substance of the muscle implicated; no nervous current occurs but at the expense of the nervous matter; nay, even in the subtle operations of the mind, no thought arises without exhausting a portion, however minute, of the grey nervine of the brain." "I have myself for several years been satisfied, and have so explained the matter in my lectures, that if the exact chemical constitution of any organic substance, however complex, were known, and if we could present the component elements to each other precisely in the right order and manner, we could form muscle, nervine, and bone."

Was the lecturer without misgiving when he spoke these words? Had he forgotten them when he denounced the Rationalist clergymen for resting overmuch on physiology and science? Rationalistic or not, this localizing of thought in the cineritious structure of the double brain, this amalgamation of logic and pure mathematics "by equivalents," with corresponding infinitesimal atoms of grey matter, always appeared to our limited apprehension as materialism burlesqued, or materialism under mystification. Thought in the double rind of the double brain!—Thought in the pineal gland!—Thought in this, that, or the other pulpy morsel!—Everywhere revealed, let us seek it nowhere. Thought is not a thing of bits and pieces. Thought is a presence; without a place.

Having had it all his own way with the Oxford and Cambridge essayists, the lecturer spares a few closing words for the correction and dismissal of the most specious of modern interlopers on the waste, unsettled district of traditional physiology.

While forbearing to touch the great argument now in process of elaboration by the master-mind in which it originated, let us not be seduced by Mr. Grainger's somewhat rhapsodical peroration into an undue admiration of ourselves in respect of our structural humanity. Let us not be frightened by an instinct of jealous horror into the absurdity of denying all community of make and material with the things that soar and the things that crawl. In every fibre of every muscle, in every filament of every nerve, does not the anatomist trace the closest parallel of resemblance between his own double symmetrical body and that of the twin-shaped mammal which he holds in subjection. No one has ever looked at the sectional bands of the abdominal recti muscles without being reminded of a serpent. We can all testify to certain disagreeable suggestions presented by the lower bones (least and lowest of all) in the sacral prolongation of the spinal column. Eating, drinking sleeping, waking, are we not in function identical with countless millions of creatures whose destiny has not been human? Why this vain denial of great physical truths patent as light to the dullest sense and plainest understanding? Why this presumptuous, wilful blindness in face of a design where nothing is dark? Why, but that in our self-assumed attribute of excellence, being vain we are wilful, being presumptuous we are blind. The family likeness, it would seem, is shocking in our eyes. Is our own race one of constant uniform beauty? Are our individual personal attractions always of the super-monkey order? Is the aboriginal Australian less hideous than the troglodyte baboon of the British Museum? Were the Irish Celt recruits of Pio Nono lovely as Antinous in the eyes of those who live beneath the Capitol? We glory in the forms, express and admirable, of our living human structure: so would the elephant and the monkey glory in theirs, had the faculties of thought and comparison been added to those of sense and sensation, which they enjoy with us in common. As regards beauty, let us ever remember that even among our human selves its standard is conventional. Content to be as God made us, let us deny no identity, refuse no analogy of physical form or structure with the other created things which, mere uncreating workers as we are, we dare to designate as the lowest of His animated works. Do we not disparage His Image by reviewing it as a thing of earthly sight, as reflected from the human face and form alone among created things? Even in a mere physical sense can we thus limit ubiquity? And can we without irreverence consent to receive in a sense entirely and exclusively physical the words in which our nature most exults? Is not the image spiritual, of Him who is Himself a Spirit? Assuredly (whatever the analogies of our structural form) it is in our faculty of thought, love, and veneration, that the image of all creative-power declares its presence by reflection. Though the shrine be human, here pictured it abides, a spirit in our keeping.

We are compelled to these remarks by the undue importance which, as it seems to us, is attached by Mr. Grainger to the external configuration of our race. The supernatural dignity ascribed by our lecturer to the human animal, as regarded in its physical aspect of form and feature, contrasts strangely with the doctrine of communism so prominent in the early part of his discourse, under which he surrenders to the chemist all the elementary particles of all the men and women who ever lived as so much waste and raw material for the one great crucible in which, organic or inorganic, all nature is compounded. Moreover, in these final passages the wit of man, as exercised in physiology and general science, is not very handsomely spoken of; so that, both chemically and intellectually, he is reduced, under Mr. Grainger's analysis, to a lower level than that which he has hitherto maintained.

The only really civil things that are said about him are those which relate to his personal appearance.

There is a dead set being made just now by certain sciolists against the human understanding. Strange perversion of thought, language, and purpose! It—the faculty by which we hold the world entire, without which all were blank negation—is degraded by these anti-rationalists to the level of a self-imposed function of irregular aim and uncertain use, vicarious with prejudice, forbidden as presumption. The superhuman privilege of our being, its element of intelligence, is discarded as a guide, denounced for its aspirations, disparaged by irreverent comparisons. An emanation it remains, and unrevealed. What is this but to blaspheme? Refusing thought, we limit worship. How dull the wisdom, how defiant the humility, that refuses to the mote its place in the sunbeam! Man, in all save apprehension, how like the beasts that perish! In fear and blindness, how unlike a god! From self-degradation such as this, if true to our calling, we must be safe—students of life, we consent without terror to the light.

Sojourning from hour to hour on the confines of life and death, it is in our privilege that men should listen when we think aloud of what is ever before us—when we invite them to the contemplation of the great twin mystery by attempting a definition of its terms. In respect of life and its phenomena, nothing has been said in England of late years more exact and comprehensive than what will be found in Mr. Grainger's retiring address to the students and others connected with St. Thomas's Hospital. We observe, and with hopeful approval, that "ladies" (wives and daughters may we say?) were not excluded in their capacity of women from this thoughtful, well-employed assembly. These few last truthful words of Surgeon Grainger give of themselves a stamp of high comparative standard to the Introductory Lectures of 1860. May the shades of Blizzard and Abernethy, undisturbed by his denunciation of their college faith, forbear to vex the nightly musings of our invalid philosopher in the expectancy of his retirement! In his zealous plea for the supremacy of the chemical and other known physical forces in all that constitutes the business of living structure. Mr. Grainger has, it may seem, been rather hard upon Hunter and his theory. With full and proclaimed admiration of its author, he demurs to the phrase of vital principle as a wordy unreality, as a mere echo of silence. Thus Hunter, as exhibited by the lecturer, is not the intellectual "giant" whom he designates as "rejoicing to run his course" across the solar heaven, but rather an earth-bound wanderer, pursuing to the end an *ignis fatuus* of error. Now, in truth, there is no absurdity in this notion of a vital principle—no false seeming in its verbal expression. A vital principle, special, exclusive, paramount in the animal organism, and in that only, defiant of all other forces, whether single, correlative, or in combination, would imply in the mind receiving it a chaos, such as under its operation would of necessity ensue to the creation which it outraged. Gravitation counteracted, chemical affinity forbidden, electricity arrested, heat, light, mechanics in their several laws of action suspended, that a whale may flounder, a flea may skip, a philosopher may walk abroad! That every living thing should carry within itself, from place to place, a law of immunity, instant and predominant, over the forces that pervade all else in nature! Impossible that the solecism of a paramount vital principle such as this should have escaped the understanding of Hunter. A vital force or principle, latent in organized structure, as elsewhere in nature, rendered active by the operation of a force similar to or co-equal with itself—a vital force that finds its special but not its exclusive development in the generation, nutrition, and other functional offices of organic bodies—a force that to living structure is necessary and essential, yet always in full and active correlation with other forces—why should such a force be forbidden to conjecture? Do we know all the forces that are now in physical operation throughout all nature? Do we know many in comparison with those that are as yet unknown, undreamt of? Is there no unbooked agency of all pervading power—no one as yet by lecture unannounced? Many and mighty things have been done by Davy, Faraday, and others within the walls of their Institution; triumphs, many and as great, are still to come from further development of powers which we register and know—from primal encounters with forces for which as yet we have no name, for as yet we know them not. Why may we not hope, now that all are lending their help—chemist, naturalist, geologist, and poet philosopher—why, with such pioneers, may we, anatomists and physicians, not look to find new treasures when we break new ground? Within us, and if within, of necessity without us, assuredly there are forces ever present, ever busy, escaping as yet the ken of those whose horizon is the widest, but lying, it may be, at our feet, casing us as does the air, and ready by the accident of a moment to reveal themselves in their agency for our use and our delight. Are we not as likely to catch these forces at their work in the living structure as anywhere else in nature? and if in living structure they first transpire, why not announce them as vital rather than by any other arrangement of letters? Vital, but not exclusively vital; vital, acting and suffering in co-relation with other all-pervading forces, chemical, electrical, galvanic, the few that we acknowledge, the many that we have yet to learn. Chemical, magnetic, why not vital, all in the same wide sense? Electricity, a name of yesterday. Why not neuricity or neurosity by the discovery of to-morrow? In galvanism there may be one day found a precedent for Hunterism. Even in our present rudimental poverty of words and things there is a fair warrant of analogy for the terms "vital" and "vitalism," as an expression of the composite physical force in its completeness, by which all that lives and grows within us is nourished and directed.

Had Hunter lived into this our present time, had he known what we know of the blood, living and without life, had he glanced at our new world of cellular origin and formation, had he experimented with Liebig, Faraday, and Matteucci, on the chemistry, the electricity and nervous currents of glandular and muscular structure, he would not have demurred to a vitalism of this wider range, not exclusive and transcendental in the living organic structure, but general and physical in its co-relation with other forces, and operating with them through universal space. He would not have accepted the few laws of the few forces which at present we assume to know, as implying, or likely to imply, in any the most remote interval of time, an explanation in full of the infinite mystery of living action, but he would have agreed to take them into the account. As a stand-point he would have consented to use them, still labouring for their expansion and further development in the operations of life; and ever yearning after new forces, or new laws of the old forces, which being vital (not exclusively vital) in their origin and development, vital in their application, would, with the biologist, be VITAL in their designation. In his battle with life, Hunter, were he now with us, would have called for more help than what his latest commentator considers sufficient. Chemical action, and other known physical forces, would not have sufficed for the living organism, as apprehended by the promulgator of the vital principle. We are assured with startling sincerity by Mr. Grainger, that if the exact chemical constitution of any organic substance, however complex, were known, and if we could present the component elements to each other precisely in the right order and manner, we could form muscle, nervine, or bone. We doubt it: the double "if" is sadly discouraging, but let him try on. Having "for several years been satisfied in the matter, and having explained it in his lectures," he will have a decided advantage over other experimentalists. Yet the stone and the elixir promise, it seems to us, a speedier return for the outlay of time and material. Mr. Grainger may turn out something from the jar or the crucible, a lump of something that the Wagners about him may agree to call bone or muscle, but one term of the condition will always be wanting. It will have no life in it, it will never have had any life in it. There will be no vital principle or force, latent or patent, in any the least of its atoms. It will not ever be vitalized. It will remain as it is, dead. The distance between chemistry on its upward way, and life in its stationary completeness, as measured by human apprehension, is great as ever—inconceivably great. Chemistry is somewhat easier to follow. Life is harder to come up with.

Chemistry has furnished us with certain analyses, more or less equivocal, of certain animal compounds. It has succeeded by synthesis in reproducing some half dozen or so of these proximate combinations of matter, but never with the life in them—never as organs, in structure or in action. But what comes of this pottering with the rawest of the raw materials of life, of this counting of atoms, and putting together of elementary scatterings? Towards the solution of the great problem it helps not, it avails not. In refusing Hunter's exclusive transcendental vital principle, we do not disparage Hunter. Of our grateful pride in the labours and achievements of that hero-student of life, let Mr. Grainger speak for us as for himself—

"Again, if we turn our regards to that other illustrious soul, the true founder of scientific geology and of philosophic surgery, if we consider Hunter, if, as every student is bound by his allegiance to do, we make oft and again the pilgrimage to his museum, the richest in the world, as presenting the very archetype of every class of organs existing in the animal kingdom, the mind is lost in amazement. How was this inexhaustible storehouse created? By labour —by the most painstaking observation—by an industry that knew no pause."

There is nothing inconsistent here in thought or feeling with the critical inquiry that follows. There is no higher appreciation of Hunter's intellect, truthfulness, and industry, than is afforded by the earnest concentration of mind which is alike necessary for those who agree or who differ from his doctrine. If ever thinker set the world on thinking it was he. May his work on the blood, as Mr. Grainger enjoins, ever be a class-book for the biologist! That life, in its animal organism, is essentially and always physical, is now a received truth. Already, after half a century of controversy, we have begun to count our gains. Let it be remembered in our future researches into the vital forces, that whatever we secure of the positive, we shall hold in succession, through Hunter, of a theory of negation. With Hunter on the same approved level as authors of the highest class in the matter of life, Mr. Grainger has associated two other distinguished names. We venture to demur, just at present, to the issue of these more recent letters patent of nobility. There has not been time as yet for full impartial verification of the documents on which the claim is made to rest. If Hunter left the blood in a muddle, its stream every day flows clearer. The nerves are still in confusion. The pathology of every hospital is continually unsettling the duality of the spinal column, sentient and motive, as proclaimed by the latest successor of Hunter in the school of his brother. Acute, eager, ingenious, long in advance on the track of the great nerve system, Charles Bell has left us still at fault. Like so many "nervous physiologists," he busies himself too exclusively with the nerves in their central arrangements, searching there, in the double hemisphere, the twin columns, the grey and the white strata, the laminæ, and decussations, for explanation of the sentient and motive functions, rather than in the different combinations of structure of which the nerves are organized parts, and from which they in truth arise. To the anatomist, the demonstration of this great ubiquitous double, or rather triple, system, is most convenient when traced from its symmetrical centres. The true functional physiological distinction of one nerve from another is obtained from examining them in their peripheral distribution, and classing them accordingly; there, in the several structures of single or double life, sleeping, sleepless, constant, periodical, voluntary, escaping or defying the will, sentient, incapable of pain, just as they are severally organized in special structural relation with the blood. Here, as everywhere in the animal organism, the one great clue, Hunter's clue, is the blood—the blood, universal, continuous, ever changing, ever the same. Hunter's blood, with its vital principle, implies too much of transcendental nerve. In Charles Bell's nerve there is not enough of blood.

Of the remaining work, on 'Constitutional Irritation,' introduced in triple unity of honour with Hunter's Blood and Bell's Nerves, to the reverent notice of the student, we have little right to speak. Its contents, we have heard and believe, go far to redeem for its author the character of reflective experience and practical usefulness, which is somewhat endangered by its title, of 'Constitutional Irritation,' a phrase conventional during two decades, the least ennobling of our professional annals; a name and address sufficiently attractive to verge on the discreditable, and especially suggestive of May-Fair physic.

Travers has his niche of fame with due observance from his friends. Hunter and Bell receive a wider homage, each in his shrine apart.

There is yet another branch of physic involving our highest professional interests, with duties the most painful and difficult, to which by the London lecturers no allusion has been made. What is our position with respect to the study, care, and charge of insanity? By this question is implied the most serious responsibility to which, as registered practitioners, we are daily and of necessity liable. We are bound under every consideration to know all that can be known about madness in its physical and moral effects, its many varieties, its behaviour, real or assumed, its prevention, its mitigation or cure by all approved methods of watchfulness, occupation, restraint, or general treatment. This heavy demand on the resources, energies, and moral firmness of every medical man is set well before the profession by Dr. Skae in his introductory lecture for the session 1860–61, delivered at Surgeons' Hall of Edinburgh, on October 1st.

"I cannot but regard it as a singular anomaly that the study of mental diseases has not hitherto received a place in the curriculum of medical education in this country, and that none of the licensing boards have ever required any instruction in this department of medicine on the part of their licentiates. The cure or alleviation of diseases of the *mind* appears so obviously to transcend in importance and interest the treatment of any bodily disorder, that we may well wonder why the study of this subject has been so completely ignored by our colleges and universities. . . . .

"You are neither instructed how to recognise its incipient symptoms, so as to interpose in time to save life, nor how to act in those circumstances of danger which paralyze every one with terror, and fill our daily papers with the most horrible tragedies. However desirable it may be to avoid overburdening you with too many classes, I cannot but regard the omission of insanity as a most serious one in the curriculum of your studies."

On this wide question of insanity in relation to practical medicine, we dare not enter. To its pressing and never-ending importance, who would demur? That, as a body, we are as competent as we should be to all that is required of art and counsel in charge of the insane, who would venture to assert? Dr. Skae, to put us right in this matter of insanity with the world and ourselves, urges in supply of our admitted deficiencies, more lectures; hints at another professorial chair. The cry is still they come. More lectures! Should there be any? What is lecturing in its double relation of talker and listener, professing, professed, and in reality? Here we are again in trouble; questioning ourselves as before—still halting for reply.

One word of suggestion. Let it be received with indulgence. It is not carelessly uttered. In the yards and galleries of the asylum is the system of protection that rests absolutely and exclusively on the vigilance and other good qualities of the attendants, so approved by reason and experience as to admit of no revision? Is the dangerous lunatic in no case to be secured by dress and other appliances against himself? Is he in all cases to be condemned to the jeopardy of non-restraint? There is a low, gathering, half-breathed sound of doubt and apprehension (has not Dr. Skae, have not his compeers, already heard it?), an anxious, fast, and often repeated wish, wide as the profession, general as the public at large, that the absolutism of non-restraint in the discipline of lunacy should submit, if not to immediate revision, to full and present dispassionate inquiry. Let us hope that the benevolent founders of our latter code of government, by kindly words and ways, will not insist on construing its text so strictly as they have done for these few years past; that, secure in their triumph over the brutality of ignorance, conquerors of the chain and whip, they will now, in further mercy, consider whether the suicidal and homicidal maniac should not sometimes be protected by improved mechanical contrivance from the mischief of his own fury. Which of us, if under the dark shadow of insanity, would not implore, while yet in his senses, that he might be thus prevented from injuring those far dearer to him than himself? Save me from this, at all risk and by any means! Save me, would be the last prayer of expiring reason; do not suffer me to stab my parent, to mutilate my child, if physically you can prevent it, from any conventional horror of a thumb muff or a jacket with long sleeves.

Enough to the full of us, and from the chair. If we talk on the subject of introductory lectures again, it will be from the benches. It is on that side of the table that the lesson is learned; and we would willingly know from those who sit there what is said and thought of their share in the bargain. It is not enough, for the future, that the pupil should be lectured. Have we assurance from himself, as from his teacher, that he is well and sufficiently taught?

Our mission from the first has been introductive. Its afterthought, as we hope, may be suggestive. There has been of late years such a parade of indifference in the matter of introductory lectures, such an affectation of distaste for all that under this designation is implied, that we have considered it urgent again to invite attention to an usage which has very often worked for good, and is never without a promise of what is better. Once in the year, at least, let us be roused in all our sections, to a sense of our high calling as students of life, in its highest aim and widest range. That a mere idler, speculative or practical, should claim to be heard on this occasion, he only among the silent, is an impertinence—a dishonest impertinence, if, with an inward sham belief that the whole affair is "a farce" and "a bore," he supersedes an efficient volunteer in the opportunities and distinction of the undertaking. There is no sort of excuse for having "nothing to say." It is the only occasion in a course of medical lectures on which there is no formal limit to what may be said. Assuredly, the lecturer should know up to the latest hour what those around him are about, the nature of the service on which he is detached, and how to fall back without loss of honour on the main body to which he belongs.

On the 1st of October the Profession stands to its arms, and the man in commission who talks on that day to his comrades of their common duty, should have been well up in front on the day before.

Review XVI.

A Description of the Human Body, its Structure and Functions. Illustrated by Nine Physiological Diagrams: Designed for the Use of Teachers in Schools and Young Men destined for the Medical Profession and for Popular Instruction Generally. By John Marshall, F.R.S., F.R.C.S., Lecturer on Anatomy in the Science and Art Department, South Kensington. 2 vols. *London.*

The first of these volumes, which are dedicated to Professor Sharpey, is devoted to "text," the second consists entirely of nine physiological plates, containing 193 tinted figures. These plates are fac-simile reductions of a set of large physiological diagrams, the publication of which was undertaken at the request of the Government Department of Science and Art, and which have been mainly derived from well-known anatomical sources. As it appears from the preface, it was originally intended merely to issue a descriptive catalogue of the figures in the diagrams, but this plan gave way to the more extended and complete one which culminated in the production of the text forming the first volume, and containing no less than 260 quarto pages. We are glad to see these systematic efforts on the part of the Government to place within the easy reach of the non-medical public the plain and elementary facts of anatomy and physiology; and Mr. Marshall has well seconded their intentions, his descriptions of the various portions of the body being "topographical rather than systematic," and being clear and for the most part free from pedantic technicalities. The language is, however, here and there not so simple as might be desired.

The teacher is recommended (and this is a good suggestion), we find, "to obtain from dead animals, whenever possible, the corresponding organs or textures, as an additional means of illustrating the descriptions and conveying information to his class," and the author proceeds to point out in which domestic animals the various organs and parts may be studied with greatest convenience. In the text we find nine chapters—one allotted to each of the diagrams—that relating to the organs of sense, which are so very well adapted for description, being apparently much the fullest and most complete. We find also at the head of the description of each of the sections a suitable introductory general notice of the special subject.

Plate 9, with its description, is devoted to the 'Microscopic Structure of the Textures,' headed by a short dissertation upon cell-growth. We should have liked to have seen in connexion with this chapter some information as to the use of the microscope, and some guidance as to the readiest way of procuring and exhibiting the numerous microscopical objects.

We shall be glad to hear that these volumes have been favourably received by those engaged in education, both in the higher and the lower schools of the country.

PART SECOND.]

Bibliographical Record.

ART. I.—*Clinical Researches on Disease in India.* By CHARLES MOREHEAD, M.D. Second Edition.—*London,* 1860. pp. 774.

FOUR years only have elapsed* since we were called upon to review the first edition of Dr. Morehead's valuable work on the diseases of India, and now the second edition is before us. On the former occasion we spoke in words of almost unqualified approval of its contents, and our verdict was confirmed by the profession at large. But we are glad to find that the author, relying less on the laudatory opinions of others than on his own singleness of purpose and clearness of judgment, has revised his labours in a far more critical and searching spirit than guided the pens of any of his professed critics. The natural result has followed, and the present edition is unquestionably in every respect a great improvement on its predecessor.

The chief improvements which have been effected, though comprehensive in their character, may be very briefly summed up. In the first place, the text has been revised throughout with scrupulous care. In the second, several defects of arrangement have been remedied. Thirdly, the author has omitted, and we think with advantage, many of the cases which were related in his earlier edition, and has replaced them by others of greater value. Fourthly, he has suppressed his account of certain diseases, such as plague and yellow fever, which seem never to occur in India, and the introduction of which into his work was obviously, therefore, an oversight and a defect; and he has added a valuable chapter on sun-stroke—a subject which, oddly enough, was scarcely alluded to in the former edition—and one on the hill sanitaria of the Deccan, which Indian medical officers will doubtless fully appreciate. Fifthly, the four years which have just expired have contributed of their wealth to our author's already extensive experience and knowledge of his profession; and, with the honesty of a true observer of nature, he has not hesitated to recognise and correct former errors of observation; thus, he now admits the value of arsenic in the treatment of intermittent fever, a remedy which, from insufficient acquaintance with its powers, he formerly underrated; and he recognises, what he previously denied, the occasional occurrence of typhoid fever among the diseases of India. Lastly, by judicious compression, and by

* See the number of this 'Review' for January, 1857.

the adoption of a smaller type, the work has been reduced to a single volume, and to as nearly as possible one-half of its original bulk.

It is out of our power in the limited space allotted to a mere bibliographical record, to do anything like justice to the book before us. If we are unable to find room to dwell upon its excellences, it seems scarcely fair that we should allow ourselves to point out its defects; yet we cannot refrain from making a few observations in the latter sense on a portion of the work which has specially interested us. We refer to the chapters dedicated to the description of febrile disorders.

We, like all practitioners of medicine whose opportunities of study have been confined to the diseases of a temperate climate, have been struck, as well by the large space which the subject of fevers occupies in the literature of India, as by the high ratio which these affections hold to the other diseases of that country. But we have been struck, too, by the general looseness and obscurity which mark the descriptions which are given of them by Indian authors; and we regret to observe that to a certain degree the same faults pervade the delineations which Dr. Morehead himself has furnished us with. We acknowledge his generally graphic descriptive power, his soundness of views in reference to treatment, and the evident truthfulness which has guided his labours; but these very qualities give strength and form to what might otherwise have been only a vague surmise, and convince us that the real source of the defects referred to is the want of precise knowledge of the subject treated on, and that there is at the present time at least as much confusion with regard to the diagnosis of febrile diseases in India, as there was not many years back in regard to the diagnosis of kindred affections here. To illustrate our meaning: The remittent fever of tropical climates is regarded as a malarious disease, or as ague in an unusually severe form; ardent continued fever, as it is called, is described as a specific affection produced, in the persons of newly arrived Europeans, by elevated temperature, excessive exercise, intemperance, and other causes. Now, we fully concede the specific nature of true malarious remittent fever, and we will allow that there is a distinct and peculiar form of fever to which the term ardent continued may be properly applied; but any one who takes the trouble to compare the descriptions of these two affections in the work before us, will see how very slight and artificial are the characters on which we are taught to found a differential diagnosis. It may be urged that a mere resemblance of symptoms, and consequent difficulty of diagnosis, imply the identity of two diseases; that typhus fever and typhoid, for example, which are now acknowledged as distinct affections, present so many points of similarity that they are still constantly confounded clinically with one another. This we admit. But surely, if no sufficient clinical distinction can be pointed out, and if further (contrary to what one finds in the case of typhus and typhoid fever) no trustworthy pathological or ætiological differences can be established, it follows either that the affections which we profess to distinguish are varieties merely of a single species, or that they represent an artificial division of an entangled web of individual but incompletely recognised diseases.

The latter alternative affords, we believe, the correct explanation of the obscurity of which we complain. It is only in the interval of time which has elapsed between the issue of the first and that of the second edition of his work, that Dr. Morehead has recognised, partly as the result of enlarged experience and partly on the authority of recent writers, the existence of typhoid fever in India. But who can doubt now that this disease prevailed there prior to the year 1856; and that Dr. Morehead, failing to recognise it as a distinct affection, incorporated his experience of it in his description of some other disease; and that the presence of this unrecognised element may have been sufficient to vitiate the whole results of his labours in reference to febrile disorders? The symptoms of typhoid fever constantly exhibit more or less of a remittent character, as is shown by the synonyms still employed by many practitioners; and it is easy to see that the general account of remittent fever furnished by Dr. Morehead is sufficiently wide to embrace the majority of cases of typhoid which may have come under his notice. But it is needless to push the argument further, for his own truthful records furnish at once the justification of our criticism and the proof of our position. We are convinced that no well-informed British practitioner could peruse the cases intended to illustrate remittent fever without entertaining strong suspicions in regard to several of them, and without claiming two at least (cases 32 and 35) as really typical and unmistakeable examples of typhoid fever. We cannot forbear adding, that Dr. Morehead has, in our opinion, still further vitiated his account of remittent fever by absorbing into it, as it were, other cases of disease as distinct from it as typhoid fever itself. Thus, Cases 29 and 31 are in every respect as well marked examples of pure meningitis as we have ever met with, and we are quite at a loss to discover what claims they can be supposed to have to the designation which is here assigned to them. The space at our disposal forbids us to pursue the subject further, but if we have said sufficient to induce Dr. Morehead to re-examine the materials out of which his chapters on fever have been framed, and to reconsider the conclusions at which he has arrived, the object we have had in view will be fully gained. We should be sorry, however, to conclude our remarks in a spirit of adverse criticism; and we beg to reiterate our conviction that, notwithstanding occasional defects, Dr. Morehead's work is unquestionably one of the most valuable contributions to practical medicine that have of late years emanated from the press of this country.

ART. II.—1. *Essai Théorique et Pratique sur la Cure de Raisins, étudiée plus spécialement à Vevey, suivi de quelques remarques sur les Conditions Hygiéniques de cette Ville, et de plusieurs Tableaux Météorologiques.* Par H. CURCHOD, Docteur en Médecine de l'Université de Berlin, &c., &c.—*Vevey et Paris,* 1860. pp. 134.

A Theoretical and Practical Essay upon the "Grape Cure," as practised at Vevey, &c. By Dr. CURCHOD.

2. *Du Raisin considéré comme Médicament, ou de la Médication par les Raisins.* Par J. CH. HERPIN (de Metz), Docteur en Médecine de la Faculté de Paris, &c. &c.—*Paris,* 1860. pp. 36.

The Grape regarded as a Remedial Agent, &c. By Dr. J. CH. HERPIN, of Metz.

THE "water cure," the "whey cure," the "grape cure," the "hunger cure," the "brandy and salt cure," the "movement cure," &c., may not inaptly be regarded as so many specimens of fancy work of the frequently shifting fashions of medical millinery. To some people a new sensation in medicine is as necessary as a new hat or a new bonnet, and sooner than not be gratified in their desires, they will accept the most exaggerated theory as eagerly as they will don the most extravagant costume. The class to whom such things appear more necessary are those who have both time and means at their disposal, and to whom it is equally the same whether they enjoy their novelties of either kind at Harrowgate or Clifton, or fly over to the baths of Kissingen or to the shores of Geneva. And herein lies the explanation of some of the beneficial effects which are found to partially attend the following out several of the methods of cure to which we have just alluded. Overstrained by full living, exhausted by dissipation, tired out by *ennui*, it is not to be wondered at that the constitutions thus "used up" should feel some advantage—often very great—in flying from the *salons* of London, Paris, and St. Petersburg, to the beautiful retreats of the Rhine, of the Pyrenees, of Switzerland, and the Tyrol. At these delightful asylums, these refuges for the destitute fashionable, it will matter little, perhaps, whether their visitors drink water, whey, or grape-juice, or attach themselves to spa No. 1 or 2. The element of their recovery lies in the general regimen and hygiene which they are obliged to adopt if they set to work to even partially carry out the particular method of cure; and whichever method they do adopt, they will find that the diet is a prescribed and a very simple one, the exercise ordered is regulated and sufficient, the hours of rest and of rising pointed out, new companions, enlivening conversation, and novel amusements are provided them, and all these beneath an exhilarating or balmy sky, and the influences of a magnificent or varied scenery.

We are far from maintaining, however, that the carrying out of these "cures" to a full intensity and prolonged extent has not in itself, independent of the collateral hygiene, any particular effect upon the constitution. Far from it. For a person to continue to eat from six to twelve pounds of grapes daily for as many weeks, or to drink as many pints of whey or twice as many pints of water, it can scarcely fail to happen that some important changes shall ensue in his functions, as brought about by the altered conditions of the blood gradually effected by pursuing such practices. What we particularly insist upon is, that the greater number of our periodically peripatetic fashionable health-seekers (who follow the "cure" in a broken way and to a limited extent), derive any benefit accruing to them from their newly-adopted and rational general regimen, rather than from a pound or two of grapes and a few tumblers of whey or mineral water. The "cure" at present before us—the "grape cure," "*traubenkur,*" "*cura dell' uva,*" or "*cure de raisins,*" if fully carried out, will, in many cases, we believe, produce not only very evident, but very beneficial effects upon the body. But in this there is nothing very surprising nor special to the particular method of "cure." The *modus operandi* of the "*cure de raisins*" is explainable to our minds by the important influence which the ingestion of fresh vegetable juices generally is known to exert upon the system, and the deleterious results observed to follow from their want. We conceive, therefore, it would not be a work of difficulty to get up a "water-cress cure" in Great Britain, equally as good as the "grape cure," if we could only transplant the scenery and climate of Vevey and Durkheim to the British Isles. The same principles which make the "grape cure" often beneficial in abdominal plethora, habitual constipation, hypochondriasis, chronic skin complaints, &c., cause the *cruciferæ* and *aurantiaceæ* to be so useful in "scorbutus," the fresh twigs of "bitter sweet," "broom," "goose grass," and "cleavers," in affections of the cutaneous surface, and fresh pot-herbs and culinary vegetables at dinner agreeable dietetic regulators of a sluggish alimentary canal. Nor can we doubt that some more very definite effects must follow if a resolute vegetarian would continue to eat a pound or two of "scurvy grass" or broom tops daily for a month. That the eating of nice ripe grapes forms one of the most agreeable modes of introducing fresh vegetable juice into the body must be granted. This particular juice, moreover, forms in its composition a kind of vegetable milk in the variety and utility of its elements. Glucose, mucilage, vegetable albumen and fatty matter, tannin, free acids, and acids joined to bases of potash, lime, soda, and magnesia, together with silica, alumina, the oxides of manganese and iron, &c., are to be found in the grape. Those who determine upon rigidly carrying on the "cure," eat not less than three, and sometimes as many as twelve pounds of the fruit daily for from three to six weeks. We have heard of even sixteen pounds being consumed within twenty-four hours. It is not surprising that diarrhœa, diuresis, stomatitis, &c., may result, attended by such depurative and eliminative effects as may permit of the patient getting rid of his abdominal congestions, and of witnessing his skin once again exhibit a healthy and transparent appearance. Nor, on the other hand, are we astonished that some who vigorously began the "*traubenkur,*" were soon forced to suspend it. But when we are told that grapes are "adoucissantes, bechiques, pectorales et alterantes, excitants, echauffants, toniques, stomachiques, astringents, corroborantes, diuretiques, laxatifs, et même purgatifs" (Herpin, p. 15); and highly useful in abdominal plethora, with all its train of dyspeptic symptoms, jaundice, biliary calculi, and hæmorrhoids, in chronic catarrh of the various mucous surfaces, in the dyscrasias, in gout, gravel, diseases of the skin, scrofula, tuberculosis, hæmorrhages, amenorrhœa, hysteria, hypochondriasis, diseases of the heart and great vessels, dropsy, albuminuria, diabetes, venereal diseases, mercurialism, and iodism (Curchod, p. 54); we can only raise our hands with becoming Oriental gravity, and exclaim *Inshallah!*

That a moderately strong and continued dose of fresh vegetable juices will benefit certain people there can be but small doubt; that in following out the "grape cure" they will adopt one of the best and most agreeable methods of taking their medicine, must be likewise granted; and we are equally willing to admit that Dr. Curchod's essay will be found a very useful guide to the "grape cure," as practised at one of the most agreeable places to which the valetudinarian can proceed.

ART. III.—1. *Description of a Deformed, Fragmentary Human Skull, found in an Ancient Quarry-Cave at Jerusalem; with an attempt to determine, by its Configuration alone, the Ethnical Type to which it belongs.* By J. AITKEN MEIGS, M.D., Professor of the Institutes of Medicine in Pennsylvania College, &c. &c. (Reprint from the 'Proceedings of the Academy of Natural Sciences of Philadelphia.') —*Philadelphia,* 1859. 8vo, pp. 20.

2. *Observations upon the Form of the Occiput in the various Races of Man.* By J. AITKEN MEIGS, M.D., &c.

"'Skulls, madam,' said the sexton: 'some of them must have belonged to strange fellows. Only see that one! Spirit of Eld, what a skull!'"—*Lavengro.*

THE skull referred to in the above title belonged to a skeleton of almost giant proportions, found in a deep and precipitous pit, about 100 feet from the entrance of a very remarkable cave which exists near the Damascus gate of Jerusalem. This interesting cavern is estimated to be 750 feet in length, and upwards of 3000 in circumference, and is supposed to have been worked as a quarry in the days of Solomon. Many circumstances favour the opinion that this quarry supplied the stones of which the first temple was constructed; for example, the heaps of chippings lying about, showing that the stone was dressed *on the spot,* according with the accounts of the building of the Temple; the absence of any other quarries of great size near the city; and the fact that in the reign of Solomon this quarry, in its whole extent, was *without the limits of the city.*

The skull found in this interesting locality is of the brachycephalic type, and is remarkable for the perpendicular flatness of its occiput. The author finds it impossible to say whether it is a very old, or quite a modern skull. Still, great interest attaches to it, on account of the fact that it presents an excellent opportunity to test the differential value of certain craniographic characters—those pertaining to the crown, the occiput, and the temporal region. Though almost unique —for an exact counterpart does not exist in the whole Mortonian collection—the author, after much research, met, in the 'Narrative of a Voyage to Madeira, &c.,' by Dr. Wilde, of Dublin, with the description of a skull brought by that writer from a tomb within the ground denominated Aceldama, or Field of Blood, which consists with the characters of the fragmentary cranium from Jerusalem. Enumerating the various races of men that have at different times occupied Jerusalem and its vicinity, the author arrives, by the process of exclusion, at the inference that the fragmentary skull in question is neither Jewish, Arabian, Egyptian (ancient or modern), Turkish, Roman, nor Persian. From statements quoted by the author, it would appear

"That the Parthians, Phrygians, and perhaps also the Cappadocians and Cretans, belong, in common with the Sclavonians, Finns, Turks, Kalmucks, &c., to the same short-headed group to which must be assigned our Jerusalem skull."

But the attempt to determine the exact place of the latter is still further complicated by the question of deformation. The author is of opinion "that the head has been artificially deformed by pressure, strongly, evenly, and continuously applied to the occipital region during growth." Glancing at the parts of the world where the custom of distorting the head is now known to have prevailed, and bearing in mind the brachycephalic character of the Jerusalem skull,

"We limit ourselves, in our attempts to determine the nationality of the latter, to a choice between the Mongols, Germans, Peruvians, Sclavonians, and a certain brachycephalic race, cranial specimens of which have been found in the catacombs of Paris by the late Dr. Harlan, and placed in the Academy's collection by his son."

From some further considerations, the author refers the Jerusalem fragment to the Burat cranial type, and "provisionally to the people and the region about Lake Baikal."

"From the foregoing remarks," observes Dr. Meigs, "it will be seen that neither occipital nor calvarial characters, *per se*, are as valuable as is generally thought by craniographers in determining the race to which any particular skull belongs. In like manner basal, facial, or lateral characters, taken singly, will not be sufficient to determine the type of a skull. This type is found neither in the base nor in the dome, neither in the occiput nor the sinciput alone. To a great extent it resides in the sutures, and is determined partly by the number and location of the ossific centres, and the rapidity with which development proceeds from such foci, and partly by the extent and direction of this development. During the centuries that have elapsed since man first appeared upon the surface of the earth, the ethnical peculiarities which appear to have originally characterized the laws of cranial development in the different races of men have become so masked or modified by hybrid interminglings of varied degree and kind, that the great principle of the correlation of forms is scarcely available in inferring from one or more fragments of a skull the typical form of that skull."

The author proceeds to show that in this respect the palæontologist and comparative anatomist are so differently circumstanced from the craniographer, that Cuvier, the discoverer of this great law of correlation, was able to announce, from the examination of a single tooth, the character of the entire skeleton of an extinct reptile, while the fragment of a fossil femur found in New Zealand, was referred by Professor Owen to an extinct genus of tridactyle struthious birds, and the correctness of this reference was afterwards attested by the discovery of numerous remains of several species of this genus. The law alluded to is, however, more applicable to the diagnosis of genera than of species, of species than of varieties; hence, as well as from the custom prevalent in some parts of the world of artificially deforming the skull, arise the principal obstacles to its practical application to human crania.

The foregoing may suffice to put our readers in possession of the leading points contained in Dr. Meigs' very interesting pamphlet. Those who are more specially interested in the subjects on which it touches may find much valuable information in the details of his highly philosophical method of argument.

The second production of Dr. Meigs at the head of this article was, like the first one, embodied in the 'Proceedings of the Academy of Natural Sciences of Philadelphia,' and may be regarded as a continuation of the leading inquiry started in that communication. It is founded upon observations on the collection of human crania in the Philadelphia Museum of Natural Sciences, which now contains no less than 1125 specimens from many different races of men; and these skulls are most elaborately compared by our author, with special reference to their occipital peculiarities; all comparison in respect of the conformation and anatomical points of other regions, such as the coronal, basal, facial, &c., being postponed for the present. It is of course impossible here to accompany Dr. Meigs in his dissertation concerning the minute characteristics of the occiput presented so variously by the skulls of very numerous diverse races and tribes. We can only refer to it in a general manner as evincing immense labour in the philosophical examination of this large collection of skulls, and also as a proof of careful research into the works of a multitude of authors, both at home and abroad, on ethnology and craniology.

As part of this conclusion, the following statements may be adduced:

That the modification of the human occiput—which varies in form, not only in the different races and tribes, but among individuals of the same race or tribe—may be arranged under five different groups, which are, however, reducible to three. These are:—1. The protuberant form, with the parietal half somewhat flattened so as to present an inclined or shelving appearance. 2. The vertically flattened. 3. The inferiorly flattened, in which the basal portion of the occiput slants upwards and backwards. 4. The round. 5. The globular. Of these, the two last merge more or less into one another; and the third may be regarded as a modification of the second.

As no form can be said to belong exclusively to any race or tribe, a marked tendency in them existing to graduate insensibly into each other, so none can be considered as strictly typical.

ART. IV.—*De la Fièvre Puerpérale devant l'Académie de Médecine.* Par le Docteur MARTINENQ.—*Paris*, 1860.

On Puerperal Fever: a communication read before the Academy of Medicine.

THE work of Dr. Martinenq is an interesting and useful contribution to the bulky literature of puerperal fever. It is one of the fruits of the protracted contest in which were engaged nearly all the more illustrious men of Paris before the Academy of Medicine in 1858. The author begins by giving in abstract the views of each of the speakers, and upon this basis builds his own deductions. His motto comprises his view of the pathology of the disease: "*Sans utérus point de fièvre puerpérale.*" He concludes that all the general and local symptoms take their rise from the morbid modification of the uterus; that the treatment must be directed to this modification; that, like all the great organic alterations, such as typhus, dysentery, variola, &c., puerperal fever may become infectious, and perhaps contagious.

The remedy advocated by M. Martinenq is the ergot of rye. He is of opinion that it acts by its property of inducing contraction of the uterus, and consequently by expelling all noxious matters. It is both prophylactic and curative.

The great lesson—one which we cannot repeat too often to our continental brethren—is to seek the extinction of puerperal fever by abolishing the practice of delivering women in hospitals.

ART. V.—*Die Operative Geburtshilfe an der k. k. Entbindungs-anstalt zu Graz.* Von MATTHÄUS FÜRNTRATT.—*Wien*, 1860.

Operative Obstetrics at the Lying-in Institution at Graz.

THIS work contains a valuable statistical and clinical history of 6770 labours which took place in the Lying-in Hospital of Graz during the four years ending July, 1859. During this time, 556 operations were performed, which seems to imply that one woman out of every twelve required obstetric interference. Some deduction, but not a large one, must be made for cases in which more than one operation was performed on the same woman. Of these operations, 147 were simply rupturing the membranes; 242 were deliveries by forceps; 63 removals of the placenta; in 4 cases turning by the head was performed; in 34 turning by the breech or legs was resorted to; excerebration was resorted to in 4 cases, and the Cæsarian section once. To the English reader the most instructive feature is the analysis of the forceps-cases. The author—theoretically, at least—condemns meddlesome midwifery. He assures us he has never resorted to art where this could harm more than aid. How, then, account for 242 forceps-operations, or 1 in 28 labours? The instrument was applied six times on account of oversize of the child's head: one mother died of uterine inflammation; three times for ossification of the sutures: one child died; nine times for malposition of head: one mother died of uterine inflammation; three mothers died; fourteen times on account of danger to child's life; six times for prolapsus of the funis: two children were saved; once for placenta prævia: the mother died; seventeen times for excess of liquor amnii; four times for rigidity of the external parts: one mother died of uterine inflammation; seventy-nine times for defect of uterine contraction: all the mothers recovered; six times for uterine cramp; seven times for inflammations of the uterus: three mothers died; once for ruptured uterus: mother and child died; thirteen times for narrowing of the pelvis: two mothers died; four times for weakness of the mother; eleven times on account of excessive efforts of the mother: five women fell ill, and three died; sixteen times for excitement of the mother; eight times for convulsions: five mothers died.

Of the total forceps-cases, 185 did well; 31 suffered from various affections (chiefly inflammation), but recovered; 26, or 1 in 9, died. The result does not strengthen the views of those who contend for a frequent resort to the forceps.

It does not appear that at Graz there was an excess of cases of pelvic distortion—a circumstance which formed a very striking characteristic of some German lying-in hospitals, the statistics of which have been recorded in this journal. Adding the 13 cases in which the forceps was applied, 4 cases delivered by craniotomy, 1 by Cæsarian section, and 1 in which turning was resorted to, we do not find distinct mention of more than nineteen instances. There is no mention of epidemic puerperal fever as a cause of death, yet forty-seven mothers, or 1 in 144 died. No specific mention is made of the use of chloroform, except in the case of Cæsarian section.

ART. VI.—*Cours Théorique et Pratique de Braidisme, ou Hypnotisme Nerveux considéré dans ses rapports avec la Psychologie, la Physiologie, et la Pathologie, et dans ses Applications à la Médecine, à la Chirurgie, à la Physiologie Expérimentale, à la Médecine legale, et à l'Education.* Par le Docteur J. P. PHILIPS, suivi de la relation des expériences faites par le Professeur devant ses élèves.—*Paris,* 1860. pp. 178.

A Theoretical and Practical Course of Braidism, or Nervous Hypnotism considered in its various relations to the different branches of Medical Science, &c. By Dr. J. P. PHILIPS.—*Paris,* 1860.

IN our April number for the past year (p. 441) will be found an article occupied with the subject to which the present work relates. We need do little more here than recal to mind the attempt which was made at Paris a short time back to substitute the condition of "hypnotic anæsthesia" for that loss of sensation brought about by chloroform. M. Broca's painless and successful operation upon a young woman in the former state was vaunted as an example of what we might avail ourselves in the future, instead of having to undergo the risks of the ponderable anæsthetics. But, as we before stated, the experiments afterwards made to bring about "hypnotic anæsthesia" available for the surgeon's purpose signally failed, so that M. Broca's case was finally left to stand almost alone. At the commencement of the trials our author, an ardent mesmerist and hypnotist, left his country asylum and proceeded to Paris to offer his experience to the eminent surgeons who were disposed to avail themselves of "nervous hypnotism" in lieu of chloroform. But he had scarcely arrived at the capital when he learnt, to use his own expression, that the champions of hypnotism had returned precipitately to their tents. This was a severe blow to Dr. Philips (by the bye, he does not indicate where he obtained his degree), who, however, immediately set about giving a series of "conferences," and has, farther, presented us with his present book. The admission which the author is forced to make towards the conclusion of his work, plainly shows, however, that the champions of hypnotism did wisely in retreating to their tents. Had they even waited for Dr. Philips and his "experiences," they must, it seems, have gone back all the same. He tells us of "Braidism" that

> "The surgeons ask of it an anæsthetic; it will afford them the most perfect so soon as we have realized certain improvements in its processes which have yet to be made, but which will certainly be the reward of serious investigation." (p. 159.)

Dr. Philips must come prepared with these improvements before he attempts to seduce the chirurgical champions of hypnotism again from their tents.

ART. VII.—*Chapters on Diseases of the Ovaries. Translated, by permission, from Kiwisch's Clinical Lectures on the Special Pathology and Treatment of the Diseases of Women; with Notes, and an Appendix on the Operation of Ovariotomy.* By JOHN CLAY.—*London,* 1860. 8vo, pp. 430.

THE first part of the above work is a translation of the 'Chapters on Diseases of the Ovaries,' in Kiwisch's well-known 'Klinische Vorträge,' an edition of which, enriched with notes, has been published since Kiwisch's death by Scanzoni. Of the eminently practical character of this work Mr. Clay's translation of the part alluded to will afford every one an opportunity of judging for themselves, and it only remains to be said that the translator has executed his task with fidelity and precision.

In the 'Appendix on Ovariotomy,' which forms the second and most important part of the work before us, Mr. Clay has collected, tabulated, arranged, and digested the results of all the operations of ovariotomy performed up to February, 1860; the appendix in question being an improved and later edition of a tabulated statement of cases of ovariotomy contained in Kiwisch's original work. The author states that he has, in all cases where it was practicable, obtained his information from the original sources, instead of relying on already tabulated statements, many of which were found on further inquiry to be incorrect. The statistical results of this important operation now placed before the profession are therefore the most complete and extensive of any yet published, and so far as statistics are capable of settling the question of the propriety, admissibility, or advisability of the operation, Mr. Clay's analysis of the results of ovariotomy up to the present time offers all that is perhaps obtainable. The author states that the various operators have in most cases willingly and cordially assisted him by forwarding accounts of their operations, but that from Professor Simpson, Dr. Frederick Bird, and Mr. Terry of Bradford, no accounts have been forwarded.

The results of 537 completed or attempted operations are as follows :

In 212 cases one or both ovaries were removed and the patients recovered. In 183 cases one or both ovaries were removed and the patients died in consequence of the operation. In other words, the result was favourable in 53·67 per cent. of these completed cases.

Completed operations performed in Great Britain and in America appear to have been pretty nearly equally successful, the proportion of successful completed cases being in Great Britain 57·20 per cent., in America 56·63 per cent. ; whereas in Germany only 25·49 per cent. of the completed operations were successful.

Tabulated statements as to the ages of the patients operated on, as to the condition of the patient at the time of the operation, as to the duration of the disease, the number of tappings, the length of the incision, the administration of anæsthetics, with the number of successful and unsuccessful operations under varieties of the circumstances in question, afford information from which no very important practical deductions can be drawn. There is one statement, however, as to the effect of *the presence of adhesions* which is interesting. Of 99 cases in which adhesions were not present, a successful result followed in 68·68 per cent., whereas of 286 cases in which there were adhesions, either slight, extensive, or requiring ligature, the proportion of successful cases was only 51·04 per cent., a result which appears to confirm the ideas which formerly prevailed on this subject, but which have been of late contested.

Table III. contains an account of 24 cases of *partial* excision of diseased ovaries. Of these, 10 recovered from the effects of the operation, and 14 died. Table IV. contains cases of attempted ovariotomy. In Sect. A, we have an account of 13 cases in which the tumour was not ovarian, chiefly cases of fibrous tumours of the uterus. In some of these cases the tumours were removed. In Section B, 82 cases are detailed in which the operation was attempted, but abandoned in consequence of the presence of adhesions; of these, 70·73 per cent. recovered from the operation. In 23 cases the operation was abandoned in consequence of the disease being found to be extra-ovarian; of these, 69·56 per cent. recovered. It thus appears that in 36 cases the operation of ovariotomy has been attempted in cases where no ovarian tumour existed at all—a fact which is indicative of the difficulty which sometimes attends the diagnosis of ovarian disease. It is somewhat remarkable, that of these 36 cases in which the diagnosis was at fault, the large proportion of 23 occurred in America.

Those who are desirous of pushing the analysis of the results of ovariotomy still further, will do well to consult the volume itself, which forms a valuable and well-timed addition to the literature of the subject.

ART. VIII.—*Beiträge zur Geburtskunde und Gynækologie.* Edited by Dr. F. W. von SCANZONI.—*Würzburg*, 1860.
Contributions to Obstetrics and Gynækology.

THE number of Scanzoni's 'Contributions to Obstetrics for 1860' contains the full proportion of valuable memoirs. Some of these have been noticed in our Periscope, or have by other channels found their way into the current stream of obstetric literature. It contains, amongst others, a paper by Simon on Vesico-Vaginal Fistula; one by Von Czihak on Extra-Uterine Fœtation; an account of the Puerperal Fever in Würzburg in 1859, by Von Franqué; a history of the Lying-in Hospital at Laibach for the year ending September, 1858; and a Memoir by Scanzoni on Amputation of the Vaginal portion of the Cervix Uteri for the Cure of Prolapsus of the Uterus.

ART. IX.—*The Pocket Formulary and Synopsis of the British and Foreign Pharmacopœias,* &c. By HENRY BEASLEY. Seventh Edition.—*London*, 1860. pp. 546.

THIS compendium, useful alike to the medical student, the practitioner, and the dispenser, has since 1855 attained another edition. As said in a previous notice, it is remarkable for the convenience of its arrangement and its fulness. It is obvious that great attempts at condensation have been exercised in pressing the material, and in this way no doubt it happens that in many cases the dose of the preparation mentioned is unfortunately not given. Moreover, we think it would tend to clearness and facility of reference if the dose were given always at the end of the descriptions, and on a separate line.

The chapter, p. 519, on the weights and measures of other countries, may be mentioned, among other things, as being very serviceable.

ART. X.—*The Elements of Natural Philosophy; or, an Introduction to the Study of the Physical Sciences.* By GOLDING BIRD, M.D., F.R.S., and CHARLES BROOKE, F.R.S. Fifth Edition, revised and enlarged.—*London*, 1860. pp. 699.

WE are glad of the opportunity of welcoming and recommending another edition of this very elaborate and practical work. It could not fail to be in demand considering that the scientific schoolmaster is so much abroad, and that not only in our great universities and public seminaries, where we are happy to find that these 'Elements of Natural Philosophy' are put into the hands of the students of physical science, but in the world at large. Moreover, a fitting appreciation of the work is shown by the authorities of the military medical service, as we find that in those cases in which the candidates for commissions desire to be examined in the elements of physics, this book is officially recommended to them. The medical profession have had to regret the death of Dr. Golding Bird since the issue of the former edition, but Mr. Brooke, in bringing out the present one unassisted, has evidently not in consequence permitted his energies to flag, but has fully kept up with the increasing requirements of the time. In his preface the author refers especially to the additions which have been made in this edition in the department of mechanical philosophy, instancing particularly the "principles of the motion of a rigid body or system;" the "theories of couples, of projectiles, and of oscillations," &c.; in all which abstruse questions the subject matter has been as much as possible simplified and rendered appreciable by the generality of students.

Mr. Brooke, in his preface, has, with much good feeling, appended a short and interesting memoir of his former talented co-labourer, Dr. Golding Bird, extracted from the 'Association Medical Journal' of January 5th, 1855.

ART. XI.—*Recent Works of the New Sydenham Society.*

"VIRESQUE acquirit eundo" would appear to be the motto of the above-mentioned medical book society; and, indeed, we know of no association which, as respects organization and growth, goes ahead at such a pace. Rising as a newly-fanned spark from the well-nigh cold ashes of the former society, whose name it was proud to adopt, its flames rapidly spread in every direction, and it has, under a vigorous and indefatigable council and secretary, steadily and faithfully fulfilled the intention with which it was originally started. With some drawbacks and exceptions as to punctuality and order in the issue of its productions, which are almost inevitable in the working and adjustment of so large a machinery, especially at its commencement, the council have put into the hands of now almost 3000 members no less than five important, interesting, and mostly well-illustrated volumes for their first guinea's subscription, and also three additional ones towards the series for the second year. The works issued for the first year, and also those expected as the remainder of the second year's set, will be seen mentioned in our "Medical Intelligence."

Of the three volumes but lately delivered by the council we would say a few words.

In our opinion the council have already selected no works the possession of which will give to the numerous members of the society more true pleasure and instruction than the 'Clinical Memoirs on Abdominal Tumours and Intumescence,' by the late Dr. Bright. The character and reputation of this great master of our art and science will of course at all times exact the closest attention to anything from his pen; but for general interest, for keenness and accuracy of observation, for conciseness of expression, and for practical value, few, if any, of his productions are comparable with this selection from the 'Guy's Hospital Reports.' Moreover, as a model of "case taking," and as an exemplar of the proper method and spirit of recording facts and utilizing clinical cases, we would gladly recommend this production to our readers.

In this matter also the Council of the Society have acted most discreetly in their choice of an editor (Dr. Barlow), who in putting into their hands this timely link between the immediate past and the present of British pathology, has added a preface in which, briefly, but with conspicuous clearness, is set before us a history of our knowledge of the relations between disease of the kidney, dropsy, and albuminuria, and in which he has pointed out with reverent affection the part which the illustrious Bright played in their discovery and elucidation.

Upon the edition of 'Frerich's Clinical Treatise on Diseases of the Liver,' of the first volume of which the council has given us a translation by Dr. Murchison, we have already commented (see Review for July, 1859). Since, however, the original was issued from Breslau, several pathological points therein dwelt upon have received additional attention at various hands, and Dr. Murchison has, in a preface, very appropriately and pointedly called attention to them, especially having regard to the views advanced by Kühne and others touching certain points in the clinical history of jaundice. From the preface we also learn that Dr. Murchison has, in this translation of Frerich's first edition, been able to avail himself of "most of the corrections and additions for the second edition through the kind co-operation of the author." Of the forty-two woodcuts given in the German edition, twenty-nine, along with one or two other figures, have been reproduced by Dr. Westmacott, and are given in the translation. The atlas, with its twelve coloured plates belonging to the original, can be obtained at Messrs. Williams and Norgate's separately. We find also a few useful observations added by the translator on the properties and composition of the German spas, and upon certain other points connected with the clearer understanding of the work. We shall eagerly look for Dr. Murchison's translation of volume the second, in which "the more important diseases of the liver will find a place."

The last of the three volumes emanating from the Council in the second year's issue is the long-expected 'Year-Book of Medicine and Surgery;' and upon this we wish that we could congratulate the Society more completely than we can conscientiously do.

We confess to some tender doubts as to the advisability of such a work being undertaken by the society, and we understand that when the question of its utility was first mooted, no small difference of opinion was expressed in the council chamber. From being, then, originally, as it were, a "bone of contention," we may say that from over-strained expectation it became a "*pièce de résistance.*" The medical public awaited its appearance with something like impatience, and no doubt it may be that from over-stimulation of the palate, the morsel when it arrived proved less savoury and nourishing than was anticipated. Certain it is that if undertaken at all, unless the 'Year Book' be merely a complete index or register of everything which has appeared, without discrimination—a plan which we are inclined to recommend—such a work should be made as perfect as circumstances will permit, both as regards judicious selection and fulness of material alluded to, and accuracy of quotation; and equally certain it is, that not only are several important papers which ought to have been mentioned in the 'Year Book' entirely unrecognised, but also a great number of unfortunate, and some ludicrous mis-translations and mis-spellings are therein to be met with. Of the mis-translations almost all are to be found in the headings or titles of the subjects quoted; and though these should not be, yet many may be fairly set down to the fact of the English printers being unaccustomed to foreign typography and orthography, and a great number are attributable to the subsequent hurried insertion of the English translation of the titles, even after proofs had gone to press, which had been by a misunderstanding omitted in the first instance. In this way it was that, instead of a real English translation of the foreign titles of papers and books, however brief, we often have merely a word or two pointing out the general nature or bearings of the original; of course, we cannot with any truthfulness or fairness mistake these mere indications for what they were never intended to represent. Very few of these errors comparatively speaking are, *bonâ fide*, from ignorance, they evidently arise mostly from downright carelessness, and fortunately but few will practically mislead the reader. We have neither space at our disposal for particularizing them, nor do we see that good would be gained by our doing so now, seeing that they have been already freely commented upon in the medical periodicals, and also that no new edition capable of profiting by our animadversions will ever appear. We are not then inclined to be implacably or rabidly critical as regards the 'Year Book.' We are disposed to think that the novelty and experimental character of the attempt, the difficulties and drawbacks connected with the construction and issue of the volume, and the other points to which we previously alluded, should tend materially to disarm our fault-finding. No doubt the greatest objection expressed or understood regarding it will be the exceeding brevity of most of the abstracts, and yet, considering the limitations of space and expense, we have found it in many ways very efficient; but there can be no doubt that to render it most useful, the subjects of which abstracts are made should be more rigidly selected; the abstracts should then be much fuller, and the collaborateurs ought to be twice or three times their present number, and should have entirely original works at their disposal, no 'Jahrbücher' being used except for the purpose of pointing out where new matter lies. Perhaps also the Council would do well to give no more translations of short foreign communications. This fuller and more perfect plan, of course, would require much more money than the council have at their command, and we shall not be surprised if it again become a question how far a 'Year Book,' under the present circumstances, as regards space and economy, is expedient.

We feel bound to say that, as respects the Reports on 'Practical' and on 'Forensic Medicine,' there is a perfect freedom from occasion of adverse stricture. We have also been informed that for the 'Year Book' for the present year the following arrangements have been made:—The type is to be smaller; a literal translation of all foreign titles of papers or books, &c., is to be given, the original being omitted; the titles are to be given in immediate connexion with the abstracts relating to them; and there is to be a new department under the head of "Special Therapeutics." Attention to these points will, of course, provide a large amount of additional space, and we feel sure that the forthcoming 'Year Book' will show how much the editors, as a body, will have gained by experience, and by criticism as well hostile as friendly.

PART THIRD.

Original Communications.

Art. I.

Clinical Researches into Morbid Pigmentary Changes in the Complexion. By THOMAS LAYCOCK, M.D., &c., Professor of the Practice of Medicine and of Clinical Medicine, and Lecturer on Medical Psychology and Mental Diseases in the University of Edinburgh.

IT is well known that the tint of the skin in disease is an easily available, and often an excellent guide to diagnosis and treatment. This arises mainly from the fact that changes in it indicate any important change in the constitution of the blood, but more especially of the blood corpuscles. Its physiognomical uses are well known in showing the race or temperament of the individual, and therewith his mental and corporeal tendencies. In these and various other similar applications, the tint of the skin is due to the presence or absence of the animal pigment, with or without changes in the blood.

Although these colour-characters are capable of such varied and important applications to practical uses, they are so imperfectly understood as to their nature and origin, that changes in the complexion in disease have had little more than an empirical value in medicine, and have, indeed, not infrequently led the observer into error; thus, the tint in "Addison's disease" has doubtless led to its being mistaken for icterus until very lately. The coincidence which Dr. Addison showed to occur between structural disease of the supra-renal capsules and a pigmentary deposit in the skin of whites (and whose conclusions have been confirmed by others), has of late years directed my attention to the clinical meaning and pathology of morbid pigment-deposits and pigmentary changes in the complexion. Although the results of my inquiries are not so definite as further delay might have rendered them, I shall have no difficulty in showing very conclusively that clearer views as to the pathology, diagnosis, and treatment of certain related groups of constitutional diseases may be deduced as well from the absence of pigment-deposit as its presence.

The following are the conclusions at which I have arrived, and which I propose to illustrate:

1. That besides blue and green, of rare occurrence, there are two common well-marked and distinct forms of morbid discoloration due to pigment deposit—the *yellow* or sallow, and the black or *swarthy*.

2. That both yellow and swarthy discoloration of the skin will

occur from the action of local irritants—as heat, light, cutaneous parasitic fungi, blisters, sinapisms, and the like, or in the progress of various cutaneous diseases of the skin and its appendages.

3. That the absence of pigment (leucopathia), as well as its deposit, may be caused by inflammatory and other diseases of the skin, affecting its chromatogenous function.

4. That morbid states of the cerebro-spinal centres will influence the deposit or non-deposit of pigment.

5. That morbid states of the genito-urinary organs in both sexes, acting probably through the nervous system, will determine the election of locality of pigment-deposit, according to the same law by which the development of sexual hair and pigment is regulated.

6. That structural diseases of the abdominal viscera and peritoneum also exercise an influence through the nervous system upon the local deposit of pigment in the skin.

7. That in disease of the supra-renal capsules, the bronzing of the skin, whether swarthy or yellow, is partly nervous, and due to the direct or indirect influence of the capsules or the kidneys and nervous system; partly hæmic, and in so far due to the morbid influence of "dyscrasic" blood.

8. That pigmentary changes in the skin of both whites and blacks may be the *result* of morbid causes, and yet may remain after the operation of the causes has ceased, and assume a physiological character.

9. That although local morbid pigmentation of the skin may occur exclusively from local causes, or the influence of the nervous system, in the majority of cases there is a morbid condition of the blood.

10. That the morbid conditions of the blood associated most commonly with pigmentary changes are characterized by those changes in the blood-corpuscles (leukæmia, leucocytosis) which are observed in cachectic states of a constitutional character (pregnancy, chlorosis, tertiary syphilis, chronic rheumatism, cancer, &c.), or which are intimately connected with "dyscrasic," visceral, or glandular diseases (of the spleen, supra-renal capsules, lymphatic glands).

11. That the tendency to discoloration increases (*cæteris paribus*) with age after a certain period of life.

12. That the morbid pigment-deposits proper, as distinguished from masses of altered blood-corpuscles, are carbonaceous excretions, and are often vicarious with the suspension or imperfect elimination of other carbonaceous excretions—as the carbonic and lactic acids, and the pigment constituents of both the urine and bile; and are consequently associated with morbid states of assimilation, as well as of elimination (through the skin, lungs, liver, kidneys).

13. That amongst the morbid states of assimilation, the rheumatic and gouty are specially to be classed, as well as those coincident with anæmia.

Semeiology.—Pigmentary changes in the skin, and pigment-deposits in the tissues, are observed clinically under the most varied conditions, and have given trivial names to groups of symptoms. *Jaundice* (*jaune*, yellow) is the simplest illustration of these. The deposit of black, or brown, or blue pigment in the skin of white races has led to the use of various nosological terms indicative of the change—as *melasma*, *melanopathia*, *nigrities*, *nigredo*, *bronzed skin*, blue skin or *cyanopathia*, *meliceris*, *stearrhœa flavescens*, *stearrhœa nigricans*, chlorosis (or *green* sickness), *melancholia*, *melanchlorosis*, *melasicterus*, &c. As to absence of pigment we have *albura*, *leuce*, *leucopathia*, *vitiligo*, *canities*, &c. The congenital absence known as albinism has always excited curious attention, and, as those who have treated albinos know, coincides with peculiar forms of disease. I need not refer to the albino forms of animals, nor to the curious ethnological doctrines and oppressive laws which have originated in the presence or absence of cutaneous pigment, except to say that a better knowledge of the pathological forms will necessarily throw much light on the physiological.

Classification of Morbid Pigments.—The pathological pigments are of two kinds. 1. The *spurious*, which consist either in foreign carbonaceous matters, or in direct modifications of the colouring matter of the blood-corpuscles after they have died; these pigments are all some form of hæmatine, and present all the shades of black, brown, yellow, and purple. 2. The *true*, being those pigments which are products of the transformation of the living blood-corpuscles, or tissues, and which must be held to differ from the preceding in the circumstance that they are the results of the action of the vital forces. They are of all colours; correspond in this respect to the normal colouring matters of animals; and are found in the cutaneous appendages and excreta, but especially in the urine and bile.

Le Cat, a surgeon at Rouen, was the first to examine systematically the morbid pigmentary changes of the human skin in their relation to anatomy, physiology, and organic chemistry. He details cases of melasma and nigrities, and distinguished what was evidently a case of "bronzed skin" from ordinary melasma and icterus.* He examined the pigment (which he termed "Æthiop's mineral") chemically, and showed that the colouring matter of the ink of the cuttle fish was identical in nature with that of the skin of negroes, and of the choroid coat of the eye. He was also the first to observe that the encephalic tissues of the negro were of a darker tint than in whites—an observation subsequently confirmed by Meckel and others, and very recently by M. Gubler. Although considerable progress in observation has been made during the last century, we may still say, with Alibert, "Les lois de la coloration sont encore couvertes d'un voile épais."

Modern inquiries have ascertained that black pigment is deposited morbidly in the tissues, mucous membranes, and capillaries (*melanosis*), as well as in the skin (*melasma*), and that it is sometimes present in considerable quantity in the blood (*melanœmia*). Its nature and composition have also been carefully examined of late years. Barruel first attempted to show that the chemical composition of the black deposit in *melanosis* was identical with that of the colouring matter of the blood. Breschet founded upon this analysis and upon his own re-

* Traité de la Couleur de la Peau Humaine, &c., p. 158. 1765.

searches the conclusion that the deposit was due to effused and modified blood with a large proportion of true colouring matter; and Heusinger, Lobstein, Andral, Trousseau and Leblanc, J. Vogel, Bruch, Rokitansky, Virchow, and others, have theorized as to the mode in which the pigment is formed from the blood.* It is now well established that although the pigment in numerous cases really consists of modified hæmatine, derived directly from the blood-corpuscles, the deposit in melanosis, melasma, and nigrities is not of this kind.

The term *melanosis* was first used by Laennec, who pointed out three forms of the disease. 1. Those in which the pigment is deposited in masses, whether encysted or not. 2. Free deposits of pigment in layers in serous membranes. 3. Infiltrations of pigment.† In 1821, Breschet added a fourth, the fluid form; and in 1829, Andral asked whether certain cases of pigment-deposit in the skin should not be classed with melanosis. Andral also called attention at the same time, not only to cases of inky discoloration of the intestinal mucous membrane, in which pigment appeared as a deposit into the tissues, but to another class (some observed by himself), in which it appeared on the surface of that membrane as a secretion.‡ These deposits of pigment in the tissues, that is to say, externally to the bloodvessels, is now well established.

Breschet and Cruveilhier seem to have been the first (in 1821) to detect pigment in the bloodvessels in the form of black, sharply-cut masses.§ It was considered to be rather a post-mortem phenomenon than a true pigment-deposit, and what they observed was probably due to the hæmatin of altered blood-corpuscles. In 1823, Dr. Halliday published a case of melanosis, in which he found fluid black pigment in the vessels at the base of the brain, and in those of the choroïd plexus.|| In 1825, Billard and Baily observed capillaries of the brain to be obstructed by pigment. Several years subsequently, Mr. Holmes Coote recorded a case of melanosis of the eye, in which he found a black matter present in the bloodvessels of the recti muscles of the globe, between the blood and lymph-corpuscles in appearance, and which moved with the blood-corpuscles when pressure was made on the vessels.¶ Of late years, this deposit of pigment in the blood vessels has been frequently observed and connected with the presence of free or celled pigment in the blood and certain viscera, but especially in the spleen. German observers have largely contributed to this portion of the subject, particularly Meckel, Ecker, Virchow, Planer, Heschl, and Frerichs.

The term *melanæmia* (first used by Frerichs) has been applied to that condition of the blood in which pigment has been found. Seeing

* Compare Rokitansky's Pathol. Anatomy, Sydenham Society's translation, vol. i. p. 204, and Virchow's elaborate paper, Die Pathol. Pigmente, in Archiv für Path. Anatomie und Physiologie, vol. i. art. 9.

† Bulletin de la Soc. de l'Ecole de Médecine, No. 2. 1860.

‡ Précis d'Anat. Path., tom. i. pp. 460–1.

§ Considérations sur une Altération Organique appellée Dégénérescence Noire. 1821.

|| London Medical Repository. 1823.

¶ Lancet. Aug. 1846.

how readily the carbonaceous matter is deposited in the skin and tissues in melanosis, and how abundantly in the capillaries, the conclusion was natural that in all cases it was a deposit made directly from the blood, without the intermediate vital action of the tissues in which it was deposited. Now, this mechanical theory may be admitted as to the blockade of the capillaries by pigment granules, the products probably of altered corpuscles, but it is by no means sufficient to explain the usual phenomena of melanosis or melasma. As to the latter, it may be observed especially that the deposit takes place in a tissue, the normal function of which is at least to receive it, but perhaps to excrete it; hence the change is in one sense a physiological process; whereas in melanosis and in blockade of the capillaries, the change is in no sense physiological, but purely pathological.

As I shall have to refer to *melanæmia* from time to time, I would observe here, once for all, that although the facts are so numerous and apparently so decisive as to the presence of free pigment in the blood, they require confirmation, and have in fact been controverted. In 1852, Zeroni stated in a contribution on the Treatment of Ague by Arsenic,* that he had examined the blood of ague-patients for the pigment-cells described by Heschl, and, to his great delight, he found them at once; but on examining the blood in other cases, and in the spleen of a fatal case, he could no longer find them, but discovered that he had used glass covers which, under a power of 300, showed objects marvellously like Heschl's drawings of his pigment-cells. Probably the doubts thus thrown on Heschl's researches are not altogether inapplicable to the researches of other inquirers, for Zeroni indicates a very certain source of fallacy. I have examined the blood of numerous individuals (certainly not fewer than 100), and am satisfied that nothing is more difficult than accurate observation of the pigment-elements. The slightest particle of dust, coal, or ash, is sufficient to give the appearances described by Planer and Heschl. Even a microscopic particle of dried blood, remaining on the slide, shows as brown pigment in a freshly-drawn specimen of blood take on the slide. So that the utmost care will hardly suffice to avoid fallacies in observations at the bedside. It appeared to me that the only method by which even an approach to accuracy can be attained clinically, is by examining the blood of a number of persons under the same conditions as to time, place, state of slides, and method of taking the blood, so that all the observations are equally liable to the same class of fallacies; in this way a comparison of the differences in the phenomena observed could be instituted. The results of clinical examinations thus made, I shall state shortly, believing that although not strictly accurate, they are sufficiently approximative for the first steps of a clinical inquiry.

Melasma considered generally.—There are two forms of pigmentation observed in the skin of men and animals; namely, those of the skin proper, and those of the appendages to it, as hair, quills, feathers, scales, shells. The pigment of the skin proper of man is contained in the soft, newly-formed cells of the epidermis, formerly termed the rete

* Deutsche Klinik, Nos. 40, 41.

mucosum, into or by which it is secreted. That of the hair, scales, and other similar appendages has fundamentally the same origin; for they are either compressed epidermic products (as scales), or produced from follicles which are fundamentally involuted portions of the derma proper. The function of these follicles is, however, modified by the fluid which is poured into them from the sebaceous glands. But these again are involutions of the derma. Hence the general relations to the pigment of the skin appendages are the same in both; it is contained in cells having a common origin at the surface of the body.

The skin in all men, whether white or dark, has normally a function of pigmentation, although the activity of it differs greatly in extent in different races, and even in the same race, from varying circumstances as to climate, food, and exposure to climatic influences. In white races this coloriferous function is almost in abeyance, especially in the clothed portions of the body; but it is easily developed under certain circumstances. In all, however, the anatomical relations of the pigment to the epidermic cells appear to be the same. Thus the pigment-deposit in "bronzed skin" has been found by various observers to have the same relation to the epidermis as in sun-freckles (*ephelis*) and as in the coloured races. The cells in which it is contained in the form of minute granules are covered with a colourless epithelium in the ordinary cases of melasma, chloasma, and the like, but in certain morbid states of the skin, in which there is rapid production of epithelial scales (desquamative or squamous diseases), the epithelium contains abundant colouring matter. This is observed clinically in pityriasis versicolor, in various forms of ichthyosis, in cases of true leprosy known as "black" leprosy, and in lepra nigricans. A similar rapid production of pigment may take place either within the hair-follicles or the sebaceous glands, and be poured out on the surface of the skin. This is seen in *stearrhœa nigricans* and *stearrhœa flavescens.* It is probable however, that it is poured out also as an excretion from the sudoriparous glands.

Leucopathia in relation to Melasma.—It is not every part of the human body which is equally dark, or has equally a tendency to become dark; on the other hand, in coloured races, and in portions of the skin or its appendages of the whites, which are naturally dark, there is sometimes a morbid defect in the pigmentation. This state has been termed white disease, or *leucopathia.* It is to be observed, however, that there may be a pathological leucopathia; that is to say, white spots due to morbid changes in the skin may appear amongst coloured patches of white skin due to excited action of the epidermis. This gives a mottled appearance to a bronzed skin—i.e., a skin darkened by disease, and may tend to puzzle the observer. In particular it may lead him to mistake a pathological tint for the dark tint of sordes of the skin. The mottling of the skin in cases of melasma, indicates a true pigment-deposit, for the white patches are due to the absence of morbid pigment, just as *leucopathia* in the negro indicates the absence of normal pigment. Now, as the pigment-deposit in these cases of melasma is due to morbid excitation of a normal but suppressed function of the rete mucosum in whites, the white mottling indicates a morbid condition of the skin at the places where the skin or the hairs remain white, when all around is darkened. This morbid condition can often be traced to some eruptive disease of the skin by which the function of the rete mucosum, so far as the production of pigment is involved, is interrupted. In other cases it is due to some other cause or causes. I have seen, for example, a case of syphilis, in which the hairs of the entire surface of the body, *cap-à-pie*, fell off, and pigmentation therewith ceased. The patient was a young man of dark complexion, and when the colourless downy hairs reappeared on his pink-looking skin, he presented a curious contrast with his former appearance. Again, although leucopathia may be thus traced to changes in the disease induced by locally inflammatory or constitutional causes, and the result of which is to interrupt the chromatogenous function of that portion of the skin affected, there are forms of leucopathia wholly unconnected with any such structural changes, and which are probably due to changes in the innervation. Thus, a man with renal and cardiac dropsy had broad patches of liver colour (melasma) and leucopathia on his right forearm. No structural change could be traced in the site of the latter, while it was observable that the hairs of the spot were white, so that the production of pigment was suppressed in them, as well as in the rete mucosum. Such cases as these I would term melasmic leucopathia.

On the other hand, it is to be noticed that portions of the skin which have been the seat of considerable structural change, are the seat also of pigment-deposit. The deep cicatrices left by a severe attack of small-pox may be observed sometimes to be thus coloured; it is no uncommon thing to observe the same appearance in the cicatrices of old ulcers on the legs, especially of gouty old people. In these cases the pigment-deposit is manifestly due to another kind of action than that which occurs in the rete mucosum or cutaneous glands in instances of melasma. In the latter, it is a natural function of normal structures exalted by some special conditions; in the former, the normal structures are destroyed, and the process is purely morbid. These two forms of cutaneous pigmentation are in truth typical forms, and correspond to what may be termed carbonaceous excretion and carbonaceous deposit (melanosis proper). The black, yellow, and blue stains following upon bruises are, of course, wholly different, being due to the effused blood-corpuscles.

Local causes of Melasma.—The conditions under which morbid pigment-deposit takes place being so very complex, it becomes necessary to determine the more important by illustrative cases. These conditions may be classed under three heads, as they involve the blood, the nervous system, and the tissues themselves. As regards the skin and its appendages, it may be here observed generally and by way of preliminary, that any stimulation or irritation applied to the skin under certain conditions of the blood, the nervous system, or the system generally, will induce pigment-deposit. Thus, heat and light tinge the rete mucosum of certain persons. In old people the shins get to be of a brown or liver-colour, from exposure to the fire (*tâches de brûlure*). The same thing happens in certain cachectic states, as in the syphilitic cachexia, in anæmic conditions (chlorotic girls), and the like. In a similar class of cases the irritation of a blister suffices to cause pigment-deposit, so that the exact size and shape of the blister is indicated by a dusky-brown or liver-coloured patch. Various skin diseases have a similar effect, as the furunculoïd, eczema, psoriasis, &c. In an inveterate case of psoriasis, treated in the clinical wards during the session 1859–60, in a man aged about fifty-eight, after convalescence the entire surface of the back and trunk was almost wholly covered with large confluent patches of dark pigment-deposit, indicating the portions of the skin which had been the seat of the disease. And seeing this result, it was not difficult to understand how in cases of this kind the complexion of the individual might be indelibly changed, so that he would exactly resemble men of the coloured races, in that particular at least, just as in some cases of leucopathia in negroes, the skin has become as white as in the white races. Now, in these cases of cutaneous pigmentary change, after convalescence from skin diseases, I have observed that the patient has been either advanced in years or cachectic (if young), or both aged and cachectic.

Psoriasis, for example, is very common in young and healthy adults, but in these cases stains are rarely left after cure, as in the aged and cachectic. It occurs in the exceptions, I think, because the patient is of a dark complexion, and is therefore predisposed naturally or normally to pigment-deposit, just as a woman of such a complexion will have a darker mammary areola from pregnancy than a fair-complexioned woman; and this indicates one of the most common conditions under which pigment-deposit will vary in intensity—namely, the ethnic, or conditions proper to the race. Probably to this class of conditions may be attributed in part the difference between swarthy and yellow bronzings in the diseases of Europeans.

The mode in which the sun's rays excites freckles (*ephelis* or *lentigo*) shows, however, that there are conditions in the skin of the face and neck which predispose it to the development of pigment in the small patches termed freckles, otherwise the change in colour would be uniform. Sometimes this appearance is very remarkable, as in a youth of twelve just arrived from India, who came under my notice, and whose face, after exposure to the sun, was suddenly dotted uniformly over with round brown spots of the size of a large pin's head. In these cases the deposit may be due to irritation of the rete mucosum at points where a group of sudoriparous or sebaceous glands pour out their products, or if not to this, to the same class of elective causes, by virtue of which herpes, variola, and other circumscribed inflammations of the skin, are induced. It is to be observed, too, that red-haired persons are more liable to this lenticular pigment-deposit (*tâches de rousseurs*) than the dark-haired, in whom the pigmentation is more uniform. I shall, however, refer to these special forms of pigmentation or leucopathia when I discuss more particularly the causes of melasma, and with these preliminary remarks will now give some illustrative cases.

Cachectic melasma (i.e., *swarthy bronzing*, as distinguished from *yellow*) is the most commonly observed; it is that specially designated by the term *melasma.*

CASE I.—*Mottled swarthy bronzing (melasmic leucopathia) of thorax and abdomen mistaken for sordes; abdominal tumour; enlarged lymphatic glands.*—Reid, a tailor, aged about thirty-five, was admitted into the clinical wards 27th May, 1856. On admission it was found he had a pulsating tumour about four inches in diameter, occupying the middle line at the epigastrium, and extending thence into the left hypochondrium. In addition to the tumour proper, there was a supplementary enlargement caused by fæcal accumulation at the left angle of the colon. Complained of constant constipation and severe shooting pains in the back and loins, extending to the left groin. Pain also more particularly referred to the region of the left kidney, where there was tenderness on pressure. On careful examination, the tumour was found to be not expansile, but a loud systolic murmur was heard over it. The skin of the trunk was universally of a darker hue than natural, but more particularly over the abdomen; the dark surface was mottled with white spots, which, on inquiry, were found to be the sites of a previous pustular eruption. Face pallid and anæmic; axillary and inguinal glands enlarged. In discussing this case at the bedside, I pointed out the bronzing of the skin, but the appearance was so like that of sordes, that several of the class got soap and water, in the confident expectation they could "wash the Ethiop white." The only result was to bring out the dark and white spots more distinctly.

This patient was so much relieved by the use of purgatives and anodynes, together with a good diet and the iodide of iron, that he insisted upon leaving the infirmary, so that the termination of the case could not be observed. It was one of a "dyscrasic" character, as indicated by the anæmia and enlarged lymphatic glands. Probably there was also disease of the glands within the abdomen, and visceral disease of a complex character. It is cases of this class that are not uncommonly associated with melasma and melanosis. The connexion of the bronzing with abdominal disease is a point not to be overlooked.

Symmetrical Melasma.—In the case just detailed, the change in colour was limited to the trunk: it is sometimes, however, symmetrical, both as to morbid whiteness and morbid dinginess; and although it by no means follows that, being symmetrical, it must be due to changes in the centric nervous system, yet in the absence of any other cause we may fairly refer it to these, as they probably were influential in the following case.

CASE II.—*Long-continued diathetic eczema and erythema; enlargement of lymphatic glands; symmetrical pigment-deposit on the skin; melasmic leucopathia.*—P—— S——, aged sixty-two, a workman in an iron foundry near Glasgow, admitted into the clinical wards 23rd

July, 1860. He states that he had always enjoyed good health, with the exception that in youth he had frequent attacks of headache until sixteen years ago, when his legs became red and swollen and covered with small vesicles, accompanied with excessive itching. In three or four weeks he was cured by the application of a yellow ointment, but the affection recurred annually every spring, and at each recurrence extended over a wider surface. About four years ago, the shoulders became affected, and for the last two or three years there has been no remission. The itching is very distressing, and the desquamation from the parts affected considerable.

About three years ago he observed that the glands of the groins were becoming large, and shortly after those of the axillæ. About this time, also, three abscesses formed; one in the right groin was opened, two others lower down in the right thigh burst. They commenced with shivering, thirst, and other febrile symptoms. Two years ago, while feeling a little weak, small brown spots began to show themselves on various parts of the skin of the groins, back, and abdomen. He cannot remember whether one patch preceded another, or whether they all appeared at the same time. He has never had rheumatic fever, but when forty years of age he suffered from three or four attacks of lumbago. His father was rheumatic, his mother not.

Examining the patient more particularly, it was found that he complained of no pain, only of a troublesome itching extending over the whole body. The skin generally is of a pinkish-red colour where not discoloured, showing a deficiency of pigment, gives off abundant scales, and is universally thickened, but more especially so on the legs, where it presents the appearance of ichthyosis. Symmetrical brown patches as large as two hands are observed over each shoulder-blade, and which extend across the median line; another large patch surrounds the neck, and extends symmetrically upwards to the face, which is uniformly dusky, presenting no patches. Symmetrical patches occupy each axilla, extending forwards to each nipple; there is a similar patch around the umbilicus, and also in each groin.

The superficial lymphatics generally are enlarged in the groins, both above and below Poupart's ligament, in the axillæ, at the elbow, and in the neck behind the sterno-mastoid. There is also an enlarged moveable gland over the left tenth rib.

The head is covered with hair, but which is almost uniformly white, contrasting with his dusky face. The facial and other characteristics are those of a degenerate form of the gouty or rheumatic diathesis. The lips do not exhibit any peculiar pallor.

The blood was examined microscopically, and found to contain an abundance of small white corpuscles. (Leucocytosis.)

A cardiac murmur is heard with the first sound, loudest at the apex; pulsation visible in the neck; pulse 69; the appetite is good; the bowels regular; urine abundant, of a light amber colour, with the normal amount of chlorides, and affording no traces of albumen or sugar. As the wards were closed for the autumn shortly after his admission, the termination of the case was not observed.

In this instance the symmetrical deposit of pigment in certain spots of the skin, and its absence in others, is the most striking point. It is also to be classed with Case I. in regard to the affection of the glandular system. The melasma presents resemblances to that observed in some of the recorded cases of bronzed skin. Thus, in No. 6 of Dr. Addison's cases,* the patches of discoloration were associated with patches of leucopathia, in which the skin presented "a singularly white or a blanched appearance;" the hairs upon these patches were also "completely white." The axillæ and groins are also very commonly the seat of discoloration in Addison's disease, and in some of the cases a symmetrical deposit has been observed, as in this. Symmetrical deposit is not confined, however, to these constitutional forms. In chronic peritonitis and chronic structural diseases of the abdominal viscera of all kinds, it is not uncommon to find a darkened hue of the skin of the abdomen, approaching in some cases to a deep tint of Indian ink. In one of this class of cases (evidently cancerous) which Dr. Warburton Begbie kindly brought under my notice at the Edinburgh Royal Infirmary, there was not only this pigment-deposit in the skin of the abdomen, but it extended symmetrically downwards along the groins to each thigh.

Desquamative Melasma.—The desquamation in Case II. was not pigmented. There are cases, however, in which the scales either contain or excrete abundant pigment. Schilling† mentions the case of an infant with previous "hyperæmia" of the skin, whose entire surface was covered with a lamp-black matter, which came off with the linen. The following presents one of these forms of pigment-deposit.

CASE III.—*Nigrities of trunk with extensive desquamation* (pityriasis versicolor); *eczema; yellow bronzing of face; profound anæmia.*—Elizabeth Fleming, aged sixty-six, admitted into the clinical wards May, 1856. On admission, physical signs of bronchitis with slight consolidation of lung, blowing systolic murmur loudest at the base, œdema of legs, recurrent headaches, and attacks of diarrhœa.

On the surface of the body were the remains of an eczematous eruption, the lips very pale, face and hands strikingly anæmic but of a pale straw colour (yellow bronzing), contrasting strongly with the hue of the arms, trunk, and extremities. These were of various shades of bronze and black; bronze on the forearms and legs, but deepening upwards towards the trunk, which was very dark, and, indeed, almost black in the axillæ and over the abdomen. As she recovered her health under chalybeates, wine, and generous diet, desquamation came on, and the epidermis came off in dark flakes, leaving the skin paler, but of its natural hue. No sporules of the pityriasis fungus (the microscoporon furfur) could be detected. The skin over the abdomen and other pigmented parts was mottled over with pale spots, which had evidently been the seat of the eczematous eruption. This was a second attack of the kind, for the patient stated that five years pre-

* On the Constitutional and Local Effects of Diseases of the Supra-renal Capsules, p. 25.
† De Melanosi, p. 32. 1831.

viously she was similarly affected, and that it then ended in desquamation.

This case presents an obvious point of resemblance to the two preceding in the circumstance that leucopathia, from the inflammatory affection of the skin, was associated with the nigrities. In other words, that some morbid condition of the skin caused a deposit in one portion of it, while another morbid condition prevented the deposit. In the blood-condition there was also a point of resemblance.

Visceral Melasma.—The cutaneous pigmentation observed in Addison's disease has been termed *melasma supra-renale.* This term is premature and objectionable, because it assumes the accuracy of the theory which connects the cutaneous pigment-deposit necessarily with structural diseases of the supra-renal capsules. In some of the cases there is melanosis as well as melasma, and these two pathological changes are, as we have seen, very distinct in their nature. The general symptoms are anæmia, gastric and intestinal irritation, and various neuroses, as neuralgic pains, convulsive movements, and mental despondency. The following case presented these, together with melasma.

CASE IV.—*Addison's disease; intense swarthy bronzing; recovery.*—Stevenson, aged twenty-nine, a ship-carpenter, from North Shields, was admitted into ward No. 1, on the 18th June, 1856. Naturally of a dark complexion, he presented nothing particular in this respect until the month of August previously, when, without any serious ailment, he began to be dark and yellowish in colour, and was treated for jaundice, without any alteration in his complexion. Feeling pretty well, he engaged as a ship-carpenter for a voyage, but had hardly been four days at sea before he was seized with most violent vomiting and purging, accompanied with a feeling of great uneasiness in the lumbar and right iliac regions. He returned from London (where he was treated) to North Shields, and had another attack on the way. Six weeks before admission he again engaged to serve on board ship, but the first night he was on board he was seized with a rigor, which continued a quarter of an hour, and was succeeded by a cold sweat. During this he became unconscious, and was carried ashore in that state. Again, on June 7th, he was seized with violent vomiting that continued four hours, and with purging and abdominal pain, especially in the right iliac region, with great weakness, headache, and total loathing of food. Has had *seven* such attacks. His complexion is absolutely that of a mulatto, so that on admission he was mistaken for a man of colour. His hands and feet are brown, like his face; dark patches in axillæ and groins; conjunctivæ and nails white; the mucous membrane of lips and mouth patched with dark inky spots; the same spots seen on the side of the tongue. Tongue clean; bowels costive, moved once in four or five days; pulse 80, feeble; cardiac impulse feeble, and sounds very faint. In left lung posteriorly crepitation. Hands very cold and moist; feet cold. Complains of pain in the spine at the level of the crest of the ilium. Under the microscope blood showed abundance of small white corpuscles. He was ordered the persesquinitrate of iron, wine, and a generous diet, but diarrhœa came on and he lost strength and weight, so that on 1st July he was reported to have lost one pound and a half during the preceding week. On that day he was ordered full doses of glycerine daily, in addition to the other remedies. He now began to improve rapidly in both strength and weight, so that by the 14th July he was so much better in every respect, except his complexion, that he went home convalescent.

This case was in all particulars so similar to the forms of melasma accompanied with anæmia which Dr. Addison has described, that I have no difficulty in classing it with them. Of course it may be objected, that there is no proof there was disease of the supra-renal capsules, because there was no post-mortem examination to determine the fact, and that as all true examples of the disease have always been fatal, the simple fact of recovery is opposed to the diagnosis. I cannot admit, however, that every case presenting the group of symptoms known as "Addison's disease" must necessarily end fatally, or that the concurrent disease of the capsules is necessarily structural; nor is it a legitimate exercise of the art to establish no diagnosis except by examination after death. The case presented all the pathognomonic symptoms of Addison's disease, and ought therefore to be placed as such nosologically.

There is one point in the history of the melasma in these cases of considerable clinical importance. The morbid conditions which lead to the pigment-deposit may be removed, and yet the deposit itself may remain; or, in other words, the primary disease may be curable, but the melasma be permanent or incurable. A patient (Lawson), aged fifty-four, was admitted into clinical ward No. 1, under my care, in profound anæmia, apparently the result of starvation. His colour was universally of the swarthy hue of a mulatto, but deepest in the face. He spoke with a strongly marked Scotch accent, or he might have passed for a Lascar just arrived. To all inquiries as to his colour, he only answered that his mother told him he was white until he was four years old, when his skin changed to dark after an illness. It is possible that the maternal history may have been a myth, for Le Cat quotes a case in which the blackness of the child of a lady was attributed to her having *seen* a negro. A case related by Dr. Parkes of this kind is, however, conclusive:—A man, aged fifty-nine, was treated in the University College Hospital for jaundice. He left the hospital apparently well; but four or five months afterwards portions of his skin became gradually dark—first on the face and neck, then in patches on the body, arms, and thighs, until the hue was very deep. For five years afterwards no change in tint took place, when he was again admitted into the hospital (seven years after the attack of jaundice), with ascites dependent on contracted liver. At this time he was like a mulatto, there being only slight variations of tint on the face, neck, shoulders, and arms; but over the trunk, and especially the abdomen, thighs, and scrotum, there were white patches (melasmic leucopathia); below the knees the skin was of its natural tint. There was a little pigment on the conjunctiva, and a dark patch on the mucous membrane of the lips. No excess of white corpuscles or free pigment were detected in the blood. After death, pigment-deposit was found beneath the epithelium of the peritoneum (melanosis). The supra-renal capsules were found to be perfectly healthy.

Now, in this case the pigmentary changes were consequent upon the attack of illness which occurred seven years before death. If they depended upon supra-renal disease (which may have been the case), then that had evidently been of a curable character, and the organs had been restored to a healthy condition. In any case the melasma and pigment-deposit were exactly like what occurs in "Addison's disease," as Dr. Parkes very truly observes.* We may therefore conclude from these examples (and others might be added) that melasma may occur in whites as a permanent coloration of the skin, resembling that of the coloured races in all essential particulars; and although the result of disease, is not itself a morbid state.

Yellow bronzing.—I have hitherto described more especially the cases of morbid pigmentation in which the discoloration was swarthy or dark. In Case III., however, although swarthy on the trunk, it was yellow on the face. As these cases of yellow bronzing are little understood, yet more common even than the swarthy, the discoloration requires a more particular investigation than it has had hitherto. For example, in all the recorded cases of "Addison's disease" the swarthy tint has, I believe, been exclusively looked for and described, so that examples of supra-renal disease in which the discoloration was yellow have been recorded as instances in which there was no pigmentation whatever. That yellow discoloration of the skin is as possible an occurrence as black discoloration in diseases of the white races is clear from the simple fact, that the races of mankind of a yellow colour, or of yellow shaded with brown, are as numerous as the swarthy or dark races. The complexions of the numerous and widely-spread Mongolian race and of its various offshoots and branches have yellow as a basis. When, however, such yellow tint (known as *sallowness*) has been observed in various morbid states—as icterus, hepatic and splenic diseases, chlorosis, scorbutus, cancer, tertiary syphilis, and the like, the change has not been connected with morbid pigment in the epidermic cells, but exclusively with morbid states of the blood. Now, that it is often due to these is certain; but there are numerous facts which incontrovertibly show that this doctrine is wholly insufficient to explain all the phenomena of the discoloration, even in those of a pure case of icterus, when bile can be detected in the blood.

Before we can form a correct diagnosis in the class of cases in which there is a morbid deposit of a yellow or xanthous pigment analogous to the swarthy, we must examine more carefully the order of the symptoms upon which our diagnosis should be founded. Ordinary swarthy bronzing (as in "Addison's disease") is easily mistaken for the tint of biliary disorder by the inexperienced eye; now, the condition of the blood in the vessels is our guide in these cases. If the con-

* Medical Times and Gazette, Dec. 11th, 1858.

junctiva be white and pearly, the nails white, and the mucous membranes very pale, it is inferred that the discoloration is not due to biliary disease, because bile-tinted blood gives a yellowish tint to the conjunctiva, nails, and mucous surfaces. On the other hand, a careful examination of the discoloration shows that the colour is not in truth that of diluted bile, being too dark, but rather of diluted Indian ink. Cases of *melasicterus* may, however, present this swarthy complexion; but for the most part the diagnosis of the tint in "Addison's disease" from that of hepatic disease may thus be made. Now, it is obvious the diagnosis of yellow bronzing must be made on the same principles, but more weight is necessarily given to the pallor of the conjunctiva than in swarthy bronzing, and less to the differences of tint, because the sallowness of pigmentation differs little from the sallowness of the icteric tint.

I would not, however, be understood to regard the colour of the conjunctiva as an absolute guide in diagnosis, for it is also pigmented in the coloured races as well as the skin, and therefore there may be pigment-deposit in it in disease; and we have seen that this occurred in Dr. Parkes's case, before quoted. It is not difficult to understand how this may be. The conjunctiva contains all the elements of skin, for the eye itself, *qua* development, is a skin-product. Hence, not only pigment may be deposited therein, but hairs may grow from it. My friend, Mr. A. M. Edwards, lecturer on surgery in Edinburgh, showed me an interesting illustration of this fact in a portion of conjunctiva with the hairs attached, which he had removed from the eye of a girl, aged eighteen, in whom the deformity was congenital.* These observations equally apply to the colour of the buccal mucous membrane and of the tongue, as both these surfaces are normally the seat of swarthy pigment-deposit in lower animals. It may be objected, that the presence of bile in the urine, its absence from the fæces, and other circumstances, may serve equally well, as the colour of the surfaces indicated to diagnose morbid pigmentation from biliary blood; but in practice it happens that the most difficult cases for diagnosis of this kind are just those in which there is simply a bilious tint without any important or noteworthy change in the secretions.

Yellow bronzing may pass into swarthy bronzing, or be conjoined with it as in Case III., or it may be symmetrical. Le Cat† quotes cases of this kind from the 'Journal Encyclopédique,' for March, 1764. A man had an apoplectic attack after being very angry, which ended in hemiplegia of the right side; at the same time this side of the body became completely yellow, without excepting the right half of the nose. Another man, addicted to drunkenness, experienced a painful feeling of numbness on the right side; when it passed off the face became green, the right side black, the left side yellow. The urine was sometimes black, sometimes green. Having taken salt of wormwood (carbonate of potash) for several days, the colour changed, the face and the right side became yellow, the left side black. In

* An account of the case, and reference to others, may be found in the October number (1860) of the Edinburgh Veterinary Review.

† Traité de la Couleur de la Peau Humaine, p. 168 et seq.

about twenty days a yellow tint only remained, and this gradually disappeared. These are not by any means solitary cases, but it is unnecessary to multiply examples. The following illustrates the occurrence of yellow bronzing in a case of supra-renal disease, expressly recorded by the reporter as being opposed to the conclusions of Dr. Addison.

CASE V.—*Yellow bronzing; disease of supra-renal capsules; multiple cancer.*—Leclerc, a labourer, was admitted into the Hospital Beaujon, April 3rd, 1857, under M. Gubler. His previous history could be ascertained with difficulty on account of his remarkable "insouciance," but two months previously he was attacked with a painful feeling of distension of the abdomen, loss of appetite, occasional vomitings, and habitual constipation. He became pale, and thin, and weak, the abdominal pain increased in violence, recurring in paroxysms, and for a fortnight before his admission he had been confined to bed.

On his admission, the most striking characteristics were his extreme thinness, and the remarkable pallor of the surface, which also presented in a remarkable degree the yellow straw tint (*teinte jaune paille*) of the "cancerous cachexia." No icterus, the sclerotics being of a very pure white, no trace of bronzed discoloration or vitiligo; bellows murmur in the neck; cough, with expectoration of greyish sputa; abdominal symptoms these of cancerous disease; urine of an ochre yellow, and containing uric acid, no albumen nor sugar.

As the disease advanced the yellow tint became deeper and deeper, the epigastric pain more intense and constant, and extending thence to the base of the thorax and the lumbar region. A little to the left, and below the umbilicus, two tumours appeared which were the seat of a powerful impulse and loud murmur. The patient died twenty days after admission.

The post-mortem examination showed that although the liver was very voluminous, and the yellow substance hypertrophied, bile had been freely secreted, for there was abundance of it in the biliary canals; the gall-bladder was large and filled with greenish bile; and although the common duct was surrounded by cancerous masses, its calibre was rather increased than diminished. The stomach, pancreas, and supra-renal capsules were the seat of cancer, as were also the mesenteric and dorso-lumbar glands. The spleen healthy and small; the kidneys large, anæmic, but not cancerous. No examination of the blood.*

In this case the post-mortem condition of the liver and bile-ducts was conclusive as to the correctness of the diagnosis during life—namely, that as the sclerotic was of a pure white, there was no icterus. The yellow bronzing could not be due, therefore, to the presence of bile in the blood, while the extreme pallor showed it was not owing to change of colour in the blood-corpuscles. Hence the only remaining conclusion that it was caused by pigment-deposit.

The examples of yellow bronzing are so common in practice that I need only leave it for further and closer observation, as a true form of morbid pigmentary change. But I may remark, by way of caution,

* M. Ferréol: Gazette Hebdomadaire. Aug. 1857.

that while the colouring matter of the bile will give a yellow tint, it must not be forgotten that a thin layer of blood-corpuscles separated upon the derma, will also simulate yellow bronzing. The most familiar illustration of this is to be found in bruises or in scorbutus; but it also probably takes place in yellow fever, and, perhaps, other forms of fever—not, however, to the exclusion of biliverdin.

Amongst the most interesting forms of morbid pigmentary changes, are those which are associated with emotional states of the nervous system, or with peculiar conditions of the genito-urinary organs. They may be termed the *neurose* forms, but as the entire group requires special and detailed examination, I shall reserve the consideration of it for a separate paper.

(To be concluded in our next.)

ART. II.

A Record of Thirty-two Cases of Pneumonia. By HANDFIELD JONES, M.B., F.R.S., Physician to St. Mary's Hospital.

THE value of this communication, if it has any, consists in its being a faithful record of *all* the cases of pneumonia which have been specially observed by the author. Most of them have been under his own care (chiefly as externs); those that have not are distinguished by an asterisk. No selection has been made of the cases; they have been taken just as they happened to present themselves. The high rate of mortality is partly to be accounted for by the severity of some of the cases, and partly, it may be, by the author's not having been sufficiently aware at the time of the danger of antimony prostrating the system and not the disease. The results are simply stated with short comments, and the deductions that seem to be warranted.

CASE 1.—*History:* S. D., aged six, female; seen Feb. 7th. Symptoms of catarrh about fourteen days, of more serious chest affection last six. Signs of hepatization of upper part of left lung. Pulse 120; resp. 57. Treated by hydr. c. creta, gr. ij. Pulv. ipec. gr. j. ter die. Hs. salin. c. pot. nitr. and tr. conii, quartis horis. Blister to left front. After five days mercurial at night only, mixture continued. On eighth day mercurial omitted. Liq. cinch. On twelfth day quite convalescent, signs of pneumonia disappeared, breathing almost quite normal.

CASE 2.—*History:* G. B., aged one and a half year, male; admitted May 30th. Ill six days, teething, diarrhœa, signs of moderate bronchopneumonia. Hydr. c. cret. gr. j. Pulv. Dov. gr. j. o. n. Ant. pot. tart. gr. $\frac{1}{4}$. Tr. opii, ♏j. Mist. ammon. acet. ℥j. quartis horis. On second day signs of chest affection increased; pot. nitr. gr. j. added to mixture; ungt. hydr. thoraci. On fourth day a blister; died on fifth.

CASE 3.—*History:* R. A., aged three, male, admitted September 27th; had been ill about eight weeks with inflamed cervical glands; was improving fast, when he was attacked with pneumonia, which went on to hepatization of the greater part of both lungs. This state was nearly or quite a month in undergoing resolution; the report does not state that the normal breathing had returned until exactly a month from the date of the well-marked physical signs. Treatment consisted of Gray and Dover's powder bis die, nitre, ant. pot. tart.

gr. $\frac{1}{6}$, tr. opii tertiis horis, with blister and ungt. hydrarg. By the tenth day there was decided improvement; ol. morrh. was given, and the mixture changed to saline with nitre and squill. On sixteenth day nitric acid, cascarilla, and tr. cinch. were ordered. By the end of fifth week was quite convalescent.

CASE 4.—*History:* E. W., aged two and a half years, female, admitted August 21st, with pyrexia and hepatization of left lung; treated by nitre, ant. pot. tart. gr. $\frac{1}{6}$, liq. opii secundis horis, ungt. hydrarg. to chest; after ten days, ol. morrh., nitre, and gentian, blister to back. Convalescent in about a month. The antimonial mixture was no doubt not given so often after the first two or three days; after a week it was ordered ter die.

CASE 5.*—*History:* T. W., aged forty-three, male, admitted April 2nd, at 8·30 A.M., not fully restored from a state of drowning. At 12 reaction, which was very great at 5 P.M; breathing oppressed, crepitations in both lungs behind, lips blue, tongue dark-coloured, pulse very strong and quick. C.c. ad ℥xii. infra scap., pot. nitr. gr. xv., vin. ant. tart. ʒss., mist. ammon. acet. ℥j. ter die. Was delirious the same night, next day breathing hurried, pulse 132, countenance depressed; 5 P.M.—breathing much worse; at 11 P.M. died.

Post-mortem examination.—Only the upper lobe of each lung remained pervious to air and crepitant, the remaining parts were in a state of gray hepatization. It was matter of general surprise to see this anatomical condition after so short an illness.

CASE 6.—*History:* J. S., aged thirteen months, male; ill thirteen days when seen, April 30th; takes no food; fever; emaciated; much râle and flat percussion sound in back; pulse quick and weak. Quinæ dis. gr. $\frac{1}{2}$, tr. hyoscy. ♏iij. quater die. May 3rd—Resp. 52-60.; no typhoïd spots; pt. c. quin. gr. $\frac{5}{6}$, quater die, pulv. Doveri, gr. j. o. n. 7th.—Improved since 3rd, taking beef tea and milk pretty well. Breathing quieter. 8th.—Sank and died this morning.

Post-mortem examination.—Body pale; heart healthy, decolorized fibrinous coagula in aorta and pulmonary artery; right lung generally crepitant, but consolidated to some extent posteriorly; left lung had the pleura roughened by a thin fibrinous exudation, upper lobe in two-thirds of its extent completely consolidated, of dull white colour, lower lobe semi-consolidated posteriorly. Lower part of ileum quite healthy.

CASE 7.—*History:* E. S., aged five, female, admitted September 20th; skin burning hot, cough, tongue coated, crepitant râle of pneumonia in both sides of the back. Ant. pot. tart. gr. $\frac{1}{4}$, liq. opii sedat. ♏ij., aq. anisi, ℥ss., ter die. 24th. —Pneumonia proceeding, pt. in mixt. quater die. Hydr. c. creta, gr. ij. pulv. Doveri, gr. j. bis die. 27th.—Less thirsty, tongue cleaning, bowels not open. Absolute dulness in left back almost up to top; right is tolerably resonant. In left back no sound in ordinary respiration, except an occasional slight whiff with inspiration, and a faint soupçon of a crackle; in the right back respiration is rather coarse, and is defective; no notable dyspnœa; skin cool; pulse steady; pt. in pulv. ter die; inf. gent. co. ℥ss., pot. nitr. gr. iv., tr. hyoscy. ♏v. ter die. October 1st.—Ol. morrh. ʒij. bis die. Pt. in mist., ungt. hydr. lat. sinistr. 8th.—Better, much. 15th.—Better, convalescent, takes food well. Resonance in left back improved, but not as good as in the right. Tolerably good breathing in left back, full and natural in right. Pt. in usu ol. and ungt., vini ferri, ʒss ter die. She went on improving, and was discharged in excellent health on Nov. 14th, but there still remained some dulness at the left posterior base, deficiency of vesicular sound, and the peculiar sound on deep inspiration of adherent air cells opening up and expanding.

CASE 8.—*History:* G. L., aged fifty-three, male, admitted March 22nd; ill one week; has short cough; thick, rather red expectoration; "urine like blood;" pulse weak; fine pneumonic crepitation at lower part of left back, with bronchial breathing above; right lung normal. Ant. pot. tart. gr. ½, liq. opii ♏iv., mist. ammon. ac. ℥i., quater die. 23rd.—Respiration 30; expectoration mucous and bloody; pulse 100; skin rather burning. Pt. in mist. quinquies in die. 24th.—Pulse 92, small, quiet; skin cool; tongue clean; breathing much easier; respiration 28; sputa rusty; left back dull—moist, fine crepitations heard all over it, except at top, where the sound is bronchial. Pt. in mist. quater die. 27th.—Pulse 72; respiration 23; skin cool; left back much more resonant; breath sounds as before. Ol. morrh. ʒij. ter die, nitre, squill, inf. calumb., ter die. The state of the lung still further improved, and he was soon well.

CASE 9.—*History:* J. F., aged twenty-seven, male, admitted March 9th; ill five days; has cough and bloody expectoration; lips livid; the signs of bronchopneumonia were evident. Ant. pot. tart. gr. ½, liq. op. sed. ♏v., inf. calumb. ʒi. tertiis horis. Turpentine stupes. 10th.—Much relieved; very little nausea; perspiring copiously; urine loaded with lithates; respiration 46; pulse 100. Both lungs involved extensively; the right hepatized posteriorly, the left less affected; general bronchial râle. 12th.—Pt. c. ant. pot. tart. gr. i. in mist.; pulse 93; respiration 40. 13th.—Pulse 75; respiration 27; left lung nearly quite free; right in full course of resolution. 14th.—Pt. in mist. quater die. 16th.—Continual nausea; omit mist., calumb. and nitre. 18th.—Both lungs quite free. 26th.—Quite well. This case is published in detail in the 'British Medical Journal,' May 16th, 1857.

CASE 10.—*History:* S. B., aged sixteen, female, admitted March 16th; ill seven days; has pyrexia and severe cough; signs of general bronchitis marked with those of pneumonic consolidation of lower and posterior part of left lung; always weakly since variola at the age of three years. Ant. pot. tart. gr. ½, liq. opii ♏iv., m. camph. ℥i. secundis horis ad vices iv., postea tertiis horis. Turpentine stupes. 18th.—Improved. Ant. pot. tart. gr. ¼; pot. bicarb. ℈i., tinct. conii ♏x., mist. ammon. ac. ℥i., tertiis horis. Blister to left side. 23rd.—Much improved; quite natural breathing all through both lungs, except in left lower lateral region, where there is still dulness and deficient breath-sound. Ol. morrh., calumb., and nitre. 30th.—Quite convalescent, some dulness still remaining in left side and lower back. (In this case there was no doubt some pleural effusion as well as pneumonia.)

CASE 11.—*History:* M. S., aged two, female, admitted Oct. 12th; ill two days; pyrexia; screams much at night; tongue coated; breathing good in left side of the back, harshish and with râles in right, where there is some dulness. Mist. pot. tart. ℥iss., pot. bicarb. ℈ij., ant. pot. tart. gr. iss., liq. opii, ♏xx. ʒi. quater die. 15th.—In right back dulness, deficient breathing, and much moist râle, with harsh expiratory sound. Pt. c. ant. pot. tart., gr. ij. ad ℥iss. 19th.—Much better; quiet breathing in both sides of the back. Saline and nitre; ol. morrh. Ceased attendance after this.

CASE 12.—*History:* W. S., aged eight, male, admitted Aug. 28th; ill one week; healthy before; pyrexia; pulse 100, thrilling; left upper front of chest dull to percussion; breathing bronchial and non-vesicular, with a few moist râles; healthy breathing in the corresponding side; urine high-coloured; saline with vin. ipecac., acid. hydrocy. dil., alkali, and liq. opii quater die. Hirudines ij. infra clavic. sinistr., ungt. hydr. postea infricand. Sept. 1st.—Is very much better; hepatization resolving. 3rd.—Left front quite resonant; breath sound, weak, but otherwise healthy; iron and quinine. 14th.—Quite well.

CASE 13.—*History:* M. A. D., aged twenty-two, female, admitted Feb. 11th; has pain in left side, shortness of breath, some cough and expectoration; tongue clean; skin cool; pulse 140, weak; urine rather pale; good breathing in right side of the back; not so free in right front; in lower half of left side of the back there is absence of vesicular murmur, some fine crepitation, bronchial breathing, and dulness; in upper third there is resonance; in left front, about third rib, there is distinct pleuritic friction. Ant. pot. tart., gr. $\frac{1}{3}$, liq. opii ♏v., mist. camph. ℥ss., om. horâ, ad vices iv., postea secundis horis. Turpentine stupes. 13th.—Pulse 114, soft; has less pain in side; breathing easier; distinct œgophony and whiffy bronchophony, with complete dulness in lower half of left side of the back; decided dulness in left front up to clavicle; breathing audible here, but not so free as in the right front; heart not displaced. Pt. in mist. quater die; pil. hydrarg. gr. ij., calomel, gr. i., opii, gr. $\frac{1}{2}$, quater die. 18th.—Omitt. mist. nitre and nitric ether; blister. 22nd.—Some ptyalism; pulse 106; scrubbing pleural friction heard all over left back, side, and front; can feel a dragging sensation herself; breathing only in the upper part of lung. Pt. in pil. and mist. ter. die. Blister. March 4th.—Medicines taken regularly till 2nd; is very much better; mouth but little affected; very faint breath-sound is now audible even below the middle of the left side of the back, attended with friction. Pt. in pil. om. nocte. Pot. iod., tinct. cinch., inf. genti. co. 11th.—Copious expectoration of yellowish matter; breathing in left back more full and clear. Pt. in mist., tinct. iodinii lat. sinist. April 8th.—Discharged well, having had ol. morrh. and iron and quinine for a fortnight.

CASE 14.*—*History:* A. R., aged twenty-six, female, admitted Jan. 22nd. Subject to rheumatism last nine years every winter. Since rheumatic fever nine years ago has had palpitation more or less always, but not to such an extent as to cause her alarm till three months ago. Action of heart very tumultuous, complains of constant pain in the region of the heart, of dull, wearing character, increased when palpitation comes on, which it does several times a day, more especially on the slightest exertion. Breathing natural; bowels costive; has pain in knees, increased at night; catamenia irregular. Feb. 11th.—Thyroid gland enlarged equably, it is less than it has been; it first enlarged six years ago; proptosis well marked; pulse very small; heart beats with considerable violence; præcordial dulness extended chiefly towards right; complains still of pain; gets little sleep. Feb. 15th.—A blowing sound is heard all over the heart, and a distinct rough systolic bruit in the second intercostal space, just to the left of the sternum, where a thrill is also felt; the systolic bruit is heard in the right carotid; health greatly improved since admission. 22nd.—Pain very severe last night, and palpitation increased; kept her awake all night. March 4th.—Pain not nearly so severe as formerly, and referred entirely to one spot near the apex; less proptosis; goître decreasing. 12th—A severe attack of pain and palpitation last night, relieved by empl. bellad. cum opio. 18th.—Much improved. 19th.—About midday became worse, with increase of pain and severe vomiting; the latter was allayed by acid. hydrocy. dil. and digitalis in effervescing saline, but the pain continued very severe till eight P.M., when eight leeches were applied to the præcordia and gave much relief, but death ensued the following morning about seven. She was leeched over the heart Jan. 25th, and had three blisters to the same part at different times with advantage. She took internally small doses of tinct. digitalis, spt. ammon. fœtid., spt. ammon. co., and tinct. ferri ammoniat. Pulse was generally about 120.

Post-mortem Examination.—Bronchial and cervical lymphatic glands enlarged; supra-renal capsules healthy; liver, spleen, and kidneys normal; a very large thymus gland (weighing 1200 grains troy) overlapped the pericardium and lay upon the aorta and pulmonary artery, reaching down to root of left lung;

both lungs in a state of pneumonic engorgement, the left of gray hepatization posteriorly; pericardium normal; heart normal, its valves effective, some vegetations on the mitral, the muscular tissue of good colour; thyroïd gland much enlarged; brain healthy.

Case 15.—*History:* K., aged eight months, male, seen June 6th. Gums lanced over upper median incisors; slight symptoms of bronchitis; saline. 7th.—Bronchitis set in severely; ant. pot. tart. gr. $\frac{1}{8}$ secundis vel tertiis horis. 9th.—State same; dose of antimony diminished to about gr. $\frac{1}{10}$, and of opium increased; sinking came on, and the child died at five P.M. In the morning the pulse had been 138; skin hot; respiration 47; the sides of the chest collapsing in inspiration. The antimonial produced vomiting and purging.

Case 16.—*History:* Mr. Q., aged thirty-five (about), an ailing man, not at all intemperate, seen May 16th in consultation. He had been attacked with pain in left side on night of 11th; the next day there were signs of pleuropneumonia; venesection to ℥viij., saline and antimony, and afterwards calomel and opium, but scarce any ptyalism was produced. In course of 16th delirium came on; at nine P.M., when I saw him, his pulse was 120, soft and bounding; his hands were sweating, but his abdomen was hot and dry; tongue dryish in middle, red; urine most highly lateritious; no articular affection, but a decided rheumatic odour; no signs of heart disease; no fever spots; bowels inclined to diarrhœa; respiration 102. He had had very little sleep for several nights; the right side of the back was quite resonant, but there was deficient respiratory murmur, and semi-moist expiratory râle extensively diffused; left back and side more dull than right, the air, however, entered pretty freely, producing extensively-diffused small, moist crepitus through the posterior and lateral parts of the lung; a severe cough he had had was much diminished since a blister to the left side. Opii gr. j. omni horâ donec somnus invadeat, ammonia and potash mixture secundis horis, wine ℥iss tertiis horis. 19th.—Slept after seven pills, no delirium, pulse much less frequent, urine clear; a little antimony added to the mixture. 26th.—Pulse 70, lung recovering, and all the symptoms amending, except that delirium has continued to show itself till to-day; the cessation seems attributable to leaving off the wine, which was done yesterday, and the delirium immediately ceased. 28th.—Is well.

Case 17.*—*History:* E. B., aged nineteen, male, admitted July 14th. Ill fourteen days, worse seven; pulse 108, weak; respiration 49, short, abdominal; tongue red, and thinly coated; hands not notably hot; urine high-coloured; no fever spots; right lung extensively hepatized posteriorly; left lung working fairly, a trace of râle in back; free breathing in both fronts; heart's sounds normal; sputa sticky and a little rusty; he passed his motions under him, was highly delirious all night, sank and died at half-past one, P.M., of the 15th. He was cupped to ℥ij., and took saline with vin. ant. pot. tart. ♏xxv.

Case 18.*—*History:* J. J., aged thirty-nine, male, of large make, admitted July 7th. Has been a very strong man, had no previous illness; ill three weeks with catarrh, worse last four days, was at work before, diarrhœa last two nights; does not appear much depressed; has marked pneumonia of both lungs, most advanced and extensive in right; sputa tenacious and rusty; respiration 40; pulse 112, very compressible; tongue coated. C.c. ad ℥xvi., ant. pot. tart. gr. $\frac{1}{4}$, pot. nit. gr. x., tinct. opii ♏x., mist. am. ac. ℥j. tertiis horis. Simple diet, beef-tea. 8th.—Is free from pain, lies easy and tranquil; sputa copious, dirty brownish. 9th.—Much pain in right side in "bouts" of numbing character both last night and this morning; tongue red, with yellow coating; feels very low and weak; sweats a good deal, more by day than night; respiration 42, short; pulse 128; hands hot. 10th.—Died this morning.

Post-mortem Examination.—Right lung, upper and middle lobes in a state of grey hepatization; left lung in a state of red hepatization; general recent pericarditis; heart's tissue flabby; valves healthy; kidneys large and heavy, weighing each nine ounces, they did not appear manifestly diseased to the naked eye; liver large.

Case 19.*—*History:* J. C., aged thirty-three, male, admitted July 2nd. Ill six days; had shivering at first, and then severe cough which continues; herpetic eruption on lips, tongue, and backs of hands; no fever spots; bowels much relaxed last three months; eyes bright; tongue moist and clean; no pain in region of cæcum (on pressure); very thirsty; no appetite; respiration 30; pulse 108, soft, undulating; expectorates clear mucus; left back resonant; lower part of right side of the back dull; no breathing audible, but obscure crepitus. C.c. ad ℥vi., hydr. c. cretâ, pulv. Dov. ana gr. iv. bis die, acid. muriat. dil. ♏20 tertiis horis. Beef-tea, milk, and egg, port wine ℥iv. By the 15th convalescence had commenced; he was discharged about the 20th. Medicine omitted on the 10th.

Case 20.*—*History:* J. Mc M., aged thirty-six, male, admitted July 5th. While at work on July 1st was attacked with pain in back towards left side, so severe that he could hardly walk; the same night he brought up a good deal of blood; has had a cough for eighteen months, which is now worse; exceedingly weak since his attack, was very strong before; marked dulness and absence of breathing at lower half of left back; no vesicular or bronchial sound; bronchophony, but no vocal fremitus; healthy breathing in left front, pretty good also in right, but with rhonchi in larger tubes; heart's sounds normal; sputa tenacious and rusty; pulse 93; respiration 39; no fever spots; was delirious last night, did not sleep; no appetite; very thirsty. Was cupped to ℥vi. with great relief to the pain in the side. Vin. ant. ♏xx., tinct. opii ♏v., pot. nitrat. gr. xv., mist. camph. ℥j., tertiis horis, hydr. c. cretâ gr. iv. omni nocte. 9th.—Pulse 76, weak; respiration 34; is very weak; dulness and almost complete absence of breathing in lower part of the left side of the back. 10th.—Omitt. haust. 15th.—Mouth affected, omitt. pulv. 17th.—Pulse 67; respiration 26; left lung not fully resonant posteriorly; breathing rather weak and bronchial. Went out well about ten days later.

Case 21.—*History:* E. D., aged one year, female, admitted December 6th, 1858. Ill five weeks, measles ceased fourteen days; her breath is short; she has no rest; is continually pining and grinding her teeth; bowels open; motions yellow with white lumps; is sick after sucking, takes only the breast; not feverish, but marked flush on right cheek; harsh bronchial breathing, with some crepitation in left side of the back; breathing in right also, harshish, and attended with expiratory sound. Hydr. c. cretâ gr. j., pulv. Doveri gr. $\frac{1}{2}$, ter die. 9th.—Improved; motions much better; breathing in both backs nearly natural. Repeat pulv. omni nocte, ol. morrh. ʒi. ter die. 13th.—Omitt. pulv. 30th.—Is pretty well; no cough; breathing not quite so free in left side of the back as in right, and attended with a trace of râle. Discharged.

Case 22.—*History:* Same child re-admitted on February 28th, 1859. Ill seven days; getting worse; has bad cough, and wheezing at chest; very prolonged, harsh expectoration, with crepitation in both backs; mouth covered with thrush. Gums lanced over prominent molars. Ant. pot. tart. gr. $\frac{1}{6}$, liq. opii ♏iss, mist. pot. tart. ʒj. quater die, lotio boracis. March 3rd.—Both backs resonant, but air enters very feebly and produces numerous small crepitations; frequent cough; not much fever; mouth better. Dec. senegæ ℥ij., spt. ammon. co. ʒiss., mist. ʒj. quater die. 7th.—All fever gone; sleeping much better; tongue very coated. Pt. ol. morrh. ʒj. tertiis die. 10th.—Clear breathing in backs; tongue still much coated; vomits oil; much cough. Mist. pot. tart. ʒi. ter die. 21st.—Better; looks blooming; resumat oleum. 28th.—Well.

Case 23.—*History:* E. L., aged thirty-six, female, admitted September 3rd. Ill two years; has cough; has sometimes spit much blood; much dyspnœa in walking; good resonance in lower halves of both backs; less clear above; respiration pretty free in right back; in left imperfect, and attended with crepitus; breathing weak with some tube sound at left upper back; weak also, with some râle at night; breathing weak in left front, good in right; resonance diminished in left; heart's sounds normal; tongue clean; pulse regular. Saline with pot. acet., squill, vin. ipec. and paregoric, quater die. 6th.—Left back notably duller at lower half than right; lateral region dull also; breathing very weak and imperfect, but without râle in those parts; better in left front, but with some râle; good breathing in right side of the back, but bronchial, and attended with crepitations in right lower lateral region; can only lie on right side; on deep respiration (which is painless) left side moves forty, right twenty; aspect good; lips of good colour; tongue clean; skin cool; no expectoration; no dyspnœa. 9th.—More resonance, and air enters much more freely into left lung at its back part; both lateral regions more equally, about thirty. Soon after this the pneumonic signs disappeared, and she recovered well.

Case 24.—*History:* G. F., aged fifty-five, male, admitted August 15th. Ill seven days; has lost appetite; speech feeble; bad cough; very weak; pulse very feeble; tongue greatly coated; bowels right. He was treated till September 1st with three doses of calomel and opium, nitric acid, quinine, and strychnia, but rather got worse than better. His urine was then found to be very clear, non-albuminous, very acid, and stinking most abominably as of rotten cabbage. His breath had the same putrid odour. On examination of the chest nothing abnormal was detected. He had much cough, which caused him to vomit. His sputa consisted of grey, dirty mucus, devoid of the peculiar offensive smell observed in the breath and urine. Liq. sod. chlorinat. ♏xx. quater die. 6th.—Same state; very great depression. A very careful examination of the chest was made. The left front was quite resonant, even encroaching on the cardiac dulness. The air entered freely into this part of the lung, but the sound was not so purely vesicular as it should have been, and attended with small crepitations; in the apex it was weaker than below. In the left back there were diffused râles with fair breathing. In right lung the resonance and breath-sound was good in front and behind; sputa same; not the least pain in the chest; skin cool; pulse feeble; breath less offensive since he had inhaled chlorine. Morphia $\frac{1}{2}$ gr. omni nocte, pt. brandy 4 oz. 9th.—Nitric acid and cascarilla, tannin with bals. Canad. 10th.—Comparative dulness in lower left back, breathing weak, and attended with in- and expiratory tube-sound; fine crepitation all through left front, and extending to left side; tolerable breathing, with expiratory tube-sound in right front; percussion note woody in both fronts; fetor and sputa as before. 13th.—Brandy ℥vi., ammon. carb. gr. iv., quater die. 16th.—Air enters well into right lung behind, but with some crepitation; air enters pretty well into left lung at lower posterior part, but at upper the breathing is null, and at middle replaced very much by crepitation; air enters freely into right lung anteriorly, but the breath-sound is coarsish. In left front the breathing is very weak and tubular, but not markedly in- and expiratory. Percussion is not dull in left front, but woody, of somewhat better character in right front, woody in both sides of the back; no notable dyspnœa now, or at any time since his admission. Ten oz. of port wine in place of brandy, adde mist. tinct. cinch. ʒi., pil. hydr. gr. iij., opii gr. $\frac{1}{2}$, ter die. 18th.—Death.

Post-mortem Examination on 19th.—Right lung emphysematous, inflamed to some extent posteriorly, with some small patches of pale exudation in it; left lung in a state of grey hepatization in all upper lobe; lower lobe also inflamed highly at posterior part. In upper lobe a very large gangrenous cavity as large as two fists, containing little but blackish, loosely-floating, shreddy pulmonary tissue in a sloughy condition. In the right lung, at upper edge of mid lobe, there was a small round circumscribed cavity of the size of a large marble, in contact by its periphery with the pleura, quite full of a dark matter. On microscopic examination this matter was found to consist of the *débris* of ramifying vessels mingled with abundant dark-red pigment and fatty-looking granules or masses. The lung just around the cavity was condensed for a very limited extent. The large gangrenous cavity in the left upper lobe was separated by a very thin layer of tissue from the pleura, which adhered pretty firmly to the costal layer over a large extent; some small remains of cretaceous tubercle in the right apex; heart and all other organs normal.

CASE 25.—*History:* A. S., aged two, female, admitted Dec. 22nd. Ill three days, had only a little cold before; breath very short, lips of dull red; skin very hot; three canine teeth very prominent (gums lanced), one just through. A very healthy child, is now plump, cut all her teeth well before; bowels relaxed; motions dark brown. No marked diminution of resonance in either back, but vesicular sound everywhere much diminished; in ausculting both backs there is heard very loud, harsh, mostly single inspiratory bronchial breathing. Ant. pot. tart. gr. iij., liq. opii sed. ♏x., aq. ℥iss., ʒj. secundis horis, ad vices iv., postea quartis horis. Nine P.M.—Respiration 82; pulse 126, not strong; skin still hot. 23rd., $10\frac{1}{2}$ A.M.—Respiration 100; much prostration; medicine has been given nearly secundis horis; chest resonant at back. Pt. c. mist. tertiis horis. Died at two P.M.

CASE 26.—*History:* T. H., aged twenty-five, married, admitted Jan. 30th. Ill three days; both legs in pain, from knee to ankles; a pretty marked eruption of purpura on the left, some spots on the right; has eaten potatoes, not much meat lately; tongue coated; is very weak; pulse very feeble; heart normal; bowels open; liver descends very low into abdomen, fully to level of umbilicus; spleen not enlarged; arms and legs stiff at times; urine smoky and highly albuminous; legs have been swelled, but never were till three days ago; very little swelling now. Urine deposits a sediment of casts and tube detritus; the casts are some homogeneous (so-called waxy), some very granular, some contain nuclear corpuscles; there are a few blood-globules. Tinct. ferri mur. ♏xx., quin. disulph. gr. j., aqua ℥j., tertiis horis. Feb. 3rd.—Feels much better; rash less; vomited each dose of the medicine. Repeat mist. quater die. 4th.—Not so well; liver not more than two finger breadths below the ribs; abdomen rather distended; tongue very coated, dry at tip; pulse frequent, large, excited; is quite conscious, but sleeps much. Mist. pot. citr. efferv. tertiis horis. 6th.—Respiration 52; tongue dry; pulse of fair force, 120; bowels much relaxed; good percussion in both backs, but very loud, noisy respiration in left, with good expansion of the pulmonary vesicles; in right there is more of rhonchus, with crepitation, and less expansion sound; the dyspnœa is extreme and increasing; venesection to ℥xv; immediately after, the pulse was 100, and not weaker; the breathing less distressed, 39 in the minute; some scanty, bloody, mucous sputa. Liq. ammon. acet. ʒij., ant. pot. tart. gr. j., tinct. opii ♏v., m. camph. ℥j., statim et post horam j., postea, secundis vel tertiis horis. He took two doses and died at seven P.M., about two hours after the venesection. His breathing continued distressed; he did not appear faint. The doses produced no apparent effect.

CASE 27.—*History:* J. S., aged forty-one, male, admitted Oct. 17th. Ill seven days; feels only exceedingly weak; looks pale; has herpes on lips; very little cough; skin cool; pulse feeble. There is marked impairment of resonance in the lower third of the right side of the back, with fine crepitation on long inspiration; nothing morbid elsewhere. Turpentine stupes; ammon. carb. gr. iv., spt. æth. chl. ♏x., mist. ammon. acet. ℥i., ter vel quater die. 20th.—Feels a wonderful deal better; dulness continues in right side of the back, with deficient and semi-tubular breathing; no dyspnœa; urine free, thick on standing; left leg and thigh much swelled, pitting markedly: no distended or cordy veins to be found in leg. He had an attack of typhus fever twenty-seven years ago; as soon as the fever turned the leg took to swelling, just as on this occasion. His leg was as large as his body for months. 31st.—Doing well under ammonia and bark; the leg continues swollen, but pits scarcely at all; no dilated veins can be found, no swollen glands, or other cause of obstruction to the circulation. Nov. 14th.—Quinine and tinc. ferri mur., ol. morrh. 21st.—Left limb swelled and hot; the swelling subsides at night; air enters freely now into both backs, but causes a slight crepitation at the right base; leg bandaged. He was discharged Feb. 20th, the swelling of the limb having considerably diminished, but still showing towards night.

CASE 28.—*History:* Miss Ed., aged seven, seen April 16th.—Some weeks ago had scarlatina, which was followed by albuminuria and dropsy, which latter yielded kindly. Urine very pale, slightly smoky, albuminous (?); skin hot and moistish; tongue thickly coated at back, red and clean at tip; pulse 135, very compressible; cheeks flushed strongly; sometimes turns pale and dark under the eyes; heart's sounds normal, but lies down without notable dyspnœa; free breathing, with sonorous dry râle in the left back; in right back dulness, absence of vesicular sound, and bronchial breathing in rather more than lower half. Has had lately vin. antimon. in diaphoretic doses; a blister has been applied to the right side. Tinc. f. muriat. ♏v., acid. hydrocy. dil. ♏ij., aquæ ʒij., quartis horis. On the 17th the urine deposited lithates, and was not albuminous; she improved steadily, and was convalescent on the 30th.

CASE 29.—*History:* J. C., aged one year and five months, male, admitted May 14th. Ill three days; breath affected, very short now; hands hot; bowels open; great thirst; pulse very quick, respiration 60; chest covered with a rash much like scarlatina; heart's sounds normal; both sides of the back are not well resonant, breathing laboured, harsh, with diminished vesicular sound; is a very fine, plump child. Vin. ipecac. ʒij. ♏x., pot. nitrat. ʒss., mist. pot. tart. ad. ℥ij. M. ʒj. omni horâ; hirud. ij. dorso. 15th.—Breathing much easier, 44 in minute; skin moist; no vomiting; no rash on body; face and chest this morning as red as a coal of fire, but it soon went off. Leeches bled well and "gave beautiful ease." In both backs there is good, fully natural breathing. Pt. in mist. quater die. 17th.—Appears quite well, except want of appetite. Omitt. mist.

CASE 30.—*History:* H. Ch., aged fifteen months, female, admitted April 19th. Ill five days. Breath very short; looks now *abattu*, pallid; skin hot, dry; pulse excited, very frequent; appetite lost; frequent short cough; bowels open; lips of good colour; both backs dull, with harsh single tubular inspiratory sound. Vin. ant. p. t. ♏ x., mist. am. acet. ʒij., since 18th tertiis horis; cat. lini thoraci; vin. ipecac. ʒij., tr. opii ♏ xvi., mist. am. acet. ℥iij., ʒi. omni horâ. 20th.—Signs of consolidation very marked in right lung, breathing pure in left; is very restless; breathes laboriously, with short frequent cough; skin hot; no vomiting. Pot. acet. added to mixture, ʒi. secundis horis. 22nd.—Respiration 72; takes milk and beef tea well. Pt. cum mist. tertiis horis. 23rd.—Pt. in mist. quartis horis. 25th.—Good resonance in left side of the back, and vesicular breath sound, with fine pneumonic crepitation; in right pretty good resonance, with increased vocal vibration, and the same fine crepitation as in left; respiration 68; pulse quick; skin rather hot; lower end of sternum falls in during inspiration; gets worse towards evening. Quin. disulph. gr. ix., acid aqua ℥iss. M., ʒj. ter. die; ol. ricini. 28th.—Better, but in both sides of the back there is most marked, extensive, fine crepitation, scarce distinguishable from true pneumonic, not crepitatio redux. 30th.—Improved. Pt. ol. morrh. ʒi. ter die. May 3rd.—Quite convalescent; good breathing, without râle in left side of the back; not so free in right, but without râle.

Case 31.—*History:* A. G., aged 1 year, female, admitted May 24th. Ill ten days; has some pyrexia; no affection of breathing, but is drowsy, and has four days later an abnormally quiet look. Pot. citr. cum acid. hydrocy. dil., quater die. 31st.—On auscultating chest the right lung was found to be extensively hepatized, the left working well; general state same; some cough, consisting of a short expiratory grunt. Hirudo j. dors. dextr., mist. pot. citr. ℥ij., pot. nitrat. gr. xij., vin. ipecac. ♏ lxxx. M. ʒj. omni horâ ad vices. vj., postea secundis horis. June 1st.—Child appeared immediately relieved by the leech, sat up at once, and appeared better all the evening; breathing quiet; lies on left side; skin cooler; more vesicular breathing, with fine crepitations in right lung, less dulness. 2nd.—Respiration 69; cough troublesome; dulness in right side of the back almost gone, muco-crepitant râle is heard extensively; blister to right side of the back. Mist. pot. citr., squill, chloric ether and conium. 4th.—Weak breathing in right side of the back, but with very little râle; air does not enter freely; single short cough; lies constantly on left side; had much cold sweat last night, and is feverish this morning; respiration 21. Quin. dis. gr. $\frac{5}{8}$ ter die. 6th.—Is very much better; some râles in both backs, but no marked dulness; is sitting up; takes beef tea well. Pt. vin. ferri ♏ xx. ter die. 11th.—A relapse commenced yesterday, same treatment as on 31st resumed, under which she was again convalescent in about a week, and was put on ol. morrh. 28th.—Much better, but weak and pallid, and has cough; good breathing on both sides of chest. Pt. oleo, vin. ferri ʒj. ter die. Aug. 9th.—No chest affection since, continues treatment for improvement of general state.

Case 32.—*History:* M. W., aged 2, female, admitted April 26th. Ill one day, poorly fourteen; looks drooping and ill; is feverish, breath short, thirsty, no appetite; clear but rather harsh breath sound in backs; drowsy. Cat. sinap. nuchæ, colchicum, ipecac. nitre, mist. am. acet. 30th—Is worse at night, her breath especially; moist râles last two days in both backs, and today bronchial breathing in left; very feverish last night and restless, cool this morning; tongue coated, red at tip. Pulv. Dover. gr. j. omni nocte, cerat. hydr. co. thoraci, quin. dis. gr. j. ter die. May 1st.—Respiration 70. Cat. lini continuè dorso. 9th.—Gradual improvement since; respiration 44; tongue moist; aspect better; appetite good; sleeping well at night with powders (not without); good percussion in both backs, and good breathing in left. Ol. morrh. ʒi. ter die, pt. 12th.—Is convalescent; breathing quiet. Pt. in ol. et pulv., acid nitric., spt. æth. s., co. tinc. calumb. 17th.—In right back I found yesterday dull percussion sound, fine crepitus, and bronchial breathing; in left some dulness, fine râle and bronchial breathing, but less than in right; she is sitting up, takes food well, looks improved and more cheerful. 18th.—Pt. ol. resumat mist. quinæ. 23rd.—Both backs more resonant, still dullish; breathing pretty free in left, less bronchial in right, and fuller. Taking since 17th vini ferri ʒss. ter die. June 2nd.—Improving very fast; tolerable resonance in both sides of the back, and good breathing, with some fine moist râles in right, clear good breathing in left. She went on to recovery steadily after this.

Comments on the Individual Cases.

1. Mercurial and saline treatment certainly appear to have promoted and accelerated the favourable termination of this case, and not to have been in any way injurious.

2. An instance in which antimonial and mercurial treatment, the antimonial in pretty full doses, failed entirely to arrest bronchopneumonia in a young child.

3. A severe double pneumonia in a child terminating favourably under mercurial and antimonial treatment with blisters. The treatment did not seem to be injurious; and though it did not arrest the disease, it may yet have checked its force, and promoted an earlier recovery.

4. Treatment of no marked efficacy.

5. The lungs were probably intensely congested during the submersion, and this congestion passed into violent inflammation, which the remedies were utterly inadequate to control. To judge from the record, the morbid process must have speedily assumed an asthenic character, and required rather stimulants than depressants. The cupping was desirable; the indication in all such cases seems to be to unload gorged vessels, and to give tone and energy to their walls and to their impelling force.

6. There was much uncertainty whether the disease was primarily fever or pneumonia. The tonic was of manifest service. The fatal event, which was unexpected, probably resulted from the fibrinous coagula having interfered with the circulation.

7. Antimonial and mercurial treatment was well borne, and the patient recovered well. But the disease seems to have been but little checked in its progress, going on to consolidation and subsequent resolution much as it might have done if left alone. The pneumonia was in its first stage when treatment was commenced. The long persistence of the peculiar sound of air-cells expanding on deep inspiration, while their walls were ordinarily collapsed and adherent, was remarkable.

8. Antimonial treatment in this case seems to have been of decided efficacy; on the second day of its employment there was marked improvement.

9. In this instance, also, no possible doubt can exist as to the highly beneficial effect of a full antimonial treatment.

10. Though a weakly girl, full antimonial treatment was in no wise prejudicial, but seemed decidedly to promote the resolution of the inflammation, and recovery of the affected lung.

11. Similar good effect of antimony.

12. Speedy resolution of hepatization took place under ipecacuan and saline, with leeches and mercurial ointment.

13. In this, pleurisy, with effusion, was predominant. Antimonial and mercurial treatment, the latter carried to ptyalism, was well borne, and with evident advantage.

14. This was an instance of that peculiar combination of proptosis,

thyroïd enlargement, and palpitation, which has been noticed by various observers. As far as my own experience goes, the fundamental condition in these cases is that which I have termed neurolysis, the nerve-power being in a very enfeebled state, but no organic disease existing. Opium, strychnine, and other tonics, repose, and change of air, appear to be the best remedies. In one extremely marked case the throbbing of the thyroïdeal arteries was a very striking feature, showing forcibly the paretic condition of their vaso-motor nerves. The attacks of pain and palpitation are ascribable, with much probability, to a neuralgic affection of the pneumogastrics. As I have argued elsewhere, the neuralgic state more or less approximates to paralysis of the nerve, and is by no means to be regarded as an exaltation of its function. Paralysis of the pneumogastric, we know from experiment, induces accelerated action of the heart: the pain and palpitation may therefore thus be accounted for. But the same assumption will enable us also to account for the sudden and fatal pneumonia. It has been shown by Bernard—and I have verified his statement—that in states of debility severe inflammation may be the consequence of vaso-motor nerve paralysis. If, then, the pulmonary plexuses, formed in great part by the pneumogastric, were in this state of paralysis, the result would ensue which actually occurred in this instance. Dr. Budd* has recently drawn attention to this cause of pneumonia occurring in cases of tumour at the roots of the lungs, and Dr. Ogle has also illustrated the same subject.† Such inflammations will generally be of asthenic character, requiring stimulants and nervines. The pressure of the enlarged thymus may have had to do with the systolic bruit over the pulmonary artery, but not with the other cardiac symptoms. The bronchial glands were not, I believe, sufficiently enlarged to have exerted injurious pressure on the pulmonary nerves. I have noticed in other instances the concurrence of enlarged thymus and lymphatic glands.

15. Severe pneumonia in a young child utterly uncontrolled by full antimonial treatment.

16. The chief remarkable feature in this case is the delirium, which scarcely seems attributable to the deficient aeration of the blood, as one lung was working tolerably well, as it was lessened by the use of opium, and as it continued after recovery had fairly set in. I should ascribe it to the same obscure influence which had produced the pneumonia acting also on the brain, and think that its causation is similar to that of continued fever.

17. A case of severe though one-sided pneumonia, probably of asthenic character, and requiring the use of stimulants. The rapidly fatal event, the delirium, and prostration make one think of poisoned blood.

18. A robust man, affected with double pneumonia, treated by full cupping and moderate antimonial doses, dying in seven days from the date of the serious symptoms. There is a doubt as to whether the kidneys were healthy; the existence of pericarditis and double pneumonia points to the infection of the blood by some poison, whether

* Medico-Chirurgical Transactions, vol. xliii. p. 215.

† Transactions of Pathological Society of London, vol. xi. p. 76.

uræmic or of atmospheric origin. The vigorous state of the health up to the commencement of the illness favours the latter view.

19. This case was treated as one of fever, but there is no sufficient evidence that such was the case. The herpetic eruption is more indicative of an influenzal (catarrhal) affection. No special treatment beyond the cupping was adopted for the pneumonia.

20. A robust man, affected with one-sided pneumonia, terminating favourably in about three weeks under a small cupping, nitre, and small doses of antimony, with mercury to gentle salivation. It is highly probable that this man would have recovered almost as quickly if he had taken nothing, or a decillionth of a grain of natron muriaticum.

21. Pneumonia after measles, which threatened to be severe, disappearing rapidly under a mild mercurial, at the same time that the intestinal secretions became more healthy. I strongly suspect that if antimony had been given in this case things would have gone ill. The influence of gastro-intestinal irritation in originating, or keeping up, or aggravating thoracic inflammations, has been noticed by several physicians, and is forcibly dwelt on by Langston Parker in his most practical work 'On the Stomach in its Morbid States,' 1838. The connexion of the morbid phenomena seems explicable on the principle of inhibitory action, the injurious impression communicated to the nervous centres from the gastro-intestinal surface determining a reflex paralysis of the vaso-motor nerves of the respiratory tract. Case 10 recorded by L. Parker, p. 224, is manifestly analogous to that related by Brodie in his work 'On Local Nervous Affections,' 1837, p. 12.

22. The same patient, two months after recovery, is attacked with symptoms of broncho-pneumonia. Antimony is given in pretty full doses with some good effect, but soon has to be discontinued (in three days), and is replaced by senega and ammonia with the best effect.

23. I mistook this case at first for phthisis, and was much surprised at my second visit to find that hepatisation had taken place. The resolution which followed may be regarded as spontaneous. The absence of dyspnœa, decubitus on the healthy side, and the greater mobility in deep inspiration of the most affected side, are points worthy of remark.

24. Here pneumonia seems to have occurred secondarily to the gangrenous process. I could not detect the signs at all during the earlier period of the disease, when, however, the very peculiar fœtor of the breath and urine positively showed that such mischief was going on. The small focus of gangrene in the right lung in a part that was not otherwise materially diseased, also goes to prove the primarity of the gangrene. I am not aware that the urine has been noticed to be specially offensive in cases of pulmonary gangrene. I greatly doubt whether in this case the grey hepatisation was consecutive to red; it seems to me more probable that it was primary; and the same occurred probably in No. 5.

25. A case in which broncho-pneumonia attacking a healthy child was entirely unchecked by full antimonial treatment. The object

aimed at was to check the disease as rapidly as possible by a vigorous treatment, which the condition of the child appeared to justify. I fear there is no doubt that the remedy was injurious, and promoted not the cure of the disease, but the death of the patient.

26. An instance of purpura, with renal and gastro-intestinal congestion at the commencement, complicated afterwards with intense pulmonary. The venesection was beneficial, but the antimonial entirely failed to relieve the dyspnœa, and may even have hastened the fatal event. The patient was very unfavourably circumstanced.

27. An instance of utterly asthenic pneumonia, with remarkable transfer of morbid action to a limb which had been similarly affected many years before. It may be incorrect to use the term metastasis, but it certainly appeared that the chest affection receded and passed into the background, so to speak, as that of the limb became prominent. Both were in some measure rather œdemas than fibrinous effusions. One cannot help asking oneself what makes the difference between these asthenic affections attended with serous effusion and apyrexia, and the sthenic with their high temperature and solidifying exudations. It certainly is not merely that disease affects in the one instance a strong, and in the other a weak system. This very patient was by no means a weakly man. To speculate on the causes which determine the difference of the reaction in different cases would be useless, but there does seem much need to insist on the great difference, the essential dissimilarity of the morbid processes in many instances. There is far too much tendency to consider inflammation as always uniform, and to be dealt with in an uniform way.

28. Treatment was neatly adjusted in this instance. The febrile movement might have deterred one from the use of the tonic, which, however, acted very beneficially. The due selection of such opportunities is one of the nice points of practice.

29. The relief afforded by the leeches in this case was most remarkable; the disease was arrested completely. The rash I believe to have been a roseola; there were no sufficient indications of the existence of scarlatina or measles.

30. This child was a poor weakly thing, very unlike the subject of the preceding case. The ipecacuan and saline treatment seemed to be of real efficacy in arresting the inflammation and promoting resolution. At a later period, when, however, the indications were by no means positive and clear, the administration of quinine was very successful.

31. The relief afforded by a leech in this case was well marked. After the pneumonia had existed probably for more than a week, and had gone on to hepatisation, the little patient immediately revived on this depletion. I cannot but hesitate to regard this change as merely the result of withdrawing so much blood from the general system. I believe rather that the local depletion exerted some direct influence in diminishing the pulmonary inflammation, difficult as it seems to account for it. The ipecacuan and saline treatment answered well, and so did the early administration of quinine. This case shows

almost certainly the beneficial effect of treatment. The disease was not recognised for a week, and no efficient remedies employed. As soon as these were put in force, improvement began, and the child was fairly convalescent at the end of the week.

32. In this instance there was question of the pneumonia or the fever being the essential affection. Under the influence of quinine she passed favourably through the disease, and (which is rather remarkable) relapsed on the quinine being withdrawn, again rallying, and recovering well under its use.

Of these 32 cases, 8 were under two years of age; of these 3 died.
" " 5 were between two and five years; of these 1 died.
" " 6 were between five and twenty years; of these 1 died.
" " 13 were above twenty years; of these five died.

There were 17 males, 15 females. Of the former 8 died, of the latter only 2. Of the 10 fatal cases, 7 were treated by antimony in smaller or larger quantities, with or without depletion and mercurials; 3 were treated by tonics.

Of the 22 recoveries, 3 had tonic treatment throughout, 1 had stimulants after three days, 3 were treated by repeated doses of saline and ipecacuan, with or without leeches, 2 had the same less frequently, 4 had mercurials chiefly, 9 had antimonials in larger or smaller doses, with or without mercurials, and depletion by venesection, cupping, or leeches.

There were 6 adults (above the age of fifteen) who took antimony in full depressing doses. Of these 4 recovered and 2 died. In 2 the good effect was very marked. Of the 2 fatal cases 1 was almost desperate before the antimony was given, in the other there is doubt as to the kidneys being sound.

There were 8 children who took antimony in depressing doses; of these 5 recovered and 3 died.

There were 2 adults (above the age of fifteen) who took small (diaphoretic) doses of antimony. Of these 1 died, 1 recovered.

There were 10 cases of depletion by cupping, leeches, or venesection. The 3 who were leeched were children, and all did well; in 2 the relief was remarkable. In the 5 who were cupped, no remarkable effect was produced, except in 1 great relief to local pain. The 2 venesections had no striking effect.

General Observations upon the foregoing Cases.

1. One point well shown by the foregoing cases is the exceeding diversity that prevails between different cases of pneumonia with regard to their most essential features. Several of those above recorded are as unlike as they can well be. What a wide difference between cases 8, 9, 13, and 14, 18, 28! How unlike is case 23 to case 16! How manifest it appears that it is impossible to class all these together, and lay down a general plan for their management? This is no new doctrine, but yet it seems often forgotten, or tacitly set aside. Writers

debate of the treatment of pneumonia, and compare statistics of mortality, according as it is treated in one mode or another, not remembering apparently that the things they compare may be utterly dissimilar. Morbid anatomy and physical signs are very unsafe guides to trust entirely to. The processes which give rise to those changes and phenomena which we can discern, are not seldom of entirely different dynamic character. One is evidently checked and subdued by an agent which is quite without effect upon another. These interior differences are often betokened by external signs, as the state of the countenance, the pulse, the muscular and nervous system, the condition of the skin and of the sputa. These are more declarative of the true quality of the disease than the sounds appreciated by the ear, or the post-mortem revelations. The adage adopted by Trousseau that "Naturam morborum remedia ostendunt," is to the clinical observer the most practically important. If in any given case he doubts, as he often will, what is the right course to pursue, let him test the state of the system cautiously with some agent of known power, and observe the effect. The result will generally put him on the right track. In some cases confinement to bed and restricted diet may be all that is necessary to insist on, and the inflammation may be left to take its course. In other cases tonics and stimulants must be freely employed. In others, again, depletion, with antimony and ipecacuan, will be of essential service. These statements, I believe, cannot be denied, and if not, it seems to me but a little step further to maintain that one or other kind may be the prevalent form of disease at different periods. It is obvious that the view here taken is the one which tends most to make the physician a careful practitioner. One who believes that all inflammatory disease is of an uniform and unvarying type, to be dealt with by brandy according to one school, by bleeding and antimony according to a second, and by expectancy according to a third, cannot fail of becoming a routinist. But he who believes that inflammatory disease, of all others, is most varying and uncertain, will ever be on the watch to adapt his treatment judiciously to the case before him.

2. If pneumonia has fairly set in, it is matter of great uncertainty whether it can be arrested, or prevented from advancing to hepatisation more or less extensive. In only one of my cases does it seem probable that this was accomplished. On the other hand, there is no question that the resolution of the inflammation and resorption of the exudation can be materially accelerated and promoted by treatment. Convinced as I am of the mischief that may be done by pushing antimony too far, I should yet be truly sorry to be deprived of it as a remedy.

3. In London children, if below the age of two years, ipecacuan, nitre, and saline mixture, with or without a little opium, given very frequently, is preferable to antimony. In tolerably strong children, especially if seen early, leeching is most useful.

ART. III.

Surgical Miscellanies. By FURNEAUX JORDAN, Assistant-Surgeon to the Queen's Hospital, Consulting Surgeon to the Birmingham Eye and Ear Dispensary, and Professor of Anatomy at Queen's College.

I. *An Undescribed Affection of the Leg.*

DURING the last six years I have seen a few cases of a peculiar affection of the leg, any description of which I am unable to find in surgical literature. The cases to which I allude are undoubtedly rare —less rare, however, I think, than many surgical diseases of which we are supposed to possess an extended knowledge. It is my intention, in these miscellaneous surgical notes, to avoid the record of individual cases, and state merely the conclusions which they have led me to form. Let the reader test such conclusions by his own experience—by his own recorded and remembered cases.

The disease to which I refer exists in two varieties, which may occur separately or together. In the first there is a convex enlargement or collar completely surrounding the lower part of the leg immediately above the malleoli. A side view of this collar-like mass is represented in the woodcut (Fig. 1) annexed. The second form (Fig. 2) is more

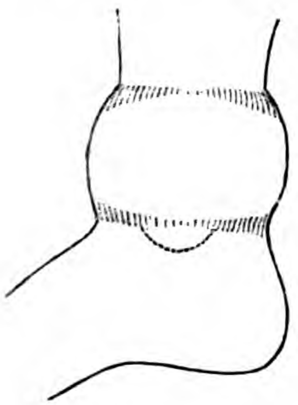

Fig. 1.

Fig. 2.

common and less peculiar; it consists in a slightly irregular, rounded, or hemispherical projection seated below and behind the external malleolus. The relative frequency of the two affections may be stated as follows: little frequent is that below and behind the outer ankle; less frequent is the combination of the two; least frequent is that around the tibia and fibula.

The rarest form of the disease, and the most characteristic, is a soft, elastic enlargement, occupying the entire circumference of the leg. Its size and shape are the same at the inner, outer, anterior, and posterior aspects. The vertical measurement of the collar is about three inches. Its greatest thickness (an inch and a half) is at the centre. The margins are thinner, but do not become imperceptibly lost in the parts above or below. The lower portions of the malleoli are not involved. At no period in the formation of the growth is there doughiness, or pitting, or pain, or tenderness, or integumental discoloration. Indeed, the skin is throughout perfectly healthy. The bones of the leg are not altered in size. The formation appears to be gradual, and unattended by noticeable constitutional phenomena. The word "noticeable" is used designedly, because the general health is certainly not good, and there are indications of cardiac or circulatory derangement in greater or less degree. In the most typical case I have seen there was marked mitral regurgitation, but there was no history of acute rheumatism or any other acute disease. All the cases were in young, adult, unmarried, and childless women.

In asking what the affection *is*, let us first ascertain what it *is not*. It is not elephantiasis arabicum; elephantiasis has a greatly thickened, hard, rough, nodulated, darkly coloured, and partially insensible skin. The bones, areolar tissue, fat, and other structures (muscles excepted), share in the enlargement. The toes and feet are implicated, and severe pain is usually present.

It is not a cutaneous outgrowth; in the cutaneous outgrowth, as Mr. Paget remarks, all the tissues take part, and the proper tissue and appended organs of the cutis are nearly as much exaggerated as the fibro-cellular substance. Fibro-cellular outgrowths, moreover, are usually pedunculated and attached to the parts in the vicinity of the vagina and rectum.

It is not a fibro-cellular tumour—at least if we use this term in its strict meaning—for, independently of the circumstance that the fibro-cellular tumour resembles in its sites the cutaneous outgrowth, the enlargement I am attempting to describe certainly could not be separated or enucleated from the neighbouring tissues.

Erythema nodosum sometimes leaves irregular bulky masses on the leg, but its history is unmistakeable, and the enlargements which it leaves have no uniformity of configuration or locality. The term phlegmasia alba dolens has an accuracy and a significance of meaning which render any observations on white leg a superfluous task.

That the enlargements in question are fibro-cellular *in structure* I think there can be little doubt, though differing essentially from the cutaneous outgrowth and fibro-cellular tumour. There is probably as little doubt that one of the two following causes operates in their development. Either (1) there is a peculiar fibrinous œdema arising from extreme obstruction (cardiac or other), of an extent so limited and a duration so temporary as to permit of the organization of the effused plastic material, or (2) there is inflammation which is neither acute, nor obvious, nor lasting. The inflammatory phenomena, however, do not disappear until plastic lymph has been effused in a locality and under circumstances favourable to organic development. The second causal agency appears to me the more probable. In dropsy, local or general, serum without fibrine is usually, and always at first, effused. In extreme obstruction, such as to involve exudation of fibrine, the large quantity of attendant serum would have a historical value too great to escape detection. But indeed fibrinous dropsy in the lower extremity, with or without general anasarca and grave constitutional ailment, is altogether improbable. In the hydrops fibrinosus of Vogel, where the fibrine was noticed in the abdominal cavity, serious disease was certainly present.

If, then, inflammation be the cause, what tissues does it affect? Why should it be located immediately above the malleoli, or beneath the outer one? Why in those localities particularly should the inflammatory product be more susceptible of organization, and less amenable to absorption than elsewhere? Probably from some evanescent and slight cardiac ailment, endocardial or pericardial, or, which is more probable, from a temporary aggravation of a chronic disease, obstruction occurs in the venous system. This, for many reasons, is chiefly evident in the lower extremities. The internal and external saphenous trunks, and the network of veins which connects them around the lower part of the leg, are plugged with coagula, which subsequently acting as foreign bodies, cause a certain degree of inflammation attended with effusion of plastic lymph. The inflammation terminating quickly, permits the lymph to organize, probably by nucleated blastema, as in the repair of subcutaneous wounds. Now, if the obstruction should be more permanent, or the inflammation more severe, we should have not development, but disintegration, in the form of ulceration, varicose or otherwise. It is possible that the inflammation may be confined to the radicles and the commencement of the external saphenous vein, which runs *behind* the outer ankle without any muscular support, and which, as we shall presently see, may explain the second form of enlargement under consideration. The obstruction and coagulation of blood in the veins is commonly attributed to the hydrostatic pressure in a long column of blood, as that in the internal saphenous vein. Apart from the fact that this hypothesis does not explain the lesion in the external saphenous vein and its radicles, it appears to me that a better explanation can be found, and that the anatomist can more easily discover it than the mechanical philosopher. There is ample proof that muscular contraction plays an important part in assisting the flow of blood in the veins. It is a remarkable fact that such assistance is almost entirely absent in the lower third of the leg and below the outer ankle. Tendons have not the slightest contractile, and therefore no expansive power. In the lower part of the leg we find (for very good reasons too) tendons only. The tendons of the deeper layer of muscles at the back of the leg have muscular fibres joining them at a lower level than is the case in the superficial layer, but they are so tightly held down to the tibia and fibula by the deep fascia as to exercise no influence on the superficial veins by their contraction. Further, on each side of the tendo Achillis there are large spaces filled with loose areolar and adipose tissue, which afford the most favourable opportunities for the inaction and dilatation of veins. In exceptional cases, I may parenthetically remark, the soleus muscles send muscular fibres to the under aspect of the tendo Achillis almost to its insertion. Such cases we may reasonably infer would be little liable to varicose or indolent ulcer, still less to the peculiar enlargements under consideration. Again, a third of the tibia is subcutaneous, and the veins (the internal saphenous especially) can receive no pressure from so passive an agent. Thus, then, many circumstances conspire to show how a lesion affecting the venous system is prone to show itself in the supra-malleolar region and below the outer ankle. In the latter spot the external saphenous vein lies on the flat calcaneum, totally unassisted by muscular action. It is true that the two peroneal tendons run obliquely across the outer and anterior part of the os calcis, but they have not the least influence in compressing the veins.

I have reason to think that the enlargement which I have described as seated below the outer ankle has been frequently, though vaguely, referred to synovial distension of the peroneal sheaths. On the supra-malleolar form of the disease I have never known an opinion given.

If it be granted that I have shown sufficient grounds for the dilatation and inflammation of certain veins, with the possibility of an exudation of plastic lymph that shall, under certain circumstances, be capable of organization—the circumstances, however, being so rare and so peculiar, that varicose and indolent ulcers must always be more common diseases than the one which forms the subject of these observations—it will also be granted, the lymph having been exuded, and the inflammation and other circumstances unfavourable to organization having disappeared, that the same agencies which operate against the flow of blood in the veins will operate also against the absorption of the lymph which is deposited in the areolar tissue at the lower part of the leg. Here, then, there is none of that muscular pressure which elsewhere is a most effective agent in the absorption of inflammatory exudation.

It is not improbable that the fluid exuded by the inflammatory process should escape notice, because the so-called serum of inflammation is really liquor sanguinis, containing all the material requisite for coagulation and organization. Probably, too, the enlargements do not result from a single pathological process, but from a series, each in itself slight, painless, and unobserved.

I have indirectly referred to a new pathology for indolent and varicose ulcers. At another time I hope to fill up the outline, and suggest a treatment which is more analogous to muscular pressure than any now in use, and which I have adopted in practice with great success.

II. *A Node on the Patella.*

In the 'Edinburgh Medical Journal,' in 1857, I described a case of bubo occurring within the abdominal cavity. Last year another singular form of constitutional syphilis came under my care. A hard, indolent, partially tender, and nocturnally painful swelling formed on the patella. This, after a lengthened period, suppurated. A practitioner, under the impression that it was an inflamed bursa, passed a seton through it. Inveterate, though varying suppuration, and short, irregular sinuses in the superjacent tissues, followed for upwards of two years. The knee-joint was not affected, but the patella was much enlarged, and its range of motion greatly diminished, The tendon of the extensor of the thigh covers very closely the anterior surface of the patella, and presents certain analogies to the relation of the occipito-frontalis muscle to the cranial bones, which are so obnoxious to syphilitic disease.

III. *A Proposition for the Treatment of Aneurismal Varix.*

The rarity, in late times, of cases of aneurismal varix must be my apology for proposing a method of treatment before I have had an opportunity of practically testing its utility. It is common to do nothing in these cases unless the symptoms are very severe, when a ligature is applied above and below the wound in the artery. It appears to me that, in a large number of cases, especially in the extremities, very considerable, if not complete, relief might be afforded by *obliteration* of the vein, above and below its communication with the artery, by means of a needle and twisted suture, or some other method, as in an ordinary (and widely different) varicose vein; perhaps in some cases a needle might also be passed with advantage under the contiguous artery for a short time, as in the acupressure method of Professor Simpson. If the practicability of the principle be granted, the method of applying it admits of great variety to meet the requirements of individual cases.

PART FOURTH.

Chronicle of Medical Science

(CHIEFLY FOREIGN AND CONTEMPORARY).

HALF-YEARLY REPORT ON PHYSIOLOGY.

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I. Physiological Chemistry and General Physiology.

1. Schönbein: *On the Chemical Polarity of Oxygen.* (Poggendorf's Annalen, vol. cviii. p. 471; and Canstatt's Jahresbericht über die Forschritte in der gesammten Medicin in 1859, p. i.)
2. Gorup-Besanez: *On the Action of Ozon on Organic Substances.* (Annalen d. Chem. und Pharm., vol. cx., p. 86; and Canstatt, loc. cit. p. 188.)
3. Bödeker: *On Quantitative Determination of Albumen.* (Zeitschrift f. rat. Med., vol. v.; and Canstatt, loc. cit. p. 205.)
4. Léconte: *The Correlation of Physical, Chemical, and Vital Forces, and the Conservation of Force in Vital Phenomena.* (The American Journal of Science and Arts, vol. xxviii. p. 305, 1859.)
5. *Physiological Riddles.* (Cornhill Magazine, July, August, September, and October, 1860.)

1. Schönbein again endeavours to prove his view, that the ozonization of oxygen is caused by a division of this body in two. One of these two bodies, designated by the author ⊕, is recognised by its property of reducing metallic acids; it exists in the binoxide of hydrogen. The other body, designated ⊖, can present itself in the gaseous condition as common ozon, exhibiting the well-known reactions. Schönbein shows that the slow combustion of phosphorus and ether, and the electrolysis of water develop always, besides the ozon (⊖), also ⊕, which latter, however, never appears in its free condition, because it combines at once with the water present (HO), to form binoxide of hydrogen (HO_2).

2. The view that the oxygen acquires within the animal organism a peculiar energy, analogous to that caused by the electric spark, by phosphorus, and other agents, led Gorup-Besanez to examine the action of ozon on organic substances, in order to compare the products thus artificially obtained with those arising from the oxidation within the animal body. Many substances, as urea, allantoin, alloxan, kreatin, sugar of milk, inosit, and hippuric acid are either not at all, or only slightly influenced by ozon. Lithic acid is dissolved and transformed into urea and allantoin. Albumen is likewise considerably changed by ozon, the products being analogous to the peptons of Lehmann. Casein is at first transformed into a substance exhibiting the qualities of albumen, but afterwards it becomes further changed, yielding a body similar to oxidized albumen. On fibrin, as well in an alkaline solution, as also in water, ozon remained without effect. Bile absorbs the ozon and loses its colour and its viscidity; ozon appears therefore to destroy the colouring matter and also the mucus. On yeast and emulsin ozon acts with great energy.

3. Bödeker suggests, instead of the usual tedious way of determining the quantity of albumen, a volumetric method by means of a solution of ferrocyanide of potassium in acetic acid. We refer for the details to the paper, which contains the results of the analysis of various albuminous fluids, obtained by this volumetric method and compared with the figures found according to the plan hitherto usually adopted.

4. Léconte's paper, on the correlation of physical, chemical, and vital forces, treats on laws of the greatest importance regarding general physiology. The mutual convertibility of the various forms of force ("correlation of forces"), and the invariability of the absolute amount of force in the midst of constant change ("conservation of force"), form the basis of all his deductions. The correctness of the theory of the correlation of *physical* forces the author considers as generally adopted; but the correlation of the *physical* and *vital* forces has not yet been sufficiently interpreted, though Carpenter has so ably drawn attention to the subject in the 'Philosophical Transactions' for 1850. Léconte assumes four planes of material existence, of which each following plane is raised a step above the preceding: 1. The lowest plane is that of *elementary existence*. 2. That of *chemical* compounds (mineral kingdom). 3. That of *vegetable* existence. 4. That of *animal* existence. It is impossible for any force in nature to raise matter at once from the lowest to the highest plane; this can be done only step by step. It requires greater and greater expenditure of force to maintain matter upon each successive plane; therefore any amount of matter returning to a lower plane by decomposition must set free or develop a force which may raise other matter from a lower to a higher condition. "Thus *decomposition* must in every case develop *force*, which force may take the form of heat as in combustion, or electricity as in electrolysis, or may expend itself in forming chemical compounds, or even in organizing matter." The various forces Léconte divides into physical, chemical, and vital, each succeeding being higher than the preceding. The author then applies his view to many phenomena in chemistry and physiology, as the peculiar energy of chemical affinity in the "nascent condition," the phenomena of the seed in germination, the growth of plants with green leaves, and that of pale plants. As an instance, we will give the author's reasoning regarding the phenomena of the hatching of the egg and the growth of the animal after hatching. The *egg*, during incubation, evolves carbonic acid and probably also water, and *loses weight*, but *gains* in *organization*, the latter being in proportion to the former. Heat is here indirectly transformed into a vital force causing *decomposition* of a part of the organic matter, which latter, by descending from the organic to the mineral plane, sets free a force which may raise the remaining portion into a slightly higher condition. The *growth* and *nutrition* of the animal after hatching is again owing to *decomposition*. Léconte's view differs in this respect essentially from that usually entertained. While it is generally considered that the animal tissue is in a state of unstable equilibrium, leading to constant decomposition, and that this *decomposition* causes the necessity of *recomposition* or of food, Léconte regards the *decomposition as necessary to develop the force by which the organization of food or the nutrition is effected.* The fact that the body is not only *maintained*, but *grows*, is not opposed to this view, because the substance of the body, by its decomposition, descends from the fourth (animal) plane to the second (mineral) plane (carbonic acid, water, urea), and thus sets free much more force than is necessary to assimilate an equal amount of food—viz., raise it from the third to the fourth plane. This surplus of force may then be employed in the assimilation of an additional amount of food, and also in the production of animal activity, heat, and electricity.

5. Similar views are expounded by the author of the very suggestive papers, published in the 'Cornhill Magazine' under the title, "Physiological Riddles."

The correlation of forces is clearly expressed by the author's words: "There is a ceaseless round of force-mutation throughout nature, each one generating or changing into the other. So the force which enters the plant as heat, or light, &c., and is stored up in its tissues, making them 'organic;' this force, transferred from the plant to the animal in digestion, is given out by its muscles in their decomposition, and produces motion, or by its nerves, and constitutes the nervous force." The "vital power" the author describes as a modification of force which opposes chemical affinity in the living organism; owing to this opposition the chemical processes in the living tissues take place under peculiar conditions and manifest peculiar characteristics. The "functions" of the body (muscular motion, nervous action, secretion) depend on the decomposition of tissue—*i.e.*, on the coming into play of the chemical affinity. The "stimuli" call forth these functions. "In the muscle," the author says, "the chemical affinity, on the one hand, and a force, which we call, provisionally, the vital force, on the other, exist in equilibrium; the stimulus overthrows this equilibrium, and thus calls forth the inherent tendency to change of state. Magnets lose for a time their magnetic property by being raised to red heat; if, therefore, to a magnet holding a weight suspended, heat enough were applied, it would permit the fall of the weight. It is thus the stimulus 'permits' the function." The *cause* of the muscular motion, however, is in the muscle itself—in its decomposition. Thus throughout, the cause of animal activity and nutrition is the decomposition of tissue, by which force is set free, which expends itself in the various phenomena of the animal organism. The author then ingeniously explains the growing of plants on decaying substances, and the phenomena of fermentation, by the principle that decomposition is always connected with the giving out of force, which force is, under favourable circumstances, expended in "organizing" a certain amount of other substance. The constant dependence of nutrition on decay, the inseparable interlinking of the two processes caused, though opposed to each other, by the same primary action—viz., chemical affinity—is compared with the phenomena of the fountain, where the same simple law of gravity produces likewise apparently opposite effects. "Chemical affinity at the same time produces and destroys the living frame, as gravity at the same time produces and destroys the fountain."

In a paper on "living forms," the same author shows that the living substance grows in the direction of the least resistance. Thus the buds of plants almost always grow in the axils of the leaves, and in the young chicken the eye "buds out" between the first and second lobes, the ear between the second and third. While the chemical forces produce the living organism, the mechanical force, by the resistance of the structures which surround the growing organism, gives it the necessary shape. The "growth under resistance" is the chief cause of the spiral form assumed by living things. "The expanding tissue, compressed by its own resisting external coat, wreathes itself into spiral curves." The formation of the heart is used as an illustration. "That organ," the author states, "originates in a mass of pulsating cells, which, gradually becoming hollow, gives the first form of the heart in a straight tube, more or less subdivided, and terminating at each extremity in bloodvessels. This is the permanent form of the heart in many animals." When the organ is to be developed into a more complex form, the straight tube growing within a limited space "coils itself into a spiral form."

II. Food and Digestion.

1. Lawes and Gilbert: *Experimental Inquiry into the Composition of some of the Animals Fed and Slaughtered as Human Food.* (Proceedings of the Royal Society, vol. ix. p. 348, 1859.)
2. Hühnefeld: *De Albuminis Succo Gastrico Factitio Solubilitate.* (Gryphiæ, 1859; and Canstatt, l. c. p. 30.)
3. Lent: *De Succi Gastrici Facultate ad Amylum Permutandum.* (Gryphiæ, 1858; and Canstatt, l. c. p. 30.)
4. Marcet: *On the Action of Bile upon Fats.* (Proceedings of the Royal Society, vol. ix. p. 306, 1859.)

1. The researches of Lawes and Gilbert regarding the composition of some of the animals fed and slaughtered as food are of great interest in relation to the physiology as well of food as also of nutrition. The animals submitted to analysis were oxen, sheep, and pigs. The authors determined the actual and percentage amounts of water, of mineral matter, of total nitrogenous compounds, of fat, and of total dry substance in the entire bodies and in different parts of ten animals. There appears, from these analyses, to be a prominent connexion between the amount of mineral matter and of nitrogenous constituents of the body. The average amount of *mineral matters*, in the entire bodies analysed, may be estimated in *lean* oxen to be 4½ to 5 per cent.; in *lean* sheep, 3½ to 4 per cent.; in *lean* pigs, 2½ to 3 per cent. In *fattened* animals the figures would be—for oxen and calves, 3½ to 4 per cent.; for sheep and lambs, 2½ to 2¾ per cent.; for pigs, 1¼ to 1¾ per cent. "Of total *nitrogenous compounds* there were in the fasted live-weight of the fat ox, 14½ per cent.; in that of the fat sheep, 12¼ per cent.; in that of the very fat one not quite 11 per cent., and in that of the moderately-fattened pig about the same—namely, 10·87 per cent. The leaner animals analysed contained from 2 to 3 per cent. more nitrogenous substance than the moderately-fattened ones." The *fat* is the principal constituent of the dry substance of most animals consumed as food. The fat calf alone contained less fat than nitrogenous substance; while the entire bodies of the fat ox and lamb contained about 30 per cent.; of the fat sheep, 35½ per cent.; of the very fat sheep even 45¾ per cent.; of the moderately fat pig, 42¼ per cent of fat. "The average composition of the six animals assumed to be well fattened, showed in round numbers 3 per cent. of mineral matter, 12½ per cent. of nitrogenous compounds, and 33 per cent. of fat, in their standing or fasted live-weight." The meat of fat animals contains a smaller percentage of water, of nitrogenous compounds, and mineral substance, and a larger percentage of fat, than that of lean animals. Lawes and Gilbert further estimated, "that of the whole *nitrogenous substances* of the body, 60 per cent. in the case of calves and oxen, 50 per cent. in lambs and sheep, and 78 per cent. in pigs, would be consumed as human food. Of the total *fat* of the bodies, on the other hand, it was supposed that in calves and lambs 95 per cent., in oxen 80 per cent., in sheep 75 per cent., and in pigs 90 per cent. would be so applied." "Assuming," the authors argue, "the proportional consumption of the fat and nitrogenous compounds to be as here estimated, there would be in the fat calf analysed 1½ times, in the fat ox 2¾ times, in the fat lamb, fat sheep, and fat pig nearly 4½ times, and in the very fat sheep 6¼ times as much dry fat as dry nitrogenous or flesh-forming constituents consumed as human food." If we compare the composition of meat with that of our vegetable foods, especially *wheat flour bread*, and assume 2½ parts to be the "*starch equivalent*" for 1 part of fat, we find that bread contains 6·8 parts of respiratory or fat-forming to 1 part of flesh-forming material, while the average of the six animals yields the proportion of 10 parts of starch-equivalent to 1 part of nitrogenous compounds; the meat of the fat calf alone exhibiting a lower proportion than the bread.

2. Hühnefeld examined, with Budge, the influence of artificial digestive fluid, mixed with various acids, on albumen. The author's experiments proved that the digestive fluid prepared with hydrochloric acid dissolved the albumen most completely and in the shortest space of time; while that fluid in which the hydrochloric acid was substituted by acetic acid was least efficacious, lactic acid keeping an intermediate position between the other two acids.

3. Lent's researches relate to the alleged power of the gastric juice to transform starch into sugar. The author found, in his experiments on rabbits, that after the extirpation of the salivary glands (parotid and submaxillary), and the application of a ligature round the œsophagus, the starch introduced into the stomach was not transformed into sugar. These experiments corroborate, therefore, the view that the change of starch in the stomach observed by some physiologists had not been caused by the gastric juice, but by the saliva swallowed.

4. Marcet inquired into the nature of the action of bile (sheep's bile being used) on neutral fats and fatty acids, with the object of throwing some farther light on the digestion of fats. His investigations led to the following results:—1. A mixture of bile and neutral fats (stearine, oleine, and margarine) heated to a temperature above the fusing-point of the fat, undergoes no change, and no chemical action takes place. 2. A mixture of bile and fatty acids (stearic, oleic, and margaric acids), heated to a temperature above the fusing-point of fatty acids, is transformed into a solution, a very few and minute globules only of fat remaining unacted upon from the presence of oleic acid. This solution becomes a perfect emulsion on cooling, and is attended with the chemical decomposition of the bile; and further, if the emulsion of bile and fatty acids be filtered when quite cold, and the residue on the filter thoroughly washed with distilled water, the filtrate and washings mixed together again possess the property of forming an emulsion with another quantity of fatty acids, being also at the same time partly decomposed, although in the previous operation the bile appeared to have exhausted its power on the fatty acids. The filtrate and washings from this second operation again act upon a fresh quantity of fatty acids, and so on; only in every subsequent operation the proportion of emulsion obtained appears to diminish, and the induced chemical decomposition to be lessened. 3. Pure oleic acid, when agitated with bile, cold or hot, produces no emulsion or chemical action whatever. 4. The stomach during digestion has the power of decomposing the fats contained in the food into fatty acids, fats acquiring thereby the property of being acted upon chemically by the bile, and of being transformed into an emulsion.

III. Respiration and Circulation.

1. Smith, E.: *Inquiry into the Phenomena of Respiration.* (Proceedings of the Royal Society, vol. ix. p. 611, 1859.)
2. Smith, E.: *Experiments on the Action of Food upon the Respiration.* (Proceedings of the Royal Society, vol. ix. p. 639, 1859.)
3. Valentin: *On Respiration in a Confined Space.* (Zeitsch. f. rat. Med., vol. x. p. 33, 1860.)
4. Pettigrew, J.: *On the Arrangement of the Muscular Fibres of the Ventricular Portion of the Heart of the Mammal.* (Proceedings of the Royal Society, vol. x., p. 433, 1860.)

1. E. Smith gives the result of numerous inquiries into the quantity of carbonic acid expired and of air inspired, with the rate of pulsation and respiration—1st, in the whole of the twenty-four hours, with and without exertion and food; 2nd, the variations from day to day and from season to season; and 3rd, the influence of some kinds of exertion. For the description

of the apparatus we refer to the paper. The quantity of carbonic acid expired in twenty-four hours varied in four healthy men from an average of 24·274 ounces in one to 16·43 ounces in another. The quantity evolved in light sleep was 4·88 and 4·99 grains per minute, and when scarcely awake, 5·7, 5·94, and 6·1 grains at different times of the night. The author estimates the amount in profound sleep at 4·5 grains per minute. "The total quantity of carbon evolved in the twenty-four hours, at rest, was, in the author, 7·144 ounces." The quantity of air inspired during the working day varied from 583 cubic inches in one subject, to 365 cubic inches in another. The respirations were to the pulsations as 1 to 4·63 in the youngest, and as 1 to 5·72 in the oldest. "One-half the product of the respirations into the pulsations gave nearly the number of cubic inches of air inspired in some of the persons, and the proportion of the carbonic acid to the air inspired varied from as 1 grain to 54·7 cubic inches to as 1 grain to 58 cubic inches. The variations in the carbonic acid evolved in the working day gave an average maximum of 10·43 and minimum of 6·74 grains per minute." The effect of a fast of forty hours, with only a breakfast meal, was to reduce the amount of carbonic acid to 75 per cent. of that which was found with food. Meals caused an increase of carbonic acid; rise of temperature caused diminution of carbonic acid. The effect of season was to cause a diminution of carbonic acid and all the other phenomena of respiration from the beginning of June to the beginning of September, independently of temperature and atmospheric pressure. Walking and the treadwheel were the kinds of exertion examined. Walking at two miles per hour caused an exhalation of 18·1 grains of carbonic acid per minute, and at three miles per hour of 25·83 grains; while the effect of the treadwheel at Coldbath-fields Prison was to increase the quantity to 48 grains per minute.

2. In another communication E. Smith examined the influence of various articles of food on the process of respiration. The author divides, from the results he obtained, the various kinds of food into two classes—viz.: *respiratory excitants* and *non-excitants*. To the *excitants* belong sugar, milk, the cereals, potatoes, tea, coffee, chicory, cocoa, alcohol, rum, ales, some wines, gluten, casein, gelatin, fibrin, and albumen. The *non-excitants* are starch, fat, some alcohols, and coffee-leaves. Of the hydro-carbons, sugar acted very differently from fat. All the "respiratory excitants" increased *depth*, but not the *rate* of respiration. Some of them acted with great rapidity, as sugar and tea, causing an increase of one grain of carbonic acid per minute in from five to eight minutes; others, as gluten and casein, with less rapidity, but greater duration.

3. Valentin endeavours to learn the nature of the chemical phenomena of respiration in animals kept in a hermetically-closed space, filled with atmospheric air, which of course must become gradually poorer in oxygen and richer in carbonic acid. For the description of the apparatus, we refer to the essay itself. The animals submitted to the experiments were frogs, serpents, rabbits, guinea-pigs, cats, fowls, mice, rats, and dogs. The symptoms of dyspnœa exhibited by these different animals vary considerably; thus the number of respiratory movements decreased in the rabbit, the dog, and the hen, while it increased in the guinea-pig and the cat; the dog, cat, and guinea-pig exhibited phenomena of distress sooner than the rabbit, the hen still sooner, while the serpent manifested the greatest degree of dyspnœa when the percentage of carbonic acid was only so small (6·1 per cent.) as scarcely to excite abdominal respiration in the rabbit. The author gives a table showing the percentage of carbonic acid, oxygen, and nitrogen of the air at the point of the greatest dyspnœa, and another table with the analysis of the air which had caused the animal's suffocation. These tables corroborate the observation that animals dying from suffocation in a confined space consume almost the

entire amount of oxygen present, while the percentage of carbonic acid exhaled by them is smaller than the diminution of oxygen would indicate. The second table further shows that mice are able to exhaust the oxygen of the air to 0·6 per cent. before death ensues; that, however, in this respect too, different individuals of the same species of animal exhibit a considerable difference, and still more the different species of animals. While Bernard* asserts that birds reduced in health bear the deteriorated air better than healthy individuals, Valentin arrived, with regard to mammalia, to the opposite result. Concerning the amount of carbonic acid exhaled, the author concludes that mammalia perish in general before the percentage of carbonic acid in the surrounding air has become so high as not to allow any further exhalation of carbonic acid, according to the law of the interchange of gases. The fact that the serpent died when the surrounding air contained only 6·72 per cent. of carbonic acid, and the further fact that this percentage had become only slightly increased during several days preceding the death of the animal, led Valentin to the inference that the blood of the cold-blooded animals contains only a small amount of carbonic acid.

In the greater number of experiments the volume of air was diminished at the termination of the experiment. There was no constant decrease of the carbonic acid exhaled within a certain space of time, as one would be inclined to infer from Vierord's law, that the quantity of carbonic acid disengaged from the blood increases or decreases in an inverse proportion to the quantity of carbonic acid contained in the surrounding air. The absorption of oxygen in mammalia, breathing in a confined space, may be at first increased, but later, with the progress of the dyspnœa, it becomes decidedly diminished, and even more so than the exhalation of the carbonic acid. The amount of nitrogen in the air of the apparatus remained in some of the experiments almost unchanged; a decided diminution was not met with in any single instance; a distinct increase, on the contrary, was observed in the greater number of experiments, especially in those on rats and mice.

4. Pettigrew, in the Croonian lecture delivered at the Royal Society, states that he considers the left ventricle of the heart to be the typical one, the right ventricle forming only a segment of the left one. By a little care, he remarks, one can unwind, as it were, the muscular substance, and easily separate the walls of the left ventricle into several layers, each of which is characterized by a difference in direction. The author assumes nine layers—viz., four external, one central, and four internal. He describes how the external fibres are continuous with the internal fibres at the apex, and also at the base. Concerning "the direction of the fibres, he showed how there is a gradational sequence in the direction of the fibres constituting the several layers. Thus, the fibres of the first layer are more vertical in direction than those of the second, the second than those of the third, the third than those of the fourth, and the fourth than those of the fifth, the fibres constituting which layer are transverse, and run at nearly right angles to those of the first layer. Passing the fifth layer, which occupies the centre of the ventricular wall, and forms the boundary between the external and internal layers, the order of things is reversed, and the remaining layers—viz., six, seven, eight, and nine—gradually return to the vertical in an opposite direction, and in an inverse order." This change in the direction of the external and internal fibres Pettigrew accounts for "by the law of the double conical spiral;" for the explanation of which law, however, we must refer to the paper. The fibres, he states, do not form simple loops pointing towards the apex, as generally supposed, but twisted continuous loops pointing alike to apex and base. "From this

* Cl. Bernard: **Leçons sur les Effets des Substances Toxiques et Médicamenteuses, p. 118. Paris, 1857.**

arrangement," the lecturer continues, "it follows that the first and ninth layers embrace in their convolutions those immediately beneath them, while these in turn embrace those next in succession, and so on, until the central layer is reached—an arrangement which may in part explain alike the rolling movements and powerful action of the ventricles." Pettigrew then described the structure of the right ventricle and of the septum. In corroboration of the view, that the left ventricle is the typical one, the right only a segment, he referred to the shape of the ventricles, as shown by casts of their interior; the left yielding a perfect right-handed conical screw; the right, although having the same twist, representing only an incomplete portion.

IV. Blood; Lymph; Chyle; Nutrition; Metamorphosis of Matter.

1. Zimmermann: *On Richardson's Hypothesis regarding the Cause of the Coagulation of the Blood, and some other Views regarding Fibrin.* (Zeitschr. für Rat. Med., vol. viii. p. 304, 1860.)
2. Bischoff: *On the Process of Nutrition in Man and Animals.* (Archives Génér. de Méd., Août, 1860, p. 129.)
3. Lawes and Gilbert: l. c. sub. ii.
4. May: *At which Temperature do Cows derive the greatest Advantage from their Food.* (Moleschott's Untersuchungen, vol. v. p. 319.)
5. Henneberg: *Experiments on the Feeding of Oxen.* (Journal für Landwirthschaft, 1859; and Canstatt, l. c. p. 69.)
6. Schmidt and Stürzwage: *On the Influence of Arsenious Acid on the Metamorphosis of Matter.* (Moleschott's Untersuchungen, vol. vi. p. 283.)
7. Pavy: *On the Alleged Sugar-forming Function of the Liver.* (Vid. sub. v.)
8. Schottin: *On Kreatin and Kreatinin.* (Vid. sub. v.)

1. Zimmermann considers Richardson's explanation regarding the cause of the coagulation of the blood as insufficient. Although he does not deny that the volatile ammonia contained in the blood, lymph, and chyle may have some influence on their fluid state, yet their coagulation, he argues, cannot be attributed to the escape of the ammonia, because the blood coagulates in spite of the presence of this substance—as, for instance, when blood has been received under mercury and oil, or even after a small quantity of caustic ammonia has been admixed, in which case, though it does not coagulate at once, yet it does so after some hours, when there is still distinct proof that the ammonia has not yet escaped. The theory in question, the author states, is further inadequate to explain the majority of those coagulations which occur in the living body. Zimmermann's own theory is, that the process of coagulation is caused by the formation of a catalytic substance (*contact-körper*), which effects a molecular change in the fluid fibrin, disposing the latter to pass into the solid state. The author cannot give an accurate description of this catalytic substance, but thinks that it exists and remains in a dissolved condition, and that it has only *in statû nascenti* the power to act *per contactum* on the fibrin.

2. Bischoff's essay gives an abstract of the author's views, which are more fully discussed in his work, lately published, 'On the Laws of Nutrition of Carnivores.' As this work will shortly be more fully reviewed in this Journal, we will now only mention that Bischoff endeavours to show the relation of the azotized and not azotized articles of food to the animal organism, the manner in which they maintain the growth and existence of that organism, and the changes they undergo in doing this.

3. We have reported already on Lawes and Gilbert's paper, as far as it relates to dietetics; but the results obtained by the authors on the process of *fattening* are of more especial value to the physiology of nutrition. Regarding

the *increase* of the weight of animals during the last months of fattening, it is estimated to contain in oxen from 70 to 75 per cent. of total dry substance; of which 60 to 65 parts are fat, 7 to 8 parts nitrogenous substance, and 1 to $1\frac{1}{2}$ mineral matter. Similar are the proportions in sheep and pigs, but the percentage of fat is larger, that of nitrogenous substance and mineral matter smaller. In many instances the authors have exactly determined the amount and composition of the food consumed during the fattening process, and have compared it with the constituents estimated to be stored up in the increase. Thus they found that in sheep, for 100 of collective dry substance of food consumed, there will be about 9 parts of dry matter in the increase stored up. Of these about 8 parts are fat, and only 0·2 part mineral matter, and 0·8 nitrogenous compounds. The average of all estimates relating to pigs showed, for 100 parts of dry matter of food consumed, $17\frac{1}{4}$ parts of dry increase, of which $15\frac{3}{4}$ parts were estimated as fat, and rather more than $1\frac{1}{3}$ part nitrogenous substance. The authors especially mention the important fact that "there were four or five times as much fat stored up in increase, as there was of fatty matter supplied in the food." There was therefore obviously a *formation of fat* in the body. On the whole, by the fattening of animals, the proportion of the nitrogenous compounds of the entire body is diminished, that of fat is increased.

4. May endeavoured to learn at which temperature cows derive the greatest advantage from the food consumed. He made on two cows four series of experiments, each of them extending over a space of ten days; the temperature being kept in the first series at 41° Fahr. (4° Réaumur), in the second at 54·5° Fahr., in the third at 65·75° Fahr., and in the fourth at 59° Fahr. The quantity and quality of food consumed was in all experiments almost the same; the greatest increase of weight took place at the temperature of 54·5 Fahr., the smallest at that of 65·75° Fahr.; the best milk was obtained likewise at the temperature of 54·5°, which temperature was also connected with the best state of general health. A slightly lower temperature than 54·5° Fahr. appeared likewise to be beneficial. The amount of the fæcal excretion was greater during the increased temperature, that of the urine smaller, while the quantity of fluid ingested was larger in the higher than in the lower temperature. The amount of solids in the urine appears, however, not to have been ascertained.

5. Henneberg and Hohmann have, according to Canstatt's report, instituted researches on the conditions of nutrition in oxen. We have not been able to obtain their communication, but it appears to contain valuable determinations of the ingesta and egesta. The temperature of 61·2° Fahr. seemed to the authors the most conducive for the nutrition of oxen.

6. Schmidt and Stürzwage examined on common fowls, pigeons, and cats, the influence of arsenious acid on the metamorphosis of matter. They found that small doses of arsenious acid cause a considerable diminution in the excretion of carbonic acid and urea (from 20 to 40 per cent.), indicating a marked diminution in the metamorphosis of matter. This effect is produced more rapidly when the arsenic is injected into the veins; more slowly, but not less intensely, when it is introduced into the digestive canal. This diminution of the change of matter the authors regard as the cause of the increase of weight in horses treated with small doses of arsenic.

V. Liver; Skin; Urine; Fæcal Excretion.

1. Pavy: *On the alleged Sugar-forming Function of the Liver.* (Proceed. of the Roy. Soc., vol. x. p. 528, 1860.)
2. Schlossberger: *The Bile of the Kangaroo.* (Annal. de Chemie et Pharmac., vol. cx.; and Canstatt, loc. cit., p. 242.)
3. Waller, A.: *Experiments on some of the various Circumstances influencing Cutaneous Absorption.* (Proc. of the Roy. Soc., vol. x. p. 122.)
4. Planer: *On the Gases of the Urine and the Transudations.* (Zeitsch. d. Gesellsch. d. Wiener Aerzte, 1859; and Canstatt, loc. cit., p. 243.)
5. Haughton, J.: *On the Natural Constants of the Urine.* (Dublin Quarterly Journal of Med., vol. xxviii. p. 17, and Vol. xxx. p. 1.)
6. Hassall, A. Hill: *On the Frequent Occurrence of Phosphate of Lime in the Crystalline Form, and on its Pathological Importance.* (Proceed. of the Roy. Soc., vol. x. p. 281, 1860.)
7. Schottin: *On the Excretion of Kreatinin and Kreatin through the Urine.* (Archiv f. Heilkunde, Jahrgang 1860, p. 417.)

The work of Dr. Parkes 'On the Urine,' which contains, in addition to a very complete abstract, much new matter on the subject, will be more fully considered in another portion of this journal.

1. Pavy's recent communication to the Royal Society, on the alleged sugar-forming function of the liver, contains, besides a confirmation of the results of his former experiments,* some additional matter. Thus he shows by analysis that although the blood collected from the right side of the heart after death, as was formerly done, affords an abundant indication of the presence of sugar, yet that when it is removed from the same part by catheterism during life, it is found to contain but a trace of the saccharine principle. Inferences therefore that have been drawn regarding the ante-mortem state, the author argues, from the post-mortem examinations, must be abandoned as erroneous. Pavy reminds us at the same time that slight causes, as disturbance of the respiration, are sufficient to induce a strongly diabetic urine; that therefore a fair specimen of blood from the right heart can only be obtained when the animal is in a quiet state during the performance of the catheterism.

Pavy's experiments on rabbits led to the same results as those previously performed on dogs,†—viz.: that injection of starchy and saccharine matter is followed by a great accumulation of hepatine (the sugar-forming substance of Bernard and others) in the liver. While sugar diffuses easily through animal membranes, hepatine has a very low power of diffusion. This opposition strengthens the author in his view that hepatine is not normally transformed into sugar in the living organism. Pavy finally communicates that after the introduction of large quantities of carbonate of soda into the blood lesions of the sympathetic system do not cause diabetes.

2. Schlossberger's analysis of the bile of the kangaroo yielded for 100 parts 85·87 water and 14·13 solids. The solid constituents consisted of mucus and colouring matter, 4·34; cholesterin and fat, 1·09; biliary acid salts, 7·59; other salts and loss, 1·11. The principal base of the biliary acid salts was soda; they contained 2·47 per cent. of sulphur. Schlossberger infers from this that the bile of the kangaroo contains, with the exception of the pig's bile, less taurocholic acid than the bile of any other animal hitherto examined.

3. Waller's researches on cutaneous absorption lead to results which appear to us important with regard to practical medicine. The subjects of the experiments were guinea-pigs and albino rats. When the leg of a half-grown

* Conf. this Journal, No. 45 p. 171, 1859; and No. 49, p. 236, 1860.
† Conf. our Report in No. 45 of this Journal.

guinea-pig was immersed into a mixture of equal parts of chloroform and tincture of aconita, the part was after fifteen minutes insensible, and the symptoms of poisoning by aconite soon followed—viz., nausea, efforts at vomiting, coldness of surface, weak circulation, laborious respiration, slight convulsive symptoms, and death. The immersion in simple tincture of aconite without chloroform did not cause any of the symptoms of poisoning. If, however, the sciatic nerve had been previously divided, then the immersion of the leg in the simple tincture of aconite was sufficient to poison the animal, evidently through the distension of the capillaries and the more rapid flow of blood caused by the division of the nerve. A ligature placed round the limb before the first symptoms of poisoning had appeared, prevented the toxic influence on the system, but it rarely did so after the appearance of the earliest symptoms. In albino rats the immersion of the leg in a solution of atropia in chloroform caused dilatation of the pupils already after two minutes and a half in young animals, after five minutes or later in old animals. If turpentine was substituted for chloroform, the dilatation of the pupils did not occur while the leg remained immersed in the fluid, but appeared immediately after the removal of the limb. The substitution of alcohol for chloroform as a solvent causes great retardation of absorption. An immersion of twenty to thirty minutes produced only very slight effects. Still more slow is the absorption when the atropia is dissolved in water with a slight addition of acetic acid, or when the watery extract of belladonna is rubbed over the leg. The immersion of the foot of a young rat in a solution of morphia in chloroform caused after about five minutes somnolency and great dilatation of pupils, which latter phenomenon certainly must appear very remarkable when we consider the effect of morphia, given in the usual way, on the pupil. Three minutes' immersion in a solution of strychnia in chloroform produced dilatation of the pupil, and after five minutes, the well-known symptoms of strychnia-poisoning manifested themselves. No effect was obtained when a solution of strychnia in alcohol was employed. These experiments clearly show how important is the choice of the menstruum in the endermic application of some medicinal substances.

4. Planer has determined the amount of gases in the urine and transudations. His tables show that the amount of free carbonic acid varies in 1000 c. c. (cubic centimetre) from 45 c. c. in the fasting urine to about 100 in the urine of food, while that of the combined carbonic acid varies in the same urines from 20·7 c. c. to 52·5 c. c. The quantity of nitrogen appeared to be constant, amounting to 8 c. c. in 1000 c. c., that of oxygen scarcely was sufficient to be traceable. By the ingestion of bitartrate of potash the proportion of free carbonic acid was increased threefold, that of the combined carbonic acid remaining unchanged. The neutral tartrate of potash caused a considerable increase of the quantity of urine, with a diminished proportion of combined carbonic acid, and without much alteration of the percentage of the free carbonic acid. Diseases with pyrexia are attended with a urine rich in free carbonic acid and salts of carbonic acid. The absorbing power of urine for carbonic acid and oxygen is, according to Planer's experiments, about the same as that of water; the urine is therefore by no means saturated with these gases. The peritoneal transudation fluid of a patient affected with ascites and cirrhosis of the liver contained the gases in about the same proportion as the urine.

5. Dr. Haughton divides the work done in the body into—1, vital work; 2, mechanical work; 3, mental work; 4, calorific work. All the work done is attended with a certain amount of tissue change, which will furnish a certain amount of excretions. The author's present communication relates only to the excretion of urea, which he considers as the product of vital, mechanical, and mental, but not of calorific work. He has arrived at the following inferences: "1. The quantity of urea passed per day by men in health varies with their

food and occupation, the latter being the principal cause and regulating the other." "2. Men employed only in manual and routine bodily labour are sufficiently well fed on vegetable diet, and discharge on an average four hundred grains of urea per day, of which three hundred grains are spent in vital and one hundred grains in mechanical work. This conclusion is in conformity with the experience of the mass of mankind employed in manual labour in all ages and countries." "3. When the work is of a higher order, a better quality of food must be supplied, sufficient to allow a discharge of five hundred and thirty-three grains per day of urea, of which three hundred grains, as before, are spent in vital work, and two hundred and thirty-three grains in mental work and the mechanical work necessary to keep the body in health." "4. The quantity of urea discharged per day varies also with the individual which influences the vital and mental work." The vital work furnishes in twenty-four hours two grains of urea for every pound, according to the author's calculation. "5. The habits, weight, and occupation of the individual enable us to account for a range of the diurnal quantity of urea, varying from three hundred to six hundred and thirty grains per day; and this discharge may be confidently predicted when the habits and weight are known." We have mentioned the author's conclusions in his own words, but must refer to the paper itself for the calculations on which they are based, as also for the observations, which appear to extend over too short a space of time, considering the difficulties necessarily connected with the subject of the inquiry.

6. While the earthy phosphates of the urine are in general described collectively, and most importance is attributed to the phosphate of ammonia and magnesia, Hassall points out that the phosphate of lime is very frequently present as a deposit in urine, even much more so, according to his experience, than the triple phosphate, "excluding those cases of the occurrence of that ammonial phosphate, arising from the decomposition of the urea of the urine subsequent to its escape from the kidneys." The phosphate of lime is, further, when it is mentioned, considered as occurring in the amorphous and granular, and never in the crystalline state. Hassall shows, on the contrary, that there does occur in the urine a crystallized phosphate of lime, being a tribasic phosphate containing two atoms of lime, and most probably one of water. "The size, form, and arrangement of the crystals of phosphate of lime, as they occur in the human urine, vary greatly; but the peculiarities are in all cases sufficiently characteristic to allow of the ready identification of this phosphate by means of the microscope. The crystals are either single or aggregated, most frequently the latter, forming glomeruli or rosettes, more or less perfect. Sometimes they are small and needle-like, and then they frequently form, by their crossing and union at right angles, glomeruli or spherules. Sometimes the crystals are thin and flat, having oblique or pointed terminations. Very frequently, however, they are thick, and more or less wedge-shaped and united by their narrow extremities, so as to form more or less complete portions of a circle; the free larger ends of the crystals are usually somewhat oblique, and the more perfect crystals present a six-sided facette." The "penniform" crystals described by Golding Bird as a variety of the magnesian phosphate, represent, according to Hassall, a modification of the crystals of phosphate of lime. With regard to the characters of the urine depositing crystallized phosphate of lime, it is usually pale; it is passed in large quantity, with frequent calls to void it, and more or less local uneasiness occasioned by its passage; the specific gravity varies, but is generally below the average, although the animal matter and urea are absolutely in excess. There is a feeble degree of acidity, but the urine becomes soon alkaline. The crystals of phosphate of lime deposit usually before the urine has become alkaline, and should, for the sake of analysis, be separated at once, before the other substances (the triple phosphate, the carbonate and oxalate of lime) are mixed with them. The

author has noticed that the occurrence of this deposit, when it is persistent, is usually connected with marked impairment of health, "and this often where organic disease does not exist." Great disorder of the digestive organs, distressing headaches, debility, emaciation, great irritability of the nervous system, weakness of sexual powers, a slow and feeble pulse, and cold skin, are given as the prominent symptoms.

7. Schottin, having during several years bestowed much attention on the circumstances under which kreatin and kreatinin appear in the urine, considers that these substances are chiefly derived from the tissue-change of the striped muscles. The author inclines to the view, that kreatin and kreatinin pass by further oxidation into urea. In healthy urine he found, while merely vegetable food was consumed, no kreatin or kreatinin, and only a very small quantity under the influence of an almost entirely animal diet. In many diseases, too, these substances were not found; in some pathological conditions, however, in which either the transformation of these bodies into urea appeared prevented (uræmia), or in which the waste of muscular tissue was increased (as in typhoid fever), the amount of kreatin excreted by the kidneys ranges between 0·2 and 1·1 grammes (about 3 to 17 grains) within the twenty-four hours.

VI. Nervous System.

1. Wagner, R.: *Critical and Experimental Researches on the Functions of the Brain.* (Göttinger Nachrichten, 1860, No. 7; and Henle's Bericht über d. Fortschritte d. Medic., in 1859.)
2. Durham, A. E.: *The Physiology of Sleep.* (Guy's Hospital Reports. Third Series, vol vi. p. 149, 1860.)
3. Brown-Séquard: *Experimental Researches on various questions concerning Sensibility.* (Proceedings of the Royal Society, vol. x. p. 510, 1860.)
4. Beale, L. S.: *On the Distribution of Nerves to the Elementary Fibres of Striped Muscles.* (Proceedings of the Royal Society, vol. x. p. 519, 1860.)
5. Lallemand, Perrin, and Duroy: *On the Comparative Action of Alcohol, the Anæsthetics, and the Carbonic Gases on the Cerebro-spinal Nervous System.* (Compt. Rend. vol. li. p. 400, 1860.)

1. R. Wagner infers, as well from his own researches on the weight of the brain of mentally-favoured persons, as also from those of older authors, that the brains of intelligent individuals cannot be proved to be heavier than those of mentally less developed people. Wagner divides the brains into two groups, those poor in convolutions and those rich in convolutions, in order to appreciate the relation of the convolutions of the great hemispheres to the intellectual condition of the brains; he acknowledges, however, that this division is by no means a strict one, as the characters of the one group gradually pass over to those of the other. The author found great development of convolutions and great weight of brain often combined, but not always. Slight development of convolutions is more often met with in women than in men, the greatest degree of development was found only in men. The brains of some highly distinguished men, as that of Gauss, the mathematician, exhibited, certainly, the greatest degree of development among all brains examined, but, on the other side, the brains of some very intelligent men were among those poor in convolutions.

2. Durham defines sleep, *psychologically*, as a state in which volition, sensation, and consciousness are suspended, but can be readily restored upon the application of some stimulus, and *physiologically*, as the period of the brain's repose, associated with the nutrition and repair of the brain substance. The author thinks that his experiments on dogs prove, that pressure of distended veins upon the

brain, is not the cause of sleep, as has been supposed, but that during sleep the brain is in a comparatively bloodless condition; and that the blood in the encephalic vessels is not only diminished in quantity, but moves with diminished rapidity. The cerebral circulation during waking, when the quantity of blood and the rapidity of its motion are increased, Durham signifies as the circulation of function, that occurring during sleep as the circulation of nutrition, the former being favourable to endosmosis, the latter to exosmosis. The question regarding the proximate cause of sleep or of the temporary suspension of cerebral activity which follows each period of healthy mental exercise, the author is inclined to answer by assuming, from analogical facts, that the accumulation of the products of decomposition of brain tissue, caused by its functional activity, interferes with the continuance of its action.

3. Brown-Séquard has examined how long sensibility lasts in parts of the body deprived of the circulation of blood, by applying ligatures on the femoral artery of animals; and after having divided this vessel between the ligatures, amputating the thigh completely, with the exception of the two large nerves. The author thus found the duration of sensibility in the toes of rabbits to vary between twenty and twenty-three minutes; in guinea-pigs, between forty and fifty minutes, sometimes even more than an hour; in dogs, between thirty and thirty-five minutes.

Brown-Séquard further inquired into the influence of temperature on the duration of sensibility in parts deprived of the circulation of blood, and shows by his experiments that the lower the temperature the longer sensibility persists in parts deprived of circulation.

The same author tried to solve the question whether augmentation in the vital properties of the spinal cord is able to influence the duration of sensibility in limbs deprived of the circulation. He therefore divided, in one series of experiments, the posterior columns of the spinal cord *before* amputating the hind limb, except the nerves; while in another series he divided the part of the spinal cord mentioned *after* having made the amputation. In both series the sensibility was found to last notably longer than in animals in which the posterior columns had not been divided. The author explains this by the fact that, after section of the posterior surface of the spinal marrow, all the parts of the body which are behind the section become much more sensitive than they were previous to the operation. A remarkable fact mentioned by Brown-Séquard in connexion with this question is, that in a rabbit in which the spinal cord is in a normal condition, and in which the toes, after partial amputation, as in the experiments described, have almost lost their sensibility, there is "a very notable return of this vital property" when the posterior columns of the spinal cord are divided in the dorsal region. "These experiments show," the author reasons, "that when sensibility seems to be lost in a part deprived of circulation, it is not completely so, but that the transmitted excitation which causes sensation is too slight to produce it, and that if, in its way to the sensorium, this excitation meets with a cause of increase, then sensation can be produced by it."

4. Beale's interesting researches show that every elementary muscular fibre is abundantly supplied with nerves, which form a network and lie upon the surface of the sarcolemma without penetrating it. The muscular fibres of mammalia and birds receive a larger supply than those of reptiles and fishes. The nerves run with the smaller arteries, and are in very close relation with the capillary vessels. The nerve-fibres in muscle, and in other tissues, may be traced into, and are continuous with, a network formed of oval nuclei and intermediate fibres. These little oval nuclei the author considers as "the agents concerned in the development of nerves and the formation of new fibres," and "the organs by which nerves are brought into relation with other textures." A great number of these bodies is associated, according to Beale,

with exalted nervous action, while their presence in a small proportion indicates that the nervous phenomena are only imperfectly performed. The manner in which the nerve-fibres are connected with the network of the small bodies and intermediate fibres mentioned, suggests to the author the idea "that an impression made upon a given portion of a sentient surface might be transmitted to the nervous centre by contiguous fibres, as well as by the one which would form, so to say, the shortest route; and it is possible that impulses to motion may be conveyed to muscular fibres by a more or less circuitous path, as well as by the direct one." Beale's paper contains other valuable information, which, however, belongs more strictly to our half-yearly micrological report.

5. Lallemand, Perrin, and Duroy have communicated to the Académie des Sciences the results of their experiments on animals regarding the action of various anæsthetic agents and the carbonic oxide gas and carbonic acid. They consider alcohol, ether, chloroform, and amylene as true anæsthetics, acting primarily on the nervous system, and only indirectly, through the medulla oblongata, on the respiration. The carbonic oxide and the carbonic acid, on the contrary, act primarily on the blood, change it in its constitution, and only through this alteration of the blood they influence indirectly the nervous system. These gases, therefore, the author designates as pseudo-anæsthetics. Thus these researches corroborate the inferences drawn by Flourens soon after the introduction of etherization.*

HALF-YEARLY REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.
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I. *Cure of Cutaneous Diphtheria by Perchloride of Iron.* (Gazette des Hôpitaux, July, 1860.)

The following case is doubly interesting, both as an example of the effect of a mustard-plaister on the skin, under the influence of a diphtheritic epidemic, and as an example of the good effects of the internal and external use of perchloride of iron. A man, forty-two years of age, of a strong constitution, was seized, in November, 1859, with a subacute form of bronchitis, and as he became delirious in the course of the complaint, Dr. Noir, who was attending him, ordered mustard-plaisters to the calves of the legs. The delirium ceased, but the mustard produced a vesication, which disturbed the patient all night. The next day Dr. Noir opened the vesicles without removing the epidermis, and a quantity of serum escaped, similar to that of blisters. There was great subsequent irritation, however, and Dr. Noir, investigating the cause of the patient's sufferings, discovered, instead of vesicles, two enormous diphtheritic patches, one on each calf; these patches were of a greyish-white colour, irregular and dry, sufficiently hard to give a sound when struck by a spatula, both surrounded by an erysipelatous areola, tending to enlarge, and more painful than the patches themselves. There were at the time several cases of diphtheria among children in the neighbourhood. Dr. Noir cauterized all the areolæ with nitrate of silver, in order to prevent the disease from spreading; and he also ordered a draught to be taken, containing twenty drops of tincture of perchloride of iron at frequent intervals. The diphtheritic patches and the inflamed skin surrounding them were washed with a solution of iron. By this treatment, the progress of the disease was arrested and the pain was relieved, and after a few days a

* Comptes Rendus de l'Acad., tom. xxiv. 1847.

decided improvement was manifest. The diphtheritic patches were gradually thrown off, and eventually two ulcers of a healthy appearance were left, which proceeded to cicatrization without any serious symptoms.

II. *On the Therapeutical Employment of the Double Iodide of Iron and Quinine.* (Gazette des Hôpitaux, July, 1860.)

Dr. Violette has given the double iodide of iron and quinine in several affections in which the preparations of iron have been recommended, and he has been enabled to prolong its use without meeting with the symptoms so often produced by the separate use of iron and quinine. He has never observed either weight at the stomach or offensive eructations, or gastralgia. In many affections of the uterus, after the local treatment has succeeded, the iodide of iron and quinine has seemed to restore the strength and revive the appetite. He has witnessed the same results in convalescence from typhoïd fever, and from all diseases which induce a considerable impoverishment of the blood. Dr. Violette considers this medicine peculiarly efficacious in chlorosis. M. Becquerel allowed him to administer it to some young chlorotic girls, whose leading symptoms were complete loss of colour of the mucous membranes, a waxy complexion, excessive weakness, total loss of appetite, blowing murmur in the heart and carotids; in such cases Dr. Violette found that a rapid improvement followed the use of the double iodide. The appetite returned very quickly, and at the same time the strength was restored; a slight rose colour replaced the pale tint of the mucous membrane, and the extreme paleness of the face was changed for the natural colour.

III. *Cases of Severe Purpura Hæmorrhagica successfully treated by Perchloride of Iron.* (L'Union Médicale, Aug. 21st and 30th, 1860.)

Dr. Sassier, of Châlon-sur-Saône, was called to see a man aged seventy, who had been seized suddenly with depression, nausea, and shiverings, and three days after these preliminary symptoms there followed epistaxis, hæmatemesis, and hæmaturia; the patient lost blood both by the gums and the rectum. At the same time petechiæ and ecchymoses were developed on the trunk and the limbs. Iced drinks were ordered, together with dilute sulphuric acid and extract of rhatany, but without success, and indeed the symptoms seemed to be increased. The hæmorrhage continued, the tongue became dry and black, and the prostration was extreme. Dr. Sassier then prescribed the perchloride of iron, dissolved in distilled water and sweetened with syrup, to be taken in spoonfuls every hour. The next day the patient's state was the same, but on the succeeding day there was a sensible diminution of the hæmorrhage, which ceased on the third day, but the perchloride was continued for two days longer. The disease seemed to be cured, but a week afterwards the hæmorrhage reappeared, and the perchloride was again ordered, and after it had been employed two days the bleeding entirely ceased, and was never again renewed. The patient recovered after a prolonged convalescence.

Dr. Bertet relates another very severe case of purpura hæmorrhagica treated successfully by the perchloride of iron, and in this case the remedy was employed to the exclusion of all other medicinal agents. Dr. Bertet considers that at present the perchloride of iron is the best remedy for purpura hæmorrhagica, and that in some cases it is almost infallible.

IV. *Severe Case of Primitive Gangrenous Angina treated successfully by Perchloride of Iron.* (L'Union Médicale, Sept. 6th, 1860.)

Dr. Henry Musset relates a case of gangrenous disease of the back of the mouth which resisted all other remedies, but yielded to the employment of perchloride of iron. This medicine was administered in a draught at periodical intervals, and under its use the fever abated, the delirium ceased, and a large patch of ecchymosis which had appeared on the right side of the chest began to grow pale. Broth and wine were then administered, and every day the improvement continued; the breath was no longer fœtid, the tissues became clean, and at length the patient, who had appeared moribund, was restored to life. Dr. Musset attributes the recovery entirely to the use of the perchloride, because it was the only medicine employed internally from the time when the worst symptoms appeared, and because the improvement continued regularly as soon as its administration was begun. With regard to the patch of ecchymosis which appeared on the chest, the perchloride seems to have acted beneficially in removing it, whether this interstitial hæmorrhage was produced by the gangrene having destroyed some vein, or whether it was the consequence of an alteration of the fluids under the influence of a general cause. It is certain that under the use of the perchloride of iron both the gangrenous disease of the throat and the ecchymosis on the chest were completely cured.

V. *On the Employment of Sulphate of Quinine and Veratria in Typhoïd Fever.* By Dr. Vogt. (Bulletin Général de Thérapeutique, May 30th, 1860.)

Dr. Vogt has collected and tabulated a number of cases of typhoïd fever treated by sulphate of quinine and by veratria, and although the cases treated by the latter alkaloid were few, he prefers it on the whole to quinine as a remedial agent. He regards both alkaloïds as valuable in the treatment of typhoïd fever, but thinks that they are useful in different stages or conditions of the disease. The action of veratria, he says, is more direct, causes no excitement, and proceeds especially from the spinal cord; that of quinine proceeds principally from the brain, and cannot be manifested in a short time without previous excitement. Hence veratria is to be preferred in all febrile diseases which show a rapid and impetuous progress. In typhoïd fever it should be employed at the commencement, more particularly when the fever is very acute and the congestion in the head very active. In cases which are more slow in their progress, with a less intense febrile reaction, with hyperæmia more dependent on venous stagnation, and in the latter periods of the disease, when anæmia or adynamia predominates, then quinine is preferable. In some cases both remedies may be employed, the veratria being administered first, and the quinine afterwards. The antipyretic power of veratria is greater than that of quinine, for where the latter has failed, the former often succeeds in a very extraordinary degree. In the eruptive fevers, whenever their tendency is to a true inflammatory localization, or when this localization has already happened, veratria must be selected, from its singular efficacy in pneumonia and puerperal fevers with intense local inflammation. But if, on the contrary, the fever is idiopathic and entails no local inflammation, quinine should be employed. The toxical action of quinine is more marked and more difficult to avoid than that of veratria. In order to attain to the antipyretic effects of quinine, it is necessary to administer the alkaloïd till it produces the peculiar noises in the head, and although these noises are anticipated, they are the more worthy of attention, inasmuch as the vomiting produced by veratria is more frequently beneficial than injurious. Veratria, says Dr. Vogt, possesses, in typhoïd fever, a power far superior to that of quinine in relieving the head, especially in the first stage of the disease. But if the affection of the head depends upon venous congestion, quinine may succeed better than veratria. In a general manner it may be stated that veratria deserves the preference in many cases as an antipyretic; and as to the other indications for these two remedies, quinine ought to be preferred in marsh-fevers with a moderately rapid progress, and in other febrile diseases which have reached an advanced period, especially when anæmia and adynamia have already commenced, and especially when it is not essential to interrupt the febrile action in a shorter period.

VI. *On Digitaline and the Products of its Decomposition, and on the Action of Sulphuric Acid on Santonine, Guaiacine, and the Resin of Scammony.* (Bulletin Général de Thérapeutique, July 30th, 1860.)

In a thesis read before the Faculté des Sciences of Nancy, M. Constant Kosman has brought forward some new and interesting views upon the nature of digitalis and some other drugs. He describes minutely the preparation and purification of digitaline, and insists upon the necessity of separating it from some other nearly-allied bodies which always accompany it, and which may mask its characters. The result of his investigations is to prove that digitaline is divisible into two substances—namely, glucose, and a body hitherto undescribed, and which M. Kosman calls *digitaliretine.* This latter substance is almost insoluble in water, sparingly soluble in cold alcohol, but more so in warm, very slightly reddening litmus-paper, insoluble in alkaline fluids, and in other respects showing no remarkable reaction with different chemical agents. When digitaline is treated with caustic soda and boiled, and the liquid is then saturated with sulphuric acid, evaporated to dryness, and finally the residue is taken up with boiling alcohol, a distinct crystalline salt is obtained, containing soda and a peculiar acid called by the author *digitalinic acid.* The results of M. Kosman's inquiries may thus be summed up:—1. Digitaline is susceptible of being resolved into a new body, *digitaliretine* and glucose. 2. Digitaline, by the action of caustic soda, is transformed into a new body, the *digitalinic acid*, which may be resolved into glucose and digitaliretine, under the influence of acids. 3. Digitaline may exist under two conditions—anhydrous digitaline and hydrated digitaline. 4. Santonine may be resolved into a new body, *santoniretine* and glucose. 5. Guaiacine may be resolved into a new body, *guaiaretine* and glucose. 6. The resin of scammony may also be resolved into scammonial and glucose.

VII. *On the Comparative Value of the Antimonial and Arsenical Treatment in Chorea.* (Bulletin Général de Thérapeutique, Oct. 15th, 1860.)

A physician, Dr. Long, has lately published a thesis, in which he records the comparative results of the treatment of chorea by antimonial and arsenical preparations. His cases were collected from the practice of M. Bergeron and M. Boucher, in the Hôpital Ste Eugénie in Paris. Twelve cases were treated by large doses of tartar emetic, and eleven were treated by arsenic. The results of the antimonial treatment were, that out of twelve cases six were cured, and in five cases there was complete failure. The longest duration of the treatment was fifty-eight days, and the shortest was nine days. Twice, the emetic draught was not borne by the patient, and was suspended in consequence of the gastro-intestinal symptoms which it produced. Of the eleven cases treated by arsenic, all were cured under the influence of the medicine, and without any unpleasant effect upon the digestive canal; on the contrary, the patients found their general condition improved, and their strength and stoutness were restored. The longest duration of the treatment was forty-eight days, and the shortest was ten days. The editors of the 'Bulletin Général de Thérapeutique' believe that these results are sufficient to encourage practitioners to commence the treatment of chorea by the administration of arsenic, even if they have recourse to other medicines in case the arsenic is unsuccessful.

VIII. *Traumatic Tetanus cured by Tartarized Antimony in large doses.* (Bulletin Général de Thérapeutique, May 30th, 1860.)

The success which has attended the treatment of chorea by large doses of tartarized antimony (that is to say, by the strongest doses that the patient can bear without vomiting or diarrhœa) has induced Dr. Conway, of Neufchatel in Switzerland, to adopt this plan of treatment in traumatic tetanus; and he has succeeded in curing two patients by the administration of this medicine. The last case was that of a man, sixty-three years of age, who was seized with violent pain in the left hand, and particularly in the middle finger, which was affected with gangrene extending to its metacarpo-phalangeal articulation. In process of time the finger became dried up, and was removed by gentle traction; but very soon afterwards the patient complained of stiffness in the muscles of mastication, and it was remarked that he could not open his mouth. Dr. Conway immediately prescribed tartarized antimony in the dose of half a grain every hour and a half. The medicine caused severe alvine evacuations, but no vomiting until the next morning. The patient had still the same difficulty in opening his mouth, and complained besides of stiffness of the muscles of the nape of the neck. Two days after the attack, the trismus was extremely well marked, and the patient spoke only with difficulty. The tartarized antimony was now given more frequently—namely, half a grain every hour; but still the symptoms were increased in intensity. The patient then experienced a slight amelioration of his complaint, the stiffness of the neck having diminished. On the other hand, the tartarized antimony had produced acute pain in the back of the mouth. Dr. Conway then ordered twenty-four pills, containing sixteen grains of tartarized antimony, one to be taken every hour and a half. The next day after this prescription the patient was still worse; and as the constriction of the jaws did not allow him to swallow the pills any longer, he was ordered to take, every hour and a half, a teaspoonful of a solution of eight grains of tartarized antimony in sixty grammes of distilled water. It was necessary, however, to abandon this latter method of administering the drug, as it caused excessive pain in the back of the mouth, with singing in the ears, and a linctus with chlorate of potash was employed. The next day but one the tetanus still advanced, but the state of the mouth was improved under the use of the chlorate of potash, and the tartarized antimony was again administered every hour and a half, together with the chlorate of potash. This treatment was continued for seven days, after which time the patient complained of pain in the stomach, and the tartarized antimony was omitted; but the tetanic symptoms diminished from this period, and it was not necessary again to have recourse to the medicine. The case is remarkable from the perseverance which was shown in the use of the tartarized antimony, and from the successful results which ensued from its combination with chlorate of potash, the latter salt appearing to modify the injurious effects of the tartar emetic, and enabling the patient to continue the antimonial treatment much longer than he would have been able to do without such an adjuvant.

IX. *On the Treatment of Dyspepsia by Arsenious Acid.* By Dr. V. GERMAIN. (Gazette Hebdomadaire, July 20th, 1860.)

Dr. Germain, in a memoir presented to the Académie de Médecine, has advocated the use of minute doses of arsenious acid in the treatment of dyspepsia; and he believes that the beneficial effects of some mineral waters are owing to the presence of minute portions of arsenic. He administers it in the dose of a single milligramme each day, taken in the form of a pill at the beginning of a meal; and he makes no change in the diet of the patients, except to avoid excess, and to abstain from food which is positively indigestible. Dr. Germain relates seventeen cases, in all of which the administration of the arsenious acid was more or less beneficial; and he concludes that the dyspepsia is really cured by the use of the drug in minute and long-continued doses. The physiological action of arsenic thus administered is to cause the cessation of constipation when this symptom has existed, but diarrhœa does not supervene. The patient has his bowels regularly opened, to the great relief of his sufferings, and the stools are healthy. If, again, diarrhœa has been the prominent symptom, it is not succeeded by constipation. Dr. Germain believes, therefore, that the arsenic acts by strengthening the system, and restoring to the organs their healthy functions, and that it is thus analogous in its action to iron and iodine. The latter medicines are considered by some authors rather in the light of alimentary substances than drugs, and indeed they are found in many of the solid bodies which are used for food. Dr. Germain asks the question, whether arsenic may not have a similar office to perform in the maintenance of health? And as this metal is extensively diffused throughout nature, it may happen that the small quantities existing in drinking waters may, by their continuous operation on the system, exercise a beneficial action, and preserve the digestive organs in a healthy condition.

X. *On the Treatment of Blennorrhagia by Injections of Subnitrate of Bismuth.* (Bulletin Général de Thérapeutique, Sept. 30th, 1860.)

The subnitrate of bismuth has been lately recommended as a local application in blennorrhagia, but as the results were not altogether satisfactory, M. Mourlon has investigated the circumstances which prevent the success of this kind of medication. He found that the injections of the subnitrate of bismuth often irritated the urethra; and on testing the salt, he ascertained that it reddened litmus paper. In order to obviate the acidity of the injections, he caused the salt to be washed until it presented no acid reaction; and under these circumstances he has found the injection almost uniformly successful, and has introduced its use into the military hospitals. In a memoir published by M. Mourlon, he states that out of 37 cases, 32 were cured, and that the average duration of the treatment was twenty-one days. With the exception of four patients who came into the infirmary for relapses, all the cases presented blennorrhagia of five to eight days' duration, and they were all acute cases. In none of them did the inflammation extend to the deep parts of the urethra, so rapidly was it arrested in its progress by the subnitrate of bismuth.

XI. *On the Suppression of Suppuration, and the absolute Disinfection of Wounds by the Permanent Application to their Surface of a Sponge soaked in a Chlorinated Solution.* By Dr. HERVIEUX. (L'Union Médicale, Oct. 25th, 27th, and 30th, 1860.)

In a series of papers, Dr. Hervieux maintains the efficacy of sponges steeped in a chlorinated solution as an application to suppurating wounds; and, after quoting the authority of many surgical authors in favour of this method of treatment, and recording the results of his own experience, he arrives at the following conclusions:—1. The permanent application of a sponge steeped in a chlorinated solution to the surface of severe suppurating wounds has the effect of transforming them into healthy-looking sores of a vermilion tint, free from exuberant fungous granulations and from suppuration. 2. While this mode of treatment suppresses the suppurative process, it favours cicatrization, which is never more regular and more certain than in the absence of suppuration. 3. This application resolves in the negative the question whether suppuration is the method employed by nature to repair the physical lesion inflicted on the living parts. 4. Among all the disinfectants of suppurating wounds there is none more efficacious than the sponge soaked in a chlorinated solution, because it suppresses the very source of fetor—namely, suppuration and its products. 5. With very few exceptions, this application causes no appreciable irritation on the affected surfaces, or on the surrounding parts. 6. This application is most advantageously employed in the treatment of phagedenic gangrene, of the eschars which succeed to severe fevers, of eczematous, scrofulous, or bony ulcers, of hospital gangrene, of perineal laceration, and generally of all suppurating wounds of an unhealthy character.

XII. *On the Therapeutical Use of the Oxalate of Cerium.* By CHARLES LEE, M.D. (American Journal of the Medical Sciences, Oct., 1860.)

The oxalate of cerium was introduced into practice about a year since by Professor Simpson, of Edinburgh, as a remedy for the vomiting of pregnancy. It has subsequently been employed in the treatment of various gastric affections, both in Europe and America, and it promises to assume a permanent place among the mineral tonics. The oxalate of cerium is a white, granular powder, inodorous and tasteless, insoluble in water, alcohol, and ether, but freely soluble in sulphuric acid. Dr. Charles Lee employed it at first only in cases of advanced pregnancy, in which the vomiting had resisted all the ordinary remedies, such as creasote, hydrocyanic acid, ice, bismuth, &c. The dose is from one to two grains. As it was found very efficacious in repressing vomiting, Dr. Charles Lee tried its effects in fourteen cases of atonic dyspepsia, and uniformly with favourable results. The effects are very speedily manifested, and the improvement consists in the relief of the nausea and the restoration of the appetite. The rapidity of the therapeutic action was particularly insisted upon by Professor Simpson, and this peculiarity in its operation is confirmed by the observations of Dr. Charles Lee.

XIII. *On the Employment of Apiol in Amenorrhœa and Dysmenorrhœa.* By Dr. JORET. (Bulletin Général de Thérapeutique, Aug. 15th, 1860.)

Dr. Joret believes that apiol is the safest remedy for the stimulation of the uterus in cases of amenorrhœa and dysmenorrhœa, and that it may be employed even in cases of incipient pregnancy. Administered in doses of fifty centigrammes every day, it never occasions either thirst, vomiting, colic, or diarrhœa. It is necessary, however, that it should be administered at a suitable period in order to produce beneficial effects, and this period ought to be that of the return of menstruation. When the menstruation has disappeared, perhaps for several months, its return may be announced by certain premonitory symptoms, such as pain in the loins, heaviness in the hypogastric region, headache, with slight elevation of the pulse, nervous irritation, &c. These phenomena indicate congestion of the uterus and the approach of the menstrual period, and the opportunity now offers itself for the administration of apiol. The remedy may be taken in gelatine capsules, each capsule containing twenty-five centigrammes of apiol. Dr. Joret administers one capsule night and morning, during the continuance of the menstrual period of four or five days, and at the following monthly period he follows the same plan. If menstruation does not supervene after the apiol has been administered five or six days, it is better to postpone its further employment until the next period. The menses usually appear at the first dose of the medicine, and are more or less abundant in quantity, but are always unattended with pain. Dr. Joret considers that the efficacy of apiol in dysmenorrhœa is indubitable. In some of the cases which he has recorded, the result was immediate; a few capsules of apiol induced a sudden cessation of the pains in the loins, groins, and uterus. On the day of the administration of the medicine, and sometimes on the next day, the menses reappeared, and flowed abundantly and easily. Still the apiol does not always cure dysmenorrhœa, but even in the unsuccessful cases it always succeeds in removing the painful symptoms. Dr. Joret concludes by observing, that when amenorrhœa or dysmenorrhœa arises from a diminution or excess, or perversion of the vitality of the uterus with local or general nervous symptoms, then the apiol administered according to the rules laid down is the best and safest emmenagogue which can be employed. It is the stimulant and the moderator of the menstrual function, and may be always administered without danger.

XIV. *On the Use of Alum and Ice in Hæmatemesis.* By H. R. de Ricci. (Dublin Quarterly Journal of Medical Science, August, 1860.)

Dr. de Ricci having previously employed alum and ice successfully in hæmatemesis, publishes the details of a very severe case of this disease, in which he used the same remedies, and with the same fortunate result. The patient was a young man, about twenty-one years of age, who, without any remarkable preliminary symptoms, was suddenly seized with vomiting of blood, which, although at first dark and grumous, was quickly followed by more of a decidedly arterial character. The first remedy employed was turpentine, but it increased the vomiting, and was consequently abandoned. Gallic acid in full doses and acetate of lead were also administered, together with matico and other vegetable astringents, but without any good effects. The patient now fell into a very alarming state of depression, and appeared to be at the point of death, when, after administering brandy and water by the mouth, and an enema with chloric ether, it was determined to give a large dose of powdered alum, to pack the stomach with small pieces of ice, and to place a bladder filled with ice over the epigastrium. It was also resolved to administer brandy, ether, and other stimulants, as well as food, by the rectum, and to give no nourishment by the mouth until the bleeding from the stomach had ceased. Even this treatment appeared at first to be unsuccessful, but by its long and persevering application, and a continuance of absolute fasting for one hundred and forty hours, the patient became convalescent, and eventually recovered. Dr. de Ricci remarks that alum may perhaps be thought an improper remedy, because in large doses it generally acts as an emetic, but in the case related its emetic powers appear to have been counteracted by the ice.

XV. *On Cauterization by Sulphuric Acid in Neuralgia.* (L'Union Médicale, August 7th, 1860.)

M. Legroux has lately read to the Société Médicale des Hôpitaux de Paris a paper on the treatment of neuralgia by cauterization with sulphuric acid.

Two cases were related, in which Dr. Dubourg, of the hospital at Marmaude, had successfully employed this kind of cauterization; and M. Legroux stated that he had himself employed the same remedial measure in several cases of neuralgia, and among others in sciatica, in femoral neuralgia, and in intercostal pains. M. Legroux employed concentrated sulphuric acid, the action of which was limited by wiping the parts in the vicinity, and he applied it by means of a pencil made of charpie. This pencil, soaked in the acid, was passed from above downwards along the course of the pain, and brought back from below upwards in the same direction; but this application often produced deep ulcerations of the skin, which were treated with difficulty, and M. Legroux has therefore proposed a modified use of the caustic. He now wipes off, by means of a linen rag, the layer of acid deposited on the skin, and the relative thickness of the epidermis indicates the necessity of allowing some time to elapse before wiping off the acid, or of doing so immediately. By this plan the application may be repeated without inconvenience in case the action is insufficient, or there is a relapse of the complaint. The epidermis thus cauterized becomes of a yellowish white colour, and is soon surrounded by a slight red border; is somewhat swollen without being raised by the serosity, and then becomes blackish, and is detached in scales without leaving wounds or scars. This mode of cauterization has effected relief as rapidly as the more deep cauterization; but M. Legroux thinks, nevertheless, that the latter may be still required in some severe and obstinate cases.

XVI. *On the Use of Ergot of Rye in the Treatment of Retention of Urine from Paralysis of the Bladder.* By Dr. ALLIER. (Bulletin Général de Thérapeutique, Sept. 15th and 30th, 1860.)

The faculty possessed by ergot of rye of producing uterine contraction suggested to Dr. Allier the idea of employing this agent in the treatment of certain forms of paralysis of the bladder; and M. Paul Guersant has subsequently demonstrated, in his practice at the Bicêtre, that the ergot is really endowed with the property of exciting contraction of the bladder. He employed it not only in cases of retention of urine from simple paralysis of the bladder, but also in order to promote the expulsion of the detritus of calculi after lithotrity. In order to ascertain the physiological effects of this substance, Dr. Allier took several times, fasting, one gramme divided into four doses; the results were a little heaviness of the head, and a slight feeling of intoxication similar to that produced by champagne. Sometimes there was a desire to make water more frequently than usual, almost always a kind of uneasiness in the pelvic organs, which prevented sleep; at other times, on the following night there was a kind of involuntary jactitation and disturbed sleep, and the rhythm of the heart's pulsation was variable. Dr. Allier relates a number of cases treated by ergot of rye, and his general conclusions are as follows: In the first place, he finds that the innoxious character of the drug is fully established, although he administered it in large doses continued for a long time, and he is therefore induced to doubt the deleterious effects attributed to it by some writers, or rather he believes that there may be different kinds of ergot, some of which are poisonous and others not so. In four out of fourteen cases related, there was evident excitement of the generative organs, which could be attributed only to the use of the ergot, and nearly all the cases exhibited more or less alteration in the nervous system, the most constant effects being a kind of pleasing inebriation, itching of the skin, nausea, uneasiness in the lower limbs, slight convulsive movements, and especially excitement of the contractility of the bladder when this function had been weakened or lost. The ergot appears especially to have a kind of mysterious predilection for the bladder, of the same nature as that which is admitted to exist in the case of the uterus, and quite as inexplicable; it is, in fact, a special and transient excitor of the nervous system, appearing to act more particularly upon the lumbar portion of the spinal cord and the hypogastric plexus of the sympathetic, which themselves react upon certain orders of muscles, and especially on those of the bladder. Dr. Allier does not agree with M. Bonjean in classing ergot among the opiates; for, according to the former physician, ergot never produces stupefaction, such as is observed in advanced drunkenness, or in opium-smoking and opium-eating. The transient intoxication produced by ergot can only be compared to the effects of a glass of champagne. It may be admitted, however, that one of the principles of ergot—namely, *ergotine*—may present some analogy with opium in its stupifying power. Dr. Allier draws the following conclusions from his researches: Ergot overcomes retention of urine when it has not yielded to catheterism, and abridges the duration of those cases which would yield in time to the catheter. It has no efficacy in the treatment of retention caused by enlargement of the prostate. Paralysis of the bladder, resulting from cerebral hæmorrhage, yields rather easily to ergot; but this is not the case with paralysis of the limbs following apoplexy. Ergot is equally efficacious in vesical paralysis connected with an undetermined lesion of the nervous centres, but has no power over paralysis of the limbs dependent on the same lesion.

XVII. *Iodine Injections in Hepatitis.* (Journal de Médecine de Bruxelles, August, 1860.)

A married woman had suffered for several years from constipation, pain in the right hypochondrium, and a slight degree of icterus. Having been exposed to a heavy shower of rain, she was seized with acute hepatitis, which did not yield to antiphlogistic treatment. Obstinate vomiting of bile and undigested food was a predominant symptom, but the jaundice and the pains in the liver had increased at the same time, and there were, moreover, intense fever and slight delirium. As the stomach constantly rejected everything, and as other practitioners had obtained favourable results from the administration of iodine in chronic affections of the liver, M. Muhauser, who relates the case, prescribed iodine injections (of iodide of potassium with tincture of iodine), to be given every four hours in linseed tea. The vomiting speedily ceased; and after this favourable change, M. Muhauser ordered frictions with tincture of iodine over the region of the liver. After these measures had been adopted for twenty-four hours, an evident amelioration was observed, the volume of the liver was sensibly diminished, and it was now scarcely felt to pass beyond the false ribs. The fever diminished very much, and there was an alvine evacuation produced by an aperient injection, affording great relief. The same course having been continued, although the iodine was administered in weaker doses, all the symptoms gradually abated, and the patient recovered in a short time.

XVIII. *On the Treatment of some Spots on the Skin* (*Ephelis*) *by Tincture of Iodine and Bichloride of Mercury.* (Journal des Connaissances Médicales, July, 1860.)

Dr. Caffe, Dr. Gouriet, and Dr. Boinet have lately recommended the tincture of iodine as a local application in certain diseases of the skin, unattended with visceral derangement. Dr. Caffe advises that before treating these complaints, a careful diagnosis should be made, so as to distinguish the cases dependent upon internal causes from those which are altogether external. He classes *pityriasis versicolor* among the *ephélides*, and describes it as being unaccompanied with any derangement of the general health, but as causing itching and altering the colour of the skin. It is altogether a local disease dependent on the presence of the *microsporon*, a kind of parasitic sporule which is destroyed and removed under the influence of any local parasiticide, such as iodine or mercury. He employs a pomade made with the oxychloride of ammonia and mercury, or a lotion containing corrosive sublimate, alcohol, and distilled water, and other physicians strap the skin with bandages steeped in tincture of iodine.

XIX. *Abortive Treatment of Acute Phlebitis by the External use of Tincture of Iodine.* (L'Union Médicale, Oct. 25, 1860.)

Dr. Spoiret, of St. Petersburgh, has published several striking cases of phlebitis cured by the external use of tincture of iodine, although in a few instances the phenomena of pyæmia had been manifested. One of the most remarkable cases is the following. A strong peasant had been bled one day for pain in the side, and on the evening of the succeeding day there were pain, redness, and swelling, not only at the wound in the vein, but over the whole arm, and compresses of Goulard's extract did not arrest the progress of the disease. When the patient came into the hospital, five days after venesection, the arm and fore-arm were enormously swollen, and a livid bluish cord was observed as thick as the finger, hard to the touch, and painful on pressure, extending from the gaping wound in the vein to the hollow of the axilla. The fever was violent, the pulse hard; there was headache, with thirst and constipation. Sixteen leeches were applied, and other remedies employed, but without any improvement, and the patient fell into a typhous condition. Camphor, opium, and arnica were prescribed internally, and at the same time abundant frictions of tincture of iodine were employed on the skin of the arm along the venous cord. After two days of this treatment the swelling, the resistance, and the pain of the inflamed vein had diminished and the fever had abated. All internal treatment was now discontinued, but the application of the tincture of iodine was persisted in. Under the use of nourishing food the strength of the patient improved, the phlebitis underwent resolution, the wound cicatrized, and eventually the patient was completely cured.

XX. *On the Physiological Effects, and the Therapeutical Employment, of the Essential Oil of Valerian.* By M. A. Barrallier. (Bulletin Général de Thérapeutique, Sept. 30th, 1860.)

At the end of January, 1856, epidemic typhus prevailed at Toulon, and was characterized by stupor, somnolence, and coma, and great debility. As the usual remedies were administered without decidedly beneficial effects, M. Barrallier had recourse to valerian, which he employed in the form of essential oil. This essence pre-exists in the fresh or old valerian root, always containing about $\frac{5}{100}$ths of valerianic acid, and consisting of two oils—one a hydrocarbon, called *valerene*, the other an oxygenated hydrocarbon called *valerol*. The essence, as it is usually found in commerce, is of a yellowish colour, becoming afterwards brown and thick; but as prepared by M. Pierlot, a Parisian chemist, it is of a light straw-colour, unaltered by keeping; its smell is not disagreeable, and resembles that of fresh valerian root; its taste is sharp and bitter; it has an acid reaction from the valerianic acid which it contains. In order to ascertain accurately the effects of this substance, M. Barrallier administered it to persons in good health, and found that the following symptoms were produced—namely, diminution of the arterial pulsations at first, and their subsequent elevation in the greater number of cases; increased heat of skin; perspiration more marked than usual, with the smell of valerian; feeling of oppression in the temporal regions; cephalalgia, most commonly frontal, and sometimes very intense; diminution of muscular force; inaptitude for intellectual exertion; inclination to sleep; deep sleep; nausea and salivation in certain cases; dislike to food when the medicine was given in the dose of thirty to fifty centigrammes; and abundant flow of urine, more highly coloured than usual, with a smell of valerian.

With respect to the therapeutical action of the essence of valerian, M. Barrallier has employed it in the treatment of epidemic typhus, not only as a remedy for the somnolence and coma which occur at the end of the second week or the commencement of the third, but he has used it also at the very commencement of the disease in order to moderate the nervous irritation which exists at the outset of this kind of fever. He has found it to be most efficacious at the more advanced periods of the disease, when it appears to rouse the patients from their lethargic condition; and as to its curative powers, he believes that the sudden cessation of the symptoms has exercised a favourable influence on the progress of the malady. When the action of the essence is confined to a short period of wakefulness, and when somnolence and coma reappear rather suddenly, a fatal termination must be anticipated. The following are the effects produced by the essence of valerian in disease—namely, rapid awakening of the patient; eyes widely open; intelligence more clear, and correct answers given to questions; increase of the arterial pulsations at first, and subsequent depression; diminution in the quantity of urine, and slight perspiration. M. Barrallier has employed the essence of valerian also in cases of vertigo, hysteria, and neuralgia, with satisfactory results, and he recommends further observations to be instituted upon the therapeutical value of this preparation.

XXI. *On the Employment of Alcohol as an Abortive Agent in Intermittent Fevers.* By Dr. Jules Guyot. (L'Union Médicale, Sept. 11th, 1860.)

Dr. Guyot proposed to treat the epidemic cholera in 1849 by the administration of alcohol, and the disease was often arrested by the adoption of this plan, which was carried out with still greater success in the epidemic of the same disease in 1854. As Dr. Guyot regards cholera as a species of fever, he proposes the same treatment in intermittent fevers, the alcohol being administered at the commencement of the paroxysm. Several cases are recorded in which this mode of treatment was successful. In one case, a man was attacked with a tertian fever, and two paroxysms were allowed to pass without treatment; but at the commencement of the third, just when the cold shivering was most developed, two small glasses of rum were administered to him with immediate relief; he became warm and felt better, and a third glass of rum was given. In half an hour afterwards the patient was dressed and walking out in the air, and the paroxysm never returned. Several other cases were treated in the same manner with equal success. In the case of a gentleman who had contracted an intermittent fever in Africa, and who came on a visit to Dr. Guyot, the paroxysm was arrested by the same means. Dr. Guyot's treatment of intermittent fever is explained in the advice given to this gentleman when consulting Dr. Guyot for his complaint. "If," said Dr. Guyot, "you have still eight hours before the commencement of the paroxysm, take twenty centigrammes of sulphate of quinine, together with a cup of warm infusion of camomile, and in two hours repeat the dose. If you have only three hours, take three cups of strong coffee, and put your feet before a good warm fire. Lastly, if you have no time, or if you are not sure of the hour when the invasion of the paroxysm will happen, allow it to come on, and as soon as you are certain that it is fully developed, take two small glasses of rum, and five or ten minutes afterwards take a third." The last alternative was adopted, and with complete success.

XXII. *On Indian Hemp, particularly in relation to its property of producing Sleep.* By Dr. FRONMÜLLER. (Vierteljahrschrift für die practische Heilkunde, 1860.)

Dr. Fronmüller first employed the Indian hemp in the case of a phthisical patient in the year 1850, and since that time he has devoted himself to the especial study of the properties of this substance. The result has been the production of a treatise founded upon the clinical observation of no less than a thousand cases in which Indian hemp was administered. This plant has been very much extolled by many practitioners in various countries, but has lately fallen into disuse, owing to the supposed uncertainty of its operation. The discredit attached to it is attributed by Dr. Fronmüller partly to the contradictory statements published concerning its operation by various writers, and partly to the difference in its effects in the Eastern hemisphere compared with those observed in Europe. The Indian hemp of India and that grown in Europe present the same external form, but they differ in the relative proportion of narcotic resin which each contains, and which is the active principle of the plant. It appears that the amount of resin depends not only upon differences of latitude, but also upon the depression or elevation of the regions where the plant is grown. Chemical analysis has discovered that the Indian hemp contains gum, bitter extractive matter, albumen, chlorophyll, ethereal oil, and a peculiar resin. This resin is called *cannabin* by some writers, and forms six to seven per cent. of the dried plant. The ethereal oil has been obtained by Martius only in small quantity; it is of a slightly yellowish colour, of a peculiar ethereal camphor-like smell, and an aromatic astringent, and afterwards bitter taste. The preparations of Indian hemp hitherto employed are the powdered plant for pills or powders, resinous extract of hemp in powders or pills, tincture of the resin, and emulsion.

With regard to its application to the practice of medicine, Indian hemp may be considered valuable as a tranquillizing antispasmodic drug. It has been employed with favourable results in tetanus and trismus, cardialgia, rheumatism, and in some mental diseases. Of 1000 cases in which this drug was administered by Dr. Fronmüller, 552 were males and 448 females, and the patients were of various ages, from one year to fifty and more. The principal diseases of the patients were tuberculosis, inflammation, surgical diseases, rheumatism, diseases of the eyes, nervous diseases, and dropsy. The greater part of the cases were treated by the spirituous extract prepared by Merk in Darmstadt; but others were treated by the extract prepared by the late Jacob Bell, of London. It is to be observed that all the observations were made on cases in which there had been no sleep for several nights, and in which the continuance of sleeplessness was to be anticipated unless some narcotic was employed.

Out of the thousand cases it was found that the narcotic property of the hemp was completely developed in 530, partially in 215, and little or not at all in 255. With the extract of Indian hemp the best effects were produced 145 times with a dose of 12 grains, 64 times with a dose of 8 grains, 63 times with a dose of 10 grains, 35 times with 16 grains, 22 times with 3 grains, 17 times with 2 grains, 15 times with 14 grains, 14 times with 20 grains, 13 times with 6 grains, 12 times with 5 grains. The period of falling to sleep, and the duration of sleep in the cases, are numerically recorded by Dr. Fronmüller, and also the number of cases in which unfavourable results ensued on the day of taking the drug, or on the next morning. Comparative observations were also made with morphia in cases where the Indian hemp had failed. Out of 29 cases in which Indian hemp had produced no effect, sleep was produced by morphia in 24. The dose of morphia was in general rather a strong one—from $\frac{1}{8}$th of a grain to 2 grains—in order to induce sleep. In the remaining 5 cases the morphia produced no effect.

The conclusions to which Dr. Fronmüller arrives as the results of his observations are the following: 1. That Indian hemp, among all the known medicines which cause stupefaction, is that which produces a narcotism most completely supplying the want of natural sleep, without occasioning any great excitement of the vascular system, without special stoppage of the secretions, without the supervention of unfavourable consequences, and without subsequent paralysis. 2. That Indian hemp, on the other hand, is not so strong nor so certain in its operation as opium. 3. That Indian hemp may be given in all acute inflammatory diseases and in typhus fever. 4. That it is worth a trial to alternate the Indian hemp with opium in cases where the latter fails. 5. That the best mode of administration is the alcoholic extract in small pills which contain an addition of the powder of the Indian hemp. The lowest dose for producing sleep may be estimated as 8 grains given in pills of 1 grain each. [As the strength of the extract of Indian hemp sold by the best London druggists is somewhat variable, from the varying strength of the plants producing it, the drug should be employed with caution, and 8 grains of the *best* extract must be considered a large dose.—REPORTER.]

XXIII. *On Bloodletting as a Therapeutical Agent in Inflammation, especially Pneumonia.* By L. M. LAWSON, M.D., Ohio. (American Journal of the Medical Sciences, January, 1860.)

Dr. Lawson combats the opinions lately expressed by Dr. Bennett, of Edinburgh, as to the injurious effects of bloodletting in all cases of pneumonia, and while he admits the facts adduced by the Edinburgh Professor, he questions the soundness of the conclusions which are drawn from them. Dr. Lawson remarks that bleeding is not so much practised as it formerly was, but he attributes this change in our plan of treatment to an alteration in the type of disease; and he draws attention to the fact that Dr. Bennett's own opinions and practice have undergone a change in relation to this subject within a rather recent period, for in 1851 Dr. Bennett recommended bleeding in pneumonia in a clinical lecture published in that year in the *Edinburgh Monthly Journal.* The evidence of statistics is considered by Dr. Lawson to be unsatisfactory, owing to the numerous fallacies necessarily involved in the tabulation of cases and cures. Pneumonia requires different treatment according to the varieties of type which it may happen to present at different periods and under special circumstances of age, climate, and individual peculiarities. Dr. Lawson assumes the existence of the following varieties of pneumonia—namely, sthenic pneumonia, asthenic pneumonia, latent pneumonia, specific pneumonia (typhoid, miasmatic, &c.), and diathetic pneumonia (rheumatic, scrofulous, &c.). In these five species, the therapeutical indications will be found to be widely different, and it will be accordingly necessary to prescribe quinine and opium for one, to bleed in another, and to adopt specific treatment in a third. It is admitted that in the milder forms of pneumonia little treatment is required, and bloodletting may certainly be often omitted; but in the severer forms the remedial treatment must be more active. In the milder forms of all diseases, the powers of nature may be sufficient to overcome the morbid action, but in others the same powers must be protected from the destructive tendencies of over-action, while in a third class, characterized by debility, the powers of nature must be sustained by stimulating agents.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By John W. Ogle, M.D. Oxon, F.R.C.P.
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I. *On Hemicrania, or Migrän.* By E. Du Bois-Reymond. (Archiv für Anatomie, Physiologie u. Wissenschaftliche Medecin von Dr. Reichart und Dr. E. Du Bois-Reymond, Heft iv. p. 461. 1860.)

This highly interesting case related and commented upon, occurred in the person of Du Bois-Reymond himself, and therefore was carefully observed. It is adduced as a contribution to the literature of morbid phenomena dependent on implication of the cervical portion of the sympathetic nerve. After certain prefatory allusion to the general subject of the influence possessed by the nervous system upon the muscular structure of minute blood-vessels, and to the existing opposition between neuro- and cellular-pathology, &c., the author details his case, which may be condensed as follows:

Ever since he was twenty years old, although otherwise in good health, he has suffered every three or four weeks after any little irregularity (such as long fasting, over-wearying company, &c.) from general indisposition, and slight pain in the right temporal region on awaking the next morning. This extends gradually, being, however, confined to that side of the head, so as to reach its height at mid-day, and again departs towards evening. Whilst at rest the pain is bearable, but on movement it increases fearfully, as also under all circumstances exaggerating the pressure of the blood to the head, such as stooping, coughing, &c. It is synchronous with the pulse of the temporal artery, which, on the side affected, feels like a hard cord; the artery on the opposite having all the time a natural character. The countenance is wan and pale, and the *right* eye small and reddened; and at the height of the attack there is nausea, but never actual vomiting. When the attack is approaching its termination the *right* ear reddens and becomes much warmer, both subjectively and objectively. The seizure is curtailed by sleep. Slight gastric disturbance remains after the attack, and often one part of the hairy scalp is painful. The attacks are more frequent in the winter; they are entirely absent during travelling on foot; and latterly have been less frequent and regular than formerly, when the author used to exercise his mind more fully. The affection just detailed is evidently that ordinarily known under the term Migrän, which is universally considered to be a neuralgia, whose anatomical site is considered by Romberg to be placed in the brain; by Lebert in the first division of the fifth nerve, &c. In these attacks the observer, and at the same time the patient, considers that in his Migrän there is a tetanus of the muscular coats of the blood-vessels on the affected side of the head; in fact, a tetanus in the part dominated by the cervical portion of the sympathetic nerve on the right side, as is shown by the condition of the temporal artery, the pallor of the face, the sinking of the eyeball. A condition of the other branches of the carotid artery is naturally surmised, similar to that of the ophthalmic artery as well as of the vertebral artery. The tendency to vomit appears accounted for (just as Woollaston explained sea-sickness) by the alteration in the blood's pressure in the brain, as also the fluttering before the eyes often accompanying the Migrän and the use of digitalis. The redness and warmth of the parts about the ear observed on the departure of the headache arises from the weariness and relaxation of the unstriped muscles of the vessels following and consequent upon their over-contraction. The redness of the conjunctiva *during* the attacks is probably either owing to the muscular coats of its vessel being earlier relaxed, or to the fact of their having begun sooner to be contracted or tetanized than the coats of other vessels.

The above-described case of Migrän is, then, not to be looked upon as an affection of the brain or cerebral nerves, but of that portion of the cervical region of the spinal cord termed by Budge and Waller the "*regio-cilio-spinalis.*" This being so, it ought to be found that a tetanus of the structures controlled by the sympathetic from this part of the cord, would be attended by a dilated pupil; and it so happened that during the attacks it was found, both by the patient and by Dr. Schacht, that on the side affected the pupil of the eye was dilated, the other one remaining natural, and this was especially noticeable in proportion as the eye was shaded. Moreover, during as well as subsequent to the attack the spinous process of the lower part of the neck was painful on pressure. In his case Du Bois-Reymond thought that the pain of the head was the result of the pressure upon the sides of the blood-vessels by the contained blood permitted by the tetanus of their muscular fibre. Du Bois-Reymond does not, however, assert that Migrän is of necessity explained in this way. On the contrary, he thinks it to be almost always a neuralgia. He concludes by drawing a parallel between his explanation of his own case and that of many epileptic attacks offered by Kussmaul and Tenuer.

II. *A Case of Epilepsy, in which attacks of Unmeaning Laughter, Tetanoïd Spasm, and peculiar Rotatory Movements occurred.* By G. E. Paget M.D., Cambridge.

This highly interesting communication, which was originally read at Cambridge, accompanied by certain commentaries, is reprinted from the 'British Medical Journal.' The case was that of a labouring man, aged twenty, who had been under observation, more or less, since July, 1856, at which date he had for a few weeks only been the subject of ordinary but severe epileptic attacks, which, however, occasionally came on in 'groups' or 'paroxysms.' But prior to the invasion of the regular epileptic attacks he had been for eight or nine months subject to "frequent bursts of unmeaning laughter," which came on day after day, lasting about a minute. They were evidently quite involuntary, and took place without apparent cause, and came on also frequently during sleep. These attacks were not, like ordinary laughter, excited by something ludicrous, but it was quite clear that they were unconnected with any pleasing emotion or idea. They were, after a time, often accompanied by dancing movements, which, along with the laughter, would stop when his attention was arrested by calling. He had been wont to pass the urine during these laughing attacks. At one time, also, he suffered from frequent spasms, resembling those of tetanus, the back being bent as in opisthotonos, during which time he retained his consciousness, and between the spasms he was wont to be very excited, and obtained relief by walking about, drinking and washing with cold water. In August, 1857, he became subject to attacks of rolling or turning on his own axis, chiefly from right to left, during which he neither lost consciousness nor became giddy. These rotatory movements could be restrained by moderate force. They were generally followed by flatulent eructations. Subsequent to this his temper became altered, and he became very obstinate and self-willed.

In his remarks or commentaries the author shows, by reasons which space precludes our adducing, that the attacks of spasmodic laughter were essentially abortive epileptic fits. He quotes a case related by Billod (Annales Médico-Psychologiques, tom. ii. 1843)—the only one which he knows of—in which epileptic fits assumed the laughing form, and remarks truly that it is singular that such cases are so rare, considering the close relation between epilepsy and hysteria, the latter of which is so often attended by spasmodic laughter. Dr. Paget discriminates between the above class of cases and those in which laughter occurred in epilepsy, not in "spontaneous fits, but excited by very slight causes, the only deviation from the normal state consisting in a proclivity to laughter far greater than was natural."

The author quotes several cases in which unwonted laughter and peculiar movements like those of the cases adduced accompanied various diseases. He concludes by stating that for some months both the regular and the laughing attacks had been preceded invariably by an aura, which commenced below the navel and rose to the throat when the attacks came on.

Dr. Paget thinks that most good has been effected in the way of treatment by blisters to the region of the navel and the use of valerianate of zinc and herbane; and latterly by bismuth and magnesia.

III. *On Paralysis of the Spinal Muscles.* By Dr. ZURADELLI. (Archives Générales de Médecine, March, 1860, p. 362. Quoted from the 'Gazetta Medica Italiana Lombarda, 1859.)

Of this affection the following characteristics are summed up:

1. Dull pain in the lumbar region, increased on long standing and diminished by lying down; in some people neuralgia often affecting the intercostal nerves; occasional loss of sensibility of the skin down the spine.
2. In the early stages, slight incurvation of the spine; in the later ones this is so great that the head rests on the breast.
3. Excavation of the vertebral gutters, so that the vertebral extremities of the ribs may be more or less easily distinguished.
4. Some painful point exists on a level with a lumbar spinous apophysis, owing probably to stretching of the ligaments which have lost tonicity.
5. In advanced cases a peculiar elasticity at the level of the spinous and transverse apophyses of the vertebræ, due to tension of the aponeuroses.
6. Impossibility in maintaining the erect posture without support.
7. Incomplete power of extending the trunk without fixation of the upper limbs.
8. Alteration in the natural curvatures of the spine, the concavity of the cervical region being the first to disappear.
9. Right declination of the whole spinal column to the right side.
10. Incomplete straightening of the spinal column by Faradization of the muscles of the vertebral gutters.
11. Freedom of movement of the cervical muscles, excepting the above-mentioned incurvation.
12. Increased mobility of the vertebræ one upon the other, owing to relaxation of the muscles, attended by occasional noise like the crepitus of certain dislocations.

IV. *On Progressive Muscular Paralysis of the Tongue, Velum Palati, and Lips.* By Dr. DUCHENNE, of Boulogne. (Archives Générales de Médecine, September, 1860, p. 283; and October, p. 431.)

The author describes seven out of thirteen cases which, since the year 1852, have fallen under his notice, in which, without apparent cause, paralysis affected the above-mentioned parts successively, producing interference with articulation and swallowing, complicated at an advanced stage and with frequent attacks of suffocation, and leading to death by starvation or syncope. Unfortunately post-mortem examinations could not be obtained in any of the cases alluded to. Going into particulars, the author details the symptoms, course, duration, prognosis, diagnosis, causes, nature, and treatment of this affection.

As respects the symptoms, the paralysis of the tongue is described as constituting, in fact, the malady, as it is the one which threatens life, by hindering nutrition of the body. This symptom is generally the first to make its appearance. Owing to difficulty in moving the tongue, the articulation of certain consonants is rendered difficult, and finally impossible; the swallowing, at first a little interfered with, becomes difficult; the saliva, rendered viscous by retention in the mouth, being constantly ejected; finally, when the tongue is entirely deprived of motor power, deglutition is as impossible as when the mouth is kept perfectly open and the tongue depressed. Then, owing to the velum palati being affected, the articulation of the labials becomes affected, the 'p' and 'b' being sounded as 'me,' 'fe,' or 've,' owing to the posterior nasal orifice not being closed, and the column of air exhaled being subdivided into two columns, one of which finds its way by the wide posterior orifice, causing a peculiar nasal noise. Moreover, food, in swallowing, finds its way by the nasal fossæ. On inspecting the inside of the mouth, there is generally no irregularity to be perceived in the arches of the palate, as the paralysis is mostly equal on both sides; but we obtain immediate proof of it by the improvement in articulating the labials which takes place when the nose of the patient is pinched at the time of forcing the air through the mouth.

As regards the paralysis of the lips, the patient at first has a difficulty in distinctly pronouncing the vowels 'o' and 'u,' and soon a difficulty in whistling or kissing appears; then the labials are imperfectly pronounced, and sometimes, owing to paralysis of the quadratus and triangularis muscles of the lips, the vowels 'e' and 'i' cannot be sounded.

Duchenne had never noticed paralysis of the orbiculares muscles of the eyelids, or of the zygomatics, of the elevators of the upper lip or of the motors of the nose, nor yet of the buccinator. Owing to paralysis of the orbiculares of the lips, the muscles moving the commissures and acting upon the lids become predominant, and thus a doleful aspect is assumed by the face, owing to the transverse increase of the line separating the closed lips, and the deepening and enlargement of the naso-labial lines, by reason of the action of the elevators of the upper lip. The disturbances in breathing are often provoked by movement, as of walking, and often take place without any apparent cause; and suffocation may be so severe as to produce death by syncope.

Along with these local symptoms there is complete absence of febrile symptoms, and the digestive organs continue in good order. Gradually the patient gets weaker, which may be partly owing to want of food and partly to a feebleness, the result of the same central cause which produces the frequent attacks of giddiness to which they are liable. In one case only was there any apparent paralysis of limb.

As regards the progress of these cases, the paralysis is always chronic, lasting between one and three years, and always advancing, not in any instance remaining even stationary.

As respects the diagnosis of this form of paralysis, that appears to be in the early stages far from easy, partly because it begins by a simple muscular debility only, and partly because the palate and the lips are not simultaneously affected. It is otherwise in the second and third stages of the affection. The various affections with which it might be confounded are (1) Simple pharyngitis. (2) Simple paralysis of the velum palati. (3) Paralysis of the seventh pair of nerves, and (4) Atrophy of the tongue in the progressive fatty muscular atrophy. As regards pharyngitis, the absence of fever and of pain, as also of redness and swelling of the pharyngeal or buccal mucous membrane, is distinctive. Again, in pharyngitis we have not the disturbances of articulation, and of course in advanced cases the immobility of the lips and tongue is conclusive. In simple paralysis of the velum palati the dysphagia is never so entire as to hinder the swallowing of the saliva, and although the pronunciation of certain consonants is interfered with, yet the palatines are not sounded as 'ch,' as is the case in the instances under consideration.

Again, comparing these cases with those of paralysis of the seventh pair of nerves, confusion could only arise if this nerve was affected on both sides of the face; otherwise distortion of the features, not noticed in the cases of progressive paralysis, would exist: also integrity of the electrical contractility of the orbicular muscle of the lips distinguishes those cases from paralysis of the seventh pair, in which feebleness or loss of this property exists.

Finally, these cases might be looked upon as the commencing stages of the fatty muscular atrophy; but it is remarkable that out of 159 cases of the latter observed by Duchenne, never once was the tongue affected by atrophy at the commencement or in the first stage of the affection. But should it ever be that atrophy of the tongue and lips exists in the early stage of the progressive fatty atrophy, we should find that in the one case the paralysis exists without atrophy, and in the other we should have atrophy without paralysis. There are cases in which these two affections may be associated, and so give rise to a difficult diagnosis. Duchenne mentions such an one.

As regards the causation of these cases in question, Duchenne has been able to obtain little or no certain information. They seem to be peculiar to adult age.

In conclusion, as to treatment, the author looks upon Faradization of the affected muscles as most likely to bring about amelioration. In one of his cases related (No. 6), the patient improved greatly, both as to powers of articulating and of swallowing, and consequently as to general strength. This should be resorted to in addition to any such treatment as may appear necessary in the individual case.

V. *On the Pathology of the Pituitary Body.* By Dr. M. Michel, of Charleston, S. C. (From the Charleston Medical Journal and Review, March, 1860, and North-American Medico-Chirurgical Review, July, 1860. p. 750.)

The author, after detailing a case of cancerous tumour of this body, founds upon it a paper, which he concludes as follows:

1. The pituitary body, however largely developed in some animals, is not a primary division of the brain, or true encephalic ganglion, since its complete destruction is never accompanied by loss of intellect, motion, or sensation, beyond what may be accounted for by the necessary pressure upon more essential parts of the encephalon.

2. That we have proof of the identity of the nature of this hypophysis with certain so-called vascular glands, as the thyroïd, thymus, spleen, &c.

3. While the diagnosis of the morbid states is rendered obscure by absence of its ascertained function, this almost constant connexion with the simultaneous production of amaurosis in both eyes, and with absence of symptoms of cross paralysis, will indicate the seat of the disease when compared with morbid states of either hemisphere.

4. The long continuance of disease in this situation may propagate inflammatory action to neighbouring parts, followed by apathy, somnolency, syncope, cephosis, and other symptoms obscuring the disease.

VI. *Three Cases of Thrombosis of the Pulmonary Artery in Infants.* By C. Rauchfuss, of St. Petersburgh. (Virchow's Archiv, Band. xviii. Heft. 5 and 6, p. 537.)

The author observes that in the course of two years, out of 1800 post-mortem examinations, he had met with thrombosis of the pulmonary artery only seven times. In four of these cases the affection was distinctly embolic in origin. In two cases there was plugging up of the "ductus arteriosus;" in one case of the right ventricle; and in one case of the auricular opening of the inferior vena cava.

To proceed with the three cases which are quoted:

Case I. was that of an infant three weeks old, who died of choleraic diarrhœa. On *post-mortem* examination, the skull was found to be very hyperæmic. The sinuses of the brain contained much blood, but no coagulum; the heart was of natural dimensions, and dark viscid blood, with scanty fresh clot, but nothing more, existed in its cavities. The pulmonary artery contained a firm brownish-red plug, which narrowed its diameter about half, stretching from a little above the valve-flap to the right subdivision of the vessel. Into this it passed as far as to the third subdivisions. The wall of the vessel was healthy; the plug but slightly adherent, and its surface in contact with the blood's current was excavated; the termination of the plug towards the arterial valve was greatly diminished in size. The chief portion of the plug was firm and fibrinous, but towards the periphery the central parts were fresher, and contained more blood; the right lung was tolerably œdematous; the kidneys and liver were full of blood; the spleen normal; the venous system full of dark viscid blood, free from coagulum.

Case II. was that of a three weeks' old child, which also died of choleraic diarrhœa. *After death*, the skin was found very livid in places; the sinuses of the dura-mater and jugular veins were full of dark viscid blood with scanty coagulum; the veins of the pia-mater were very injected, and also those of the brain, which was natural. The right lung was much smaller, and at its edges were fibrinous deposits; the heart was of natural size; the right auricle contained dark viscid blood with but slight coagulum on its walls, but the walls of the right ventricles had much firm old coagulum adherent to them, partly broken down, and grumous and colourless, and containing, as seen by the microscope, fatty matter and broken-down blood-cells. The pulmonary artery was full of thick firm plug, unevenly excavated on its upper surface, which passed to a great extent up into the artery of the right lung, but only slightly into that of the left one. The coagulum in the lower subdivisions of the right pulmonary artery was very advanced in disintegration, containing much fatty, and granular, and pigmentary detritus. In the corresponding part of the lung, softening masses of fibrin and blood also were found, and the entire lung was œdematous. The ductus arteriosus and umbilical veins were somewhat involved in like manner; the liver, spleen, and kidneys were full of blood.

In the last case, the author thinks that the plugging up of the pulmonary artery was "embolic," and secondary to the disintegrated deposit in the right ventricle.

Case III. was that of a four weeks' old child, who also died of diarrhœa. *After death* the entire ascending aorta was found full of coagulum, and from the spot corresponding to the ductus arteriosus to that giving off of the sixth intercostal artery, a firm, greyish-red plug was found, in part occluding the vessel, and in one place only very adherent to its walls. At this spot the coagulum gradually diminished in size, and between the eighth and ninth intercostal artery a second thrombus existed, extending some distance. Firm adherent plugs were also found in the renal and some lumbar arteries. No softening existed in the plugs, and after being in spirit some time they were found to be distinctly laminated. In the right kidney hæmorrhage of some of the Malpighian bodies existed, and small masses of degenerating fibrine.

Case IV. was one of a child two months old, who died of diarrhœa. *After death* the cranial sinuses were found full of dark viscid blood, and the veins of the pia mater were full of blood. From the opening of the inferior vena cava a thick, firm plug of coagulum projected into the right auricle, otherwise the heart only contained viscid blood. Towards its division the pulmonary artery contained a thick fibrinous plug, which passed to a great extent into the branch of the left lung, in which was a quantity of corresponding hæmorrhagic infarction. The abdominal organs were very full of blood.

VII. *An instance of Stenosis of the Aorta at a point corresponding with the opening of the "Ductus Arteriosus" in a child three weeks old.* By the same Author as the above cases. No. 6 (Virchow's Archiv, Band xviii. Hefte 5 and 6, p. 544).

The child died after a two days' illness. Atelectasis of the lungs was found, and catarrh of the bronchi. The heart was as large as that of a child six or eight months old, and the walls of the left ventricle were very thickened. The pulmonary artery and left ventricle were much larger than they should be. The valves and membranous part of the septum of the ventricles were natural; the endocardium of the left ventricle somewhat thickened. The aorta was natural as to its walls; but beyond the giving off of the innominata it began to narrow considerably, and to such an extent that the tube of the arch of the aorta was continuous with that of the descending part only by an opening of the size of a pin's head; and this point of extreme contraction exactly corresponded with the "ductus arteriosus," the walls of which were thickened, their lining membrane being granular and knotted.

The author quotes an interesting case of Bochdalek's (Prag. Vierteljahrsschr. 1845, iv.), in which a plug filling up the ductus arteriosus and projecting into the aorta caused obliteration of that vessel.

VIII. *On Involuntary Seminal Emissions, and their Influence upon the Production of Insanity.* By Dr. Lisle, formerly Director of the Private Asylum at Gros-Caillou. (Archives Générales de Médecine, Sept. and Oct. 1860.)

The observations on this subject are founded on the well-known works of Lallemand and Esquirol, and are put forth in the form of comment upon seven cases, all instances of melancholy and hypochondriasm, and in one case of mania, which are given with great minuteness. In several of these cases a cure is said quickly to have followed the cauterization of the prostatic part of the urethra, which was resorted to for the cure of the local affection.

IX. *A Case of Polydipsia following Cerebral Disturbance.* (Archives Générales de Médecine, March, 1860. p. 359.)

This was communicated to the Hospital Medical Society by Dr. Moutard-Martin. It was that of a man aged twenty-seven, previously enjoying good health, and never having complained of excessive thirst or the necessity of passing much urine, who fell from a height upon his head and sustained a fracture of the skull. He remained insensible for eleven days, and remained forty days in hospital almost always confined to bed. He went out, but was unable to work, and again was admitted, suffering from headache in the neighbourhood of the injury, and constant lowness of spirits, general feebleness, giddiness, and some degree of paralysis of the muscles on the right side of the face. There was loss of vision of the right eye, and imperfect vision in the left one. Moreover, there was slight loss of power of the right arm, and the sensibility of the skin of the same limb was diminished. There was great disinclination for sleep. After a few days the patient observed that he had had very unusual thirst ever since the accident, and had passed an unusual amount of urine. The urine was pale, and gave no indications of containing sugar. In a single day he drank about six litres of water, and passed eight litres of urine. Under treatment all the symptoms diminished except those of the excessive thirst and passage of urine; but after a time the water drank and the urine passed became reduced to a normal standard.

X. *On Elephantiasis, or Cochin Leg.* By F. DAY, Esq., F.L.S., Civil Surgeon, Cochin. (The Madras Quarterly Journal of Medical Science, No. 1, p. 37.)

This disease, from which Europeans are nearly exempt, receives the name of Cochin leg because, although occurring along the seashore and banks of tidal rivers in India and Ceylon, &c., it is mostly prevalent at Cochin, where it is endemic. Mr. Day grounds his statements upon this disease on several cases personally observed, in many of whom he had the opportunity of noting the results after amputation of the affected limbs. In 93 per cent. the lower limb was diseased, and in all cases the patients suffered from what is called elephantoïd fever, the intensity of which was quite independent of any variety as to the site of the affected part. Elephantiasis is in fact defined by the author as consisting constitutionally of a peculiar fever returning at irregular intervals; and locally of hypertrophy of the skin and subjacent areolar tissue due to adventitious deposits. The type of the elephantoïd fever resembles a quotidian without premonitory symptoms, hot, cold, and sweating stages existing, and sometimes delirium lasting from one to four days. During the paroxysm an enlarged and painful state of some gland generally exists in the affected limb, between the site of the effusion and the trunk, subsiding without suppuration, but not entirely departing. During each attack of the fever the local swelling generally becomes more marked, and it may be hot and throbbing, or only itching. There is generally a hardened reddish or dusky line along the course of the absorbents, between the local effusion and the glands. The enlargement often fluctuates, at one time being greater, at another less. It is not necessarily confined to the limbs, and when it is they are still serviceable, as the joints are unaffected, until they become too bulky. The attacks of elephantoïd fever are not prevented by salivation, and may be induced by sudden checks to perspiration, by over-exertion, and by use of certain drinks. As a rule, but not an invariable one, the severity of the constitutional disturbance is an index of the rapidity of the effusion.

As regards the nature of elephantiasis, it is doubtful whether it is due to malaria, and if so, whether the various effusions should be looked upon in the same light as the enlargements of the spleen in ague. The only analogy to ague is to be found in the succession of febrile stages. These are endemic, unchecked by anti-periodics, and are free from corresponding constitutional injury. The localities where it abounds are not those where malaria is most intense.

As regards treatment, local means, in addition to amputation, consist locally, in acute stages, of rest and elevation, with leeching in the neighbourhood of painful lymphatics, cold Goulard or spirit lotion, or warm fomentation; and in chronic conditions, of bandaging, with weak iodine and biniodide of mercury applications. In some instances blisters are useful. As regards general treatment, tonics with bichloride of mercury in small doses, and opium, along with removal to an unaffected locality. Amputation must be resorted to when irritative fever is set up by extensive ulceration and when progression is sufficiently interfered with.

In this paper the author does not discuss elephantiasis of the scrotum, as full accounts had been published of this form in Bengal and Bombay.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *Successful Digital Compression in a Case of Wound of the Brachial Artery.* (Gazette des Hôpitaux, 1860, No. 112.)

M. Boinet brought this case under the notice of the Paris Surgical Society. A man, aged thirty-six, received a violent blow of the fore-arm, which gave rise to great tumefaction. Two incisions were made, and from the direction of these, and the amount of hæmorrhage which ensued, it was concluded that the brachial artery had been wounded at about the middle of its course. Compression having been tried in vain, the patient was brought to the hospital at Saumur four days after the injury. It was not thought advisable to resort to the ligature on account of the bruised and swollen state of the parts, which threatened to become gangrenous, and it was resolved to institute digital compression at the upper part of the brachial. This was accomplished by means of a pupil and three hospital sisters, who relieved each other at the end of every hour. After forty-eight hours all bleeding had ceased, but the compression, for security's sake, was kept up twelve hours longer. The accident occurred on the 17th June, and the patient was discharged quite cured on the 31st July. The conclusion which M. Boinet draws from the case is, that when in secondary hæmorrhage the inflamed or infiltrated state of the tissues renders the discovery of the ends of the divided vessel difficult or even impossible, digital compression should be preferred to the application of a ligature. He also is of opinion that even in primary arterial hæmorrhage a trial should be given to this means. Its chance of success is here far less, but as it is entirely exempt from any danger, the attempt should be made.

During the discussion which ensued, M. Vallemier expressed an opinion that in the absence of fuller details only one of the collateral branches, and not the trunk of the brachial, had been wounded. He also considers that there is now prevalent a disposition to exaggerate the advantages of digital compression as compared with ordinary compression methodically applied, and for his own part he has obtained excellent effects in hæmorrhage from the latter. M. Giraldès also regarded the fact of obtaining in so short a time the cicatrization of a wound of so large an artery as the brachial by means of mere compression so unlikely, that he doubted its accuracy. The bleeding from the lower end of the vessel must have been arrested, an effect not even produced by ligature of the trunk. He considered, also, one hour as too long a period for a person to maintain efficient compression with the fingers. M. Verneuil observed that whichever the artery concerned might be, a very large quantity of blood had been lost, and a successful result had been obtained. He had himself already detailed to the Society cases of the successful employment of compression in secondary hæmorrhage. M. Legouest regarded the case as one of great interest, it being the first instance of the cure of great traumatic hæmorrhage by means of digital compression; but he was unable to agree with M. Boinet in anticipating advantage from the same means in primary traumatic hæmorrhage of large vessels. M. Marjolin was of opinion that even in a recent wound, when the two ends of the divided vessel cannot be seen, digital compression should be resorted to. He has himself employed it in three cases. The first was a wound of the palmar arch; the second, a wound of the radial artery in a child; and the third, obstinate hæmorrhage after lithotomy, also in a child. This was arrested by carrying the finger into the rectum, and maintaining compression during three-quarters of an hour. During the discussion, M. Velpeau related an interesting case which occurred in his practice. A workman wounded his wrist, opening the radial artery. Compression was made over the wound during three days, and continued in a gentler form for other three days. The patient was supposed to be cured, but on the removal of the compresses, an aneurism the size of a walnut was found to have formed. Digital compression was applied to the brachial during sixty hours, the only effect being the production of painful swelling of the hand and fore-arm. Eight drops of the perchloride of iron were now introduced within the sac, and the pulsations immediately and permanently ceased. Ten days afterwards, when the wound had nearly cicatrized, and the tumour had notably diminished, suppurative inflammation of the sac was set up, and a radical cure was thus accomplished.

II. *On the Arrest of Venous Hæmorrhage.* By Professor Langenbeck. (Archiv für Klinische Chirurgie, Band i. p. 543.)

In the course of an interesting paper by Professor Langenbeck on the 'Surgical Pathology of Veins,' illustrated by numerous cases, he observes that styptics are not suitable for the arrest of venous hæmorrhage. The best of these, the liquor ferri sesquichlorati, is dangerous, owing to the extensive thrombus formations and subsequent irritating effects it gives rise to. In all cases, when obstinate venous bleeding proceeds from several small veins, he gives decided preference to the actual cautery, as most certainly guarding against the breaking up of thrombi and pyæmia. When the bleeding proceeds from a large vessel, compression, ligature of the vein, or ligature of the corresponding artery, should be resorted to. In wounds of the large veins of the extremities, *compression* of the peripheric end by means of the finger will usually suffice; and in wounds of the jugulars, we should at once apply the finger to the central end to prevent the entrance of air, and then to the peripheric end to arrest the bleeding. In the case of a large wound of the jugular, the finger can only act provisionally, and the best means of proceeding consists in closing the lips of the outward wound by strips of plaister (which must not extend to the uninjured side of the neck, where they would compress the opposite jugular), so applied as to exert the most equable compression around the wound without impeding the circulation. In the case of the veins of the extremities, bandages may also be exactly applied, commencing at the toes or fingers. When the injured vein is at the bottom of a wound, the author places some cerated linen in contact with it, fills the wound with charpie, and then brings its edges together with plaister.

Ligature of the Vein.—In general, tying the peripheric end of a wounded vein of the extremities suffices; but a ligature both above and below the wound may be required when a considerable branch enters just above the central end. To avoid the loss of blood during the removal of large tumours, the provisional ligature of several large subcutaneous veins, which sometimes acquire the size of the finger, may be requisite; and in such cases the author always applies a double ligature, and divides the vein between, removing the ligatures after the completion of the operation. This practice is the more to be recommended from our ignorance of the conditions under which air gains entrance by dilated subcutaneous veins. In wounds of the external jugular, the central end should always be tied, as it should be prior to operations likely to lead to its being opened. Under other circumstances, the ligature of large veins should be avoided as much as possible, especially in hospitals, where the danger from thrombosis and pyæmia is increased. The internal coat of a large vein is not divided by the ligature, as in the case of an artery; and the inner walls approximated by the ligature may unite before the separation of the latter—the vessel remaining pervious, though somewhat narrower, up to the very seat of the ligature. When inflammation follows a wound or a ligature, more or less extensive thrombosis may succeed, which may lead to detachment of coagula or pyæmia. When this is not the case, the vein gradually becomes pervious again; and so great is the regenerative capacity of veins, that, even when large portions have been removed, these may be reproduced, effecting a junction between the separated ends of the still pervious vessel.

Ligature of the Artery.—It is obvious, *à priori*, that compression or ligature of the corresponding arterial trunk, by preventing the access of blood, must arrest hæmorrhage from a large vein; but it does not appear that any one has yet practised the ligature for such a purpose. One reason of this seems to be derived from the fact that when compression of an arterial trunk, as the carotid, subclavian, or femoral, has been made, through the soft parts, at some point between the heart and the bleeding wound, the bleeding from the jugular, axillary, or femoral vein has not immediately ceased. This is partly because the peripheric veins still continue to pour their blood into the injured vein, and partly because an effectual compression of the carotid or femoral artery is not possible without accompanying compression of the jugular or femoral vein. Compression of the arterial trunk, therefore, not having been attended with the same immediate effect as in arterial hæmorrhage, it has been believed that no good result was to be expected from the ligature. Again, it has continued, until the most recent times, a cherished opinion among most surgeons, that the simultaneous tying of a large venous and arterial trunk must give rise to gangrene—a fear which experience has shown to be unjustifiable. In fact, when both artery and vein are tied, not only does no gangrene follow, but there is less disturbance of the capillary circulation than when only one of these vessels is submitted to the ligature. In two cases related by the author in which the carotid and common jugular were both tied, no disturbance whatever of the cerebral circulation took place, and neither patient exhibited any of the symptoms which have been met with when ligature of the carotid alone has been practised. During the establishment of the collateral circulation an equilibrium between the arteries and veins has been maintained. In this ligature of the artery, then, we have a safe means of treating venous hæmorrhages which may otherwise prove fatal, and the author relates a case in which he made application of it. During the removal of a large sarcomatous tumour from the thigh, the much-enlarged and brittle femoral vein was opened. The hæmorrhage was excessive, repeatedly resisting every attempt to arrest it, and the patient was well-nigh lost. The femoral artery, already exposed during the operation, was tied, and the bleeding at once ceased. The ligatures which had been passed around the vein were removed, and the wound dressed. The patient did well. This, as far as the author is aware, is the only case in which an arterial trunk has been intentionally tied for the arrest of a dangerous venous hæmorrhage. He refers, however, to cases quoted by Dr. Crisp, in which bleeding from wounds of the vein made during the operation for popliteal aneurism ceased after the application of the ligature to the artery. The author recommends that as soon as compression proves without avail in hæmorrhage from large venous trunks that the artery should be at once tied—the simultaneous tying of the injured vein being unnecessary and unadvisable from the danger of thrombosis it gives rise to.

III. *Case of Hæmatocele communicating with the Cavity of the Abdomen.* By M. Rochard. (Union Médicale, 1860, No. 101.)

This man, aged fifty-one, a labourer in the port of Brest, had already been a patient in the Marine Hospital some years since with epididymitis and an irreducible hernia, both on the left side. According to his imperfect account, the tumour of the testis had been twice punctured since he left the hospital. On this second admission the scrotal tumour descended as far as the lower third of the thigh, while it was continuous, by means of a cylindrical prolongation, in the direction of the inguinal canal, with another tumour within the abdomen, the anterior wall of which it thrust forwards. The latter mounted as high as the umbilicus, and receded beyond reach into the iliac fossa. The abdominal tumour measured seventeen centimetres in the vertical direction, and the scrotal twenty-three centimetres, the circumference of this last being forty-five centimetres. Both tumours were alike hard, and the firmest pressure made no difference in their reciprocal dimensions, although the vibrations resulting from striking any part of the one were propagated to the other. Within the substance of the tunica vaginalis were felt extensive plates presenting all the resistance of osseous tissue. Notwithstanding that there was no transparency present, and the known prior existence of a hernia, the nature of the case was plain, and it was resolved to treat it by means of iodine injections. After the envelopes of the scrotal tumour had been incised layer by layer, a small jet of fluid was observed to issue, and the trocar was passed in. As much as 2910 grammes of a brownish-green, inodorous fluid flowed out, which on cooling deposited fibrous masses and shreds of false membranes. Its composition showed it to consist of blood, probably mingled with the serosity of the hydrocele of six years' standing, and constituting what M. Gosselin terms a consecutive hæmatocele. After the puncture, the tumour of the abdomen, as well as that of the scrotum, subsided, and the two lateral halves of the tunica vaginalis could be applied against each other like the two valves of a shell, which in hardness they resembled. Not a vestige of the hernia could be discovered. Next day the tumour of the abdomen did not exceed an egg in size, having emptied itself into the scrotal portion, which had resumed almost its former size. By means of a gum elastic bourgie three hundred grammes of fluid were discharged from the same aperture the next day, and on the third day two hundred grammes were so obtained. As the fluid now began to manifest fœtidity, some injections of a solution of chloride of soda were thrown in, and on the fourth day a new puncture was made and iodine injections were commenced. These were repeated every day, the proportion of tincture of iodine being gradually increased from a sixth to the half, and the discharge, putting on a purulent character, diminished in quantity. After the fortieth day the injections were only continued every other day, and were increased in strength. By the sixty-fourth day only a few drops of pus issued from the aperture, and the patient was discharged as cured. A small, hard, conical prolongation was all that remained of the abdominal tumour, the inguinal canal still continuing enlarged. The scrotal tumour had contracted upon itself.

IV. *On Lithotomy and Lithotrity in Children.* By M. Dolbeau. (Moniteur des Hôpitaux, No. 108.)

M. Dolbeau, in temporary charge of M. Guersaut's wards at the Children's Hospital, has taken the occasion of the presence of two cases to make some interesting observations upon the operations best suited for the relief of stone in the child. One of these patients, four years of age, had been submitted to bilateral lithotomy by M. Guersaut, and did very well, except that a month after the operation a fistula remained, which will require surgical interference. The other child, seven years old, had a very hard calculus, measuring three centimetres in one direction by two and a half in another. M. Dolbeau liberated him of this by means of lithotrity, eleven *séances*, each of about a quarter of an hour's duration, being required between the 11th July and the 20th of

August. Chloroform was employed, and but very little local irritation was produced. The bladder, possessing but little power, discharged the fragments very imperfectly, and considerable difficulty was often experienced in their removal.

In estimating the comparative value of the two operations for children, in 1849 M. Guersaut gave the decided preference to lithotomy, except in those cases in which the stone admitted of being crushed in a single *séance;* but, although this surgeon has published no statement since that time, M. Dolbeau states that he now performs lithotrity much more frequently than he did, by no means limiting its application to cases which can be disposed of in a single *séance*, and that his success is far more considerable than heretofore. Several of the circumstances which have been objected to lithotrity are really due to the want of skill on the part of the surgeon. A serious inconvenience, however, is the engagement of the fragments in the urethra—a complication far more common in children than in the adult. In place of moderating the efforts at micturition, performing them in the horizontal posture, and using various other precautions, they expel all the fragments through the dilatable neck of the bladder into the urethra. Other inconveniences, as incontinence of urine, and sometimes a very notable diminution in the contractile power of the bladder, are in general of no long duration.

In M. Dolbeau's opinion, the two operations may be thus compared:—Lithotomy is applicable to all cases. It very frequently succeeds, but it exposes to accidents, such as inflammation and the establishment of fistula, which are difficult of cure. As a general rule, the operation is simple and easy of execution. Lithotrity is not applicable to all patients, but in determinate cases its results are excellent. Its execution necessitates a special dexterity, as it presents greater difficulties than does lithotomy. It may also give rise to accidents, as urethral fever, which may carry the patient off, the engagement of fragments in the urethra (a very serious complication), and incontinence or retention of urine. As the operation of lithotrity may have to be extended over a long period (a child nine years of age, a patient of M. Civiale, required seventy *séances*), the health should be good, and the bladder healthy and of good capacity. Nor must the calculus be too large or too hard; its volume may be considerable if it is also friable, but in the case of a hard stone, three centimetres is an extreme size. In spite of the success of lithotomy, it is an operation that should not be performed except when lithotrity is inapplicable. A *séance* of lithotrity may always be first tried in doubtful cases, in order to judge whether this can be borne, and that without any prejudice to the ultimate success of lithotomy. Chloroform should always be had recourse to, its advantages far outbalancing any inconvenience which may result from its use. As none but small instruments will enter an urethra of the amount of development in a child, the habit of manipulating with such must be acquired. The seizure of the stone is a matter of difficulty, and that not so much from the want of capacity in the bladder as from its form and situation. In place of being comprised within the pelvis, it occupies a part of the abdomen, while the absence of the prostate prevents the formation of the depressed part termed the *bas-fond*, where in the adult the stone is so frequently found. The bladder is large, since it mounts up into the abdomen, and the very moveable stone has no fixed situation. In the author's opinion, the manipulation is facilitated by leaving only a little fluid in the bladder, and by raising the buttocks so as to keep the stone in the most dependent part of the bladder. When the stone is hard and large, owing to the smallness in size of the instrument, it is held and broken up with difficulty. When the stone has been broken up, it is the engagement of the fragments in the deep portion of the urethra that is to be most feared. We must especially endeavour to reduce the smaller fragments to powder, or they may sometimes be gently removed in the grasp of the instrument. Fever seldom follows either catheterism or lithotrity operations in the young, and in consequence of the little reaction which takes place, the *séances* may be longer and sooner repeated (every two or three days at most) than in the adult.

V. *A Case in which the Lateral Operation and the High Operation for Stone were performed on the same individual.* By Dr. Güntner. (Wien Medicinische Wochenschrift, Nos. 14, 15.)

The patient, a copper-miner, forty-one years of age, was admitted to the Salzburg Surgical Clinic complaining of symptoms of stone which had prevailed for about five years. On sounding him, a large hard stone was detected, which seemed to be confined to the left side of the bladder. The lateral operation was resorted to 15th December, but the operator found to his surprise that the bladder was placed so high, that the end of the forefinger could only with difficulty reach the end of the wound in the prostate. Moreover, when the forceps were passed in and moved on every side, no stone could be felt until a more curved pair had been introduced and directed to the left side. It was found even then impossible to move the stone, and the conclusion arrived at was, that it was fixed in a diverticulum. After innumerable and varied tractions, the stone was broken on employing great force and bending a lithotrity instrument, and six fragments of various sizes were extracted. The cutting of the stone out of its diverticulum was, however, out of the question, seeing that the finger could not be made to reach it. The patient had suffered much in spite of the use of chloroform, and this, together with the violence which had been done to the bladder, and the fatigue of the operator, compelled the postponement of further proceedings. Not a single bad consequence ensued, the wound having completely healed by the fifteenth day; but the enuresis afterwards becoming still more distressing than it had been prior to the operation, it was resolved to endeavour to remove the stone by the high operation.

This was resorted to on the 23rd of January of the past year, and the stone was found to be, as expected, fixed in a diverticulum on the left side of the posterior wall of the bladder. The extraction even now was attended with some difficulty, but was at last accomplished after ten minutes' traction. The bleeding, whether from the abdominal parieties or the bladder, was surprisingly slight. The stone, when the removed fragments had been replaced, measured two inches nine lines in its long, and near two inches in its transverse diameter, and it weighed twenty-four and a half drachms. It almost entirely consisted of uric acid. By the eighth day after the operation the febrile symptoms, which at first had been somewhat alarming, had entirely subsided, and by the twelfth day the urine was discharged at regular intervals by the urethra, none passing by the wound after the fifteenth day. The external wound was, however, very tedious in healing, this not having been completely accomplished until the sixty-fourth day. The enuresis had completely disappeared. Dr. Güntner states, that after the high operation he much prefers leaving strips of cerated linen hanging from the angles of the wound to closing this by sutures. They serve as conductors of the urine, and prevent infiltration, while they induce far less irritation than do the sutures. The outer wound only should be brought together by some points of suture.

VI. *Cases of Fracture of the Sacrum.* By MM. Hamon and Mercier. (L'Union Médicale, 1860, No. 115.)

As this accident is of such rare occurrence, and is so concisely treated of by writers on surgery, M. Hamon supplies the details of a case which occurred in his practice.

A woman, aged twenty-eight, July, 1856, fell upon her seat from a height of about ten feet. On examination, the lower portion of the sacrum, at nine centimetres above the point of the coccyx, was found flattened and carried forwards, free movement being imparted to the fractured portion, without any pain being induced when the finger was passed into the rectum. The bladder and rectum were paralysed, both requiring aid for the removal of their contents. While nothing remarkable was observed with respect to the thighs, the legs were paralysed. Seen two years afterwards; the rectum had recovered its power, but the catheter still had frequently to be used, while the paralysis was so far amended as to admit of the patient walking, although with difficulty and with the aid of crutches. M. Hamon saw the patient last early in the past year, and nearly four years after the accident. He found that menstruation, defæcation, and the discharge of urine all took place in a normal manner. A very projecting angle could be felt at the seat of fracture. While the thighs were of their normal size, the muscles of the legs had undergone considerable atrophy. The motors of the legs were only enfeebled; and although all movements of the foot were found to be impossible when it was held free in space, these became executable as soon as the heel rested on the ground. All the motors of the toes were completely paralysed. Imperfect perambulation by means of a crutch and stick was alone possible.

In reporting upon this case, which was read at the Paris Medico-Practical Society, M. Mercier relates another example of the accident, which occurred during his *internat* at the Hôtel Dieu:—A young mason fell from a height on to a stone in the sitting posture, and a compound fracture of the sacrum was the result; the fracture extending obliquely from one side to the other, at three inches above the point of the coccyx, and the detached portion being carried to the right. The rectum seen at the bottom of the wound remained intact. The skin covering the middle portion of the posterior surface of the pelvis had lost its sensibility to the extent of three or four inches transversely. Beyond this the sensibility was preserved, and the lower limbs were not paralysed. The expulsion of neither urine or fæces took place without aid, but after some days loose stools were discharged involuntarily. The patient sank exhausted twelve days after the accident. Large fragments of the sacrum were found separable and bathed in pus. The cauda equina was destroyed, the inflammation and suppuration invading the portion occupying the lumbar spine as high as the third vertebræ. All the roots of the sacral plexus, excepting the last lumbar and first sacral pair, were destroyed.

M. Mercier concludes, from these two cases, that the paraplegia is much less the effect of the rupture of the nervous cords compromised by the fracture, than of the consecutive compression and inflammation of the nervous plexus of the pelvis; and he suspects that the prevalence of the paraplegia in the slighter case, and its absence in the severer one (although in this last any difference which might prevail between the condition of the thighs and the legs was not sought for), may depend upon the effused blood not obtaining the outward issue in the one case which it did in the other.

M. Mercier further draws attention to this question. Why, in any case of paraplegia coming on rapidly, does the paralysis of the rectum, and especially that of the bladder, habitually commence with the retention of the contents, incontinence manifesting itself only at a more or less distant period? So complete is this retention in some cases of lesion of the spinal marrow, that the bladder would burst rather than allow a few drops of urine to escape. Supposing there to exist an equal inertia of the body and the sphincter of the organ, the slightest repletion ought to lead to a continuous discharge; and admitting, what seems to be the fact, that the sphincter is more under the influence of the spinal cord than the body of the reservoir, it ought to be the first affected by such lesions, and incontinence should at once appear; while it is just the contrary which is observed to take place. The neck of the bladder, in fact, is not closed by a simple wrinkling, as is generally supposed. The muscle, acting as the occluding agent, gives rise to a true valve, which the tonicity of this muscle is enabled to keep closed until the distension of the organ and the passage of instruments at last have destroyed the last remains of the contractility of tissue. Then, not only is there regorgement of urine, but a true incontinence. M. Mercier has expressed his views upon this subject at length in the 'Gazette Médicale' for 1854. The remarks made on the tonicity of the sphincter of the bladder may be applied to the anal sphincter. And although the functional mechanism of the latter is not so favourably disposed, this is the less necessary, in consequence of its strength, and the compact nature of the matters it has to retain.

VII. *On the Diagnosis of Dislocations of the Shoulder.* By M. MAISONNEUVE. (Moniteur des Sciences Médicales, No. 122.)

It very often happens, M. Maisonneuve observed in a recent clinical lecture, that even experienced surgeons may hesitate respecting the existence of a luxation of the shoulder; and you are aware of the learned dissertations to which the differential diagnosis of these luxations, fractures of the neck of the humerus, and even simple contusion, has given rise to. Numerous are the pages, even in the most recent works, devoted to this important discussion; and certainly, after reading and meditating upon them, one can but be persuaded that this diagnosis is one of the most delicate and difficult in surgery. This does not arise from the enumeration of the characteristic symptoms of each lesion being incomplete. Far from it; for real and doubtful symptoms, vague and precise symptoms, are so accumulated that even the most skilful can scarcely make them out, while the simple practitioner is utterly at a loss. Still there is a simple and easily-discovered symptom which will always enable you to recognise with certainty not only this but any other dislocation, whatever swelling of the surrounding soft parts may exist. This symptom is based upon the fact that *in all dislocations the normal movements are impeded or abolished*, while in simple contusion these movements persist, and in fractures others of an unusual nature are added to them. Take hold of the arm, and endeavour to make it execute the movements proper to the articulation. If these are found to be impossible, or very limited, there is without doubt dislocation; while if these remain intact, no luxation exists, and the presence or absence of shortening and crepitation will determine whether the accident is a fracture or a mere contusion. This sign alone will enable the diagnosis of the dislocation to be made. The study of the symptoms may be carried farther, and analysed in detail. We may verify the flattening and the depressibility of the shoulder, the projection of the head of the humerus, the elongation of the limb, and the various circumstances which determine the variety of the dislocation. But the mere fact of the abolition of the movements of the joint had already placed the fact of the dislocation beyond all doubt.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D., F.R.C.P.
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I. The Unimpregnated State.

1. *A Rare Termination of an Uterine Fibroid.* By Dr. E. Lumpe. (Ztschr. d. Ges. d. A. zu Wien, No. 29, 1860.)
2. *A Second Case of Atresia of the Left Half of a Two-horned Uterus.* By Rokitansky. (Op. cit. No. 31, 1860.)
3. *Death after Treatment of an Ovarian Cyst by Iodine Injection.* By Dr. R. Löwenhardt. (Monatsschr. f. Geburtsk, October, 1860.)
4. *On Tearing Asunder of the Fallopian Tubes and Ovaries, and on Strangulation of the Ovaries through Axial Twisting.* By Rokitansky. (Allg. Wien. Med. Wochenschr. 1860; and Monatsschr. f. Geburtsk, October, 1860.)

1. Dr. Lumpe records an interesting example of the spontaneous cure of an uterine-fibroid by calcareous degeneration, disintegration, and discharge. A woman who married in her thirty-first year, had two abortions at an early period of pregnancy, and then menorrhagia. Soon after the first abortion she experienced pains in the pelvis, and perceived a tumour the size of an egg. This enlarged very slowly but gradually. In 1856 the repeated uterine hæmorrhages had produced an alarming degree of anæmia. She was at this step seen by Dr. Lumpe, who ordered quinine and iron. The metrorrhagia was subdued for a time; but returned with profusion in the summer of 1858. In June, 1859, a frightful hæmorrhage occurred. Dr. L. then found a fibroid tumour growing in the fundus uteri of the size of a man's head, moveable, but little sensitive, of firm consistence, even-shaped, and without fluctuation. The hæmorrhages now began to subside; and the left foot began to swell, the urinary secretion to diminish, and after a pause of five weeks there was discharged several times daily a sero-purulent, offensive, organic detritus. This continued almost without intermission for four months.

The œdema of the left leg had nearly quite disappeared concurrently with a copious secretion of urine and spontaneous diarrhœa. But a few weeks later the œdema reappeared, not alone in both feet, but also in the face, back, and hands, and lasted till October. Then extreme anæmia, with irritative fever and progressive emaciation, followed, so that life seemed in imminent danger. Suddenly a complete revolution took place; appetite, sleep, returned; the foul discharge diminished, and a normal menstruation appeared in December, 1859. In the meantime the fibroid tumour had shrunk gradually, so that the segment projecting in the abdomen hardly reached two fingers' breadth above the symphysis. Before this period there had set in a copious discharge of calcareous concretions of various sizes. These the patient had washed, and formed a considerable collection. This continued over the successive menstrual periods, the diminution of the tumour proceeding. On the 21st June, 1860, when the last examination was made, the remains of the tumour sat like a cap on the fundus of the uterus. Dr. Lumpe makes several speculations as to the nature of the changes which attended the involution of the tumour.

2. Rokitansky refers to a first case of atresia of one side of a two-horned uterus described by him in the cited journal in 1859, and now describes another instance. On the 11th July, 1860, he opened the body of a woman aged twenty-four, who had died of the symptoms of pelvic inflammation following upon a concussion. In the abdomen was found five or six pounds of turbid purulent fluid. There had been an abscess in the hypogastrium, and adhesions connecting the uterus, sigmoid flexure, and intestines.

The uterus was provided with a complete septum. The right half of the uterus measured from the summit to the os externum uteri, 3″. It opened with a normal virgin orifice in a simple discoloured vagina. The left half was larger, near the cervix expanded into a capsule, containing a dirty yellowish thick ichor; its surface was uneven from numerous cicatrised erosions. This capsule only formed a projecting fluctuating protuberance into the left roof of the simple vagina. The septum between the two uteri was perforated by a small hole.

Rokitansky remarks that in this case, with atresia of the left uterine cavity, there had been an imprisonment of menstrual fluid, causing first distension of the cervix, then inflammation and perforation of the septum, with inflammation of the collateral (left) ovary, leading to abscess. With regard to the diagnosis of similar cases, this, he says, consists in the flattening and then projecting fluctuating leathery tumour at the roof of the vagina, which consists of the dilated cervix of the blind half of the uterus.

3. Dr. Lowenhardt relates a case of death following the injection of iodine into an ovarian cyst. A multipara, aged forty-one, still regularly menstruating, had suffered from ovarian dropsy for two years. She had been tapped several times, the fluid was highly albuminous but clear; the sac was considered to be unilocular. On the 6th November a puncture was made with a straight trocar in the linea alba midway between the navel and symphysis; twelve quarts of fluid were evacuated, being all that could be drawn; an injection, consisting of tincture of iodine and distilled water, of each two ounces, and ten grains of iodide of potassium, was slowly thrown in, and allowed to remain only four or five minutes; as much as possible was withdrawn. Collapse set in immediately, from which stimulants failed to rouse the patient; she complained of pain in the sacrum and abdomen, and irritation of the bladder; in full consciousness she died fourteen hours after the operation. On dissection was found, in the left side of the abdomen, a completely collapsed cyst, not very thick-walled, nor adhering, and having a pedicle the thickness of the finger; there was no trace of inflammation; the cyst contained about a pint of iodine-coloured fluid. The right ovary contained three small cysts; the mucous membrane of the uterus was hyperæmic and covered with sanguineous mucus. In the peritoneal sac was a small quantity of clear brown weakly-iodized fluid; no sign of inflammation. The author attributes the death to the impression made upon the nervous system by the injection.

4. Rokitansky describes some hitherto unnoticed conditions in the pathology of the Fallopian tubes and ovaries. He relates many cases, which may be arranged under the following heads:

(1.) Atrophy and tearing asunder of a tube through the dragging of its ovary.

(2.) Atrophy and tearing asunder of a tube through pseudo-membranous dragging.

(3.) Tearing asunder of a tube and of its corresponding ovary through pseudo-membranous dragging.

(4.) Strangulation of the ovary and tubes through axial twisting.

He relates three cases of the first description. In one there was a pulling asunder of the left tube through dragging by an ovarian fatty cyst. The tube sat in the form of a blind stump, 5‴ long, upon the uterus. To this was attached a long fibrous cord, on the end of which the remains of the Fallopian fringes were seen, the greater part of which adhered to an ovary degenerated into a fatty cyst about the size of a child's head. The second case showed atrophy and separation of the right tube through a serous ovarian cyst. In the third case the right tube was obliterated to the thickness of a thread, and was attached to a fatty cyst of the right ovary the size of a fist.

Under the second head two cases are recorded. In one the tearing was produced through adhesion of the right tube to a portion of the small intestine, the wall of which was drawn out like a funnel. The tube was drawn out and atrophied to a thin string.

Under the third head the author describes seven cases. The adhesion of the affected tube and ovary occurred in two instances in the recto-vaginal space; the tearing was caused in one through the uterus during repeated pregnancies; in the other, where the adhesion had no doubt been acquired in infancy, the cause was the growing uterus.

The author relates six cases of tearing asunder of the tubes and ovaries, and strangulation of the ovary through axial twisting. The direction in which the twisting took place was various. Inordinately enlarged ovaries may become twisted by a rolling over of the tumour backwards or forwards. The adhesions which the ovary commonly exhibits are acquired either before or after the twisting. In one case of a free normal ovary the twisting was probably occasioned by the habitual position of the ovary and tube in an inguinal hernia. In five cases the ovary was diseased and enlarged from cystic formations. In three of these the twisting was caused by the attempts of the much enlarged ovary to adapt itself to the surrounding organs, and especially through the pressure of the intestines.

The anomaly is important, since it may give rise to fatal accidents.

II. Gestation.

1. *Twin-pregnancy outside the Uterus.* By Dr. Rupin. (Gaz. d. Hôp., No. 13, 1860; and Monatsschr. f. Geburtsk., Oct. 1860.)
2. *On Hypertrophy of the Thyroid Gland in Pregnant Women.* By M. Natalis Guillot. (L'Union Méd., Sept., 1860.)

1. Dr. Rupin relates a case of extra-uterine twin-pregnancy. A woman, aged thirty, had previously borne two children normally. Her present gestation had reached mid-term when she was repeatedly seized with violent symptoms. Extra-uterine fœtation being diagnosed, and the excessive pain, with disturbance of the functions of the bladder and rectum, led the author to operate. An incision was made at a point of the posterior wall of the vagina which was strongly depressed by the tumour; after the escape of liquor amnii a fœtus was easily extracted, upon which was observed a strong contraction of the cyst. On account of violent bleeding the placenta was not removed. The patient died three days after the operation from continued hæmorrhages. Dissection showed: a tumour lying deep in the small pelvis; behind this, and a little to the right, the uterus enlarged; the placenta adherent to the fundus of the cyst, bigger, but thinner than usual, and enclosing a fœtus of about four months. The fundus of the cyst was thick, vascular, and provided with numerous venous sinuses; and there were observed distinct, although scanty, muscular fibres. The mucous membrane of the uterus was villous. The right ovary, of normal size, contained a corpus luteum, whilst the left appeared atrophied. Nothing unusual seen in Fallopian tubes or broad ligaments.

2. M. Guillot has communicated to the Medical Society of the Hospitals of Paris two cases of hypertrophy of the thyroid gland in pregnant women, in which he believed that all other causes than gestation were excluded. The following was the course of events in the two cases: progressive development of the thyroid gland; increasing difficulty in the movements of the neck and in respiration; pains radiating from the neck to the precordial region, and accompanying facial neuralgias, palpitations, vomiting, syncope preceded by vertigo,

and followed by intermittent asthma; alteration in the tone of the voice; exacerbation of all these symptoms, and death by suffocation. An examination, post-mortem, was made in one case. The thyroid gland had nearly reached the size of the human brain; it included the two pneumogastric nerves, carotids, and the trachea. The trachea was flattened in the antero-posterior diameter. There was evident compression of the carotids and pneumogastric nerves against the transverse processes of the cervical vertebræ. The lungs were congested. The tumour was firmer than the normal gland. Extreme abundance of fibrous tissue, forming throughout numerous thick and large cavities. The larger cavities contained no trace of epithelial cells, nothing but transparent granulations, spheroidal, some nucleated, some not. M. Guillot therefore regards the lesion as an hypertrophy of the fibrous and granular elements which constitute the organ. He associates the affection with the activity of the production of fibrine occurring during pregnancy. Some corroborative observations and cases were brought forward by members of the society before whom the memoir was read.

III. Labour.

1. *A Contribution to the Knowledge of Abortion and Fibrinous Polypus.* By Rokitansky. (Zeits. d. G. d. A. zu Wien, No. 33, 1860.)
2. *On Decapitation and Decapitating Instruments.* By F. W. Scanzoni. (Würzb. Med. Ztsch., 1860, and Schmidt's Jahrb., No. 10, 1860.)
3. *A Statistical Inquiry into the Causes, Symptoms, Pathology, and Treatment of Inversion of the Womb.* By Charles A. Lee, M.D. (Amer. Journ. of Med. Science, October, 1860.)
4. *Complete Inversion of the Uterus at Four Months of Utero-Gestation.* By E. W. Woodson, M.D. (Amer. Journ. of Med. Science, October, 1860.)

1. Rokitansky describes two cases which illustrate the pathology of abortion and fibrinous polypi. He cites the opinion of Kiwisch, that fibrinous polypi arise in peculiar unknown conditions from long-persistent hæmorrhages; that with dilatation and softening of all the arteries, especially of the yielding cervix, the coagulum attains, like the containing cavity, a considerable circumference; that the coagulum acquires the polypus-form, is occasioned by the yielding nature of the cervix, which expands in the shape of a ball whilst the thicker uterine wall resists the expanding force; that hence the lower part of the coagulum is larger than the upper, which mostly consists of fibrin adhering by a stalk to the uterine wall, whilst the lower consists of red, soft coagulum, which only on its surface is covered with a layer of fibrin. These polypi always occasion a profuse metrorrhagia with intense pains. The whole uterus during the menstrual period expanded, but chiefly the cervical canal, and the os uteri is more or less opened.

This doctrine is opposed by Scanzoni, who contends that these are cases of abortion.

Rokitansky's cases support the latter view. We give a summary of his second case. In June, 1860, Rokitansky opened the body of a girl aged seventeen, who had died of typhus and pneumonia. The expanded roof of the vagina presented a rounded fluctuating swelling, in the centre of which the os uteri was felt, with sharp edges and a round opening admitting the tip of the finger, which then struck upon a fluctuating bladder. From fundus to os the uterus was 3″ 5‴ long and 2″ 3‴ wide between the insertions of the Fallopian tubes. The walls were 8‴ thick, becoming thinner towards the cervix. The wall of the uterus was covered with a thickened mucous membrane, smooth within, and marked with pores like needle-tops. From the anterior uterine wall, and near the border of the cervical canal, was suspended, by a stalk 5‴ long and of like thickness, a bluish-red fluctuating bladder, ball-shaped, and with walls 2‴ thick. The stalk consisted throughout its whole length of fibrous decidua —that is, of a bundle of long uterine glands. It was denuded in a circle, as if the decidua reflexa had been peeled off. The bladder consisted of a chorion and of an amniotic sac. The latter contained an embryo 1″ long. The bladder was nowhere adherent to the cervical canal, which it distended.

Rokitansky concludes that in these cases an ovum, after fixing itself in the mucous membrane of the uterus, and after complete clothing with a decidua reflexa, is soon driven downwards by uterine contractions into the cervical canal, its attachment lengthening into a stalk by the growth and stretching of the decidua and the uterine glands, detachment not taking place; that the ovum continues to grow in the cervical canal, distending it, and constituting a true *cervical gestation*. The free-hanging ovum thus excites uterine contractions, and occasions from time to time hypostatic mechanical hyperæmia and then floodings. The death of the embryo is readily produced under these circumstances. And it may happen that when the ovum is ruptured by the contractions or burst by necrosis, the embryo escaping, a considerable portion of the membranes, or at least a part of the stalk, may remain behind, and form with the bleeding the basis of a fibrinous polypus, by the accretion of coagulum and fibrinous layers.

2. Professor Scanzoni, contemplating the occurrence of cases in which the choice lies between decapitation and embryulcia, decides in favour of the former operation, and describes an instrument for facilitating the performance of it. The instrument, which he calls *Auchenister*, is fourteen inches long, and consists of four pieces—1, the hook, the curve of which is to be so laid over the child's neck that the point reaches far beyond, and can be guarded by the hand; 2, the knife, with its self-closing sheath; 3, the apparatus for setting the knife in motion; and 4, the handle. After the application of the hook over the neck, a strong pull downwards by means of the hand which grasps the handle, fixes the neck as steadily as possible in the centre of the pelvic brim, whilst the hand in the pelvis fixes the instrument and sets the screw in motion. This gradually draws the driving-bar downwards towards the handle, so that the free point of the knife approaches the point of the hook. By further turns of the screw the blade is so moved that its edge completely cuts through the child's neck; then the point of the knife is moved into the concave side of the curve of the hook, rendering a wounding of the mother's soft parts impossible. After cutting through the neck, the instrument is strongly drawn downwards, so that its upper part may be passed through between head and trunk. Dr. Sickel, the reporter of the obstetric department of Schmidt's 'Jahrbuch,' says the principle of this instrument is the same as that of the instrument described by Concato.

3. We can only direct attention to a laborious, careful, and useful *résumé* of cases of inversion of the womb, by Dr. Charles A. Lee. It is especially valuable to obstetrical statists.

4. Dr. Woodson recounts a case of inversion of the womb at four months of utero-gestation. A woman, aged twenty-seven or twenty-eight, was taken with labour-pains whilst engaged in washing. Feeling the fœtus protruding, she pulled it down with great force, bringing the uterus out, and producing complete inversion. She forced the uterus back into the vagina, after tearing off most of the placenta. She was not seen until five days had elapsed. The fundus then presented externally, the organ was about the size of a large pear, a portion of placenta, almost decomposed, still adhered. An instrument resembling the uterine sound, but with a ball at the end the size of a half-ounce

bullet, was passed through a speculum, the ball being applied to the fundus. Under steady pressure the uterus went back with a sudden jerk. The pain and hæmorrhage were not great. She recovered favourably.

IV. The Puerperal Condition.

On an Epidemic of Puerperal Colpitis and Endometritis observed in the Winter of 1859–60. By Dr. Edward Martin, of Berlin. (Monatsschr. f. Geburtsk., September, 1860.)

The system of delivery in hospitals in Germany furnishes many instructive illustrations of the varieties of puerperal fever. Dr. Ed. Martin does not sanction the opinion that puerperal fever is a blood-disease, or the result of a blood-poisoning. He contends that it is necessary to analyse and to study apart the several local affections of the genital passage during the puerperal state. He says that when the lower obstetric clinique of the University of Berlin came under his direction in the autumn of 1858 and afterwards, he observed from time to time inflammations of the uterus, with more or less fever, and rarely with a fatal issue. The latter occurred chiefly after severe injuries, the result of forcible or unsuccessful attempts to deliver; and there were two cases of peritonitis following the escape of a purulent secretion from the Fallopian tube. In October and the beginning of November, 1859, there came under observation numerous illnesses, following upon endometritis and colpitis, generally with diarrhœa and metroperitonitis, but always with a favourable issue, until the morning of the 13th November, when a primipara who had suffered during gestation from a muco-purulent discharge of the vagina entered the clinique soon after the escape of the liquor amnii, and was delivered without aid of a living boy, under the usual symptoms of endometritis *in partu*. She got a chill from leaving her bed, had shivering and signs of peritonitis on the 16th, then cough and other signs of acute lung-disease, which lasted till death on the sixth day. Autopsy: copious flocculent effusion in left pleura, and recent hepatization at lower border of left lung; abdominal cavity filled with yellowish-green purulent exudation; convex surface of liver covered with a greenish exudation; spleen enlarged; uterus, tubes, and ovaries invested with a more or less adherent exudation. The ovaries were enlarged, the left contained small purulent foci; the outer ends of the tubes were very red, the fimbriæ œdematously swollen. The lymphatic vessels of the uterus were filled with purulent fluid. The veins even near placental site free from coagula. The inner surface of the uterus was covered with a discoloured, reddish, stinking sanies; in the cervix were superficial ulcerated lacerations with adhering diphtheritic covering.

Two days before this fatal event a primipara entered with aphthous colpitis marked by the aphthophyton, erosions of the vagina and eczema of the thighs. The waters had already escaped. Febrile movement followed labour; meteorism, vomiting, great dyspnœa, and death on the fourth day.

The child had died two days before of umbilical phlebitis. Autopsy: the abdominal cavity contained a large quantity of turbid, flocculent fluid; the internal genitals were covered with flocculent exudation; the spleen large, breaking down; the ovaries were œdematously swollen and contained small cavities filled with pus. The uterus was contracted; on either border was a brawny infiltration of the cellular tissue, and the lymphatic vessels were much enlarged and carried a purulent fluid. The substance of the uterus was flabby and pale; veins free from coagula; inner surface covered with a greenish sanies; the cervical canal was coated with an adherent greenish exudation. The vagina was free.

From this time until the middle of January, 1860, especially during the latter half of November and during December, 1859, 29 more out of 90 puerperæ who were delivered during these two months were taken ill, of whom 11 died. After the middle of January, until the end of May, the hospital was free from severe sickness and deaths.

The constant feature of the disease was endometritis and diphtheritic colpitis, which had in most cases begun during labour; there had usually been premature discharge of the liquor amnii, deficient pains, and defective uterine contraction after labour; not seldom hæmorrhage. The lochial discharge mostly very offensive. Dissection showed the uterus less contracted than usual at the period, coated on its outer surface in stellate patches, with a lightly adhering layer of exsudation, its substance pale, tolerably firm, the veins in most cases, excepting those of the placental site, without remarkable coagula, in a few cases bearing firm or sanious thrombi, especially where the lymphatic vessels filled with pus ran near the veins. At the placental site there were seen, as is usual in newly-delivered women, in the torn veins small blood plugs.

In the 11 fatal cases, as well as in 9 others seen by the author in other establishments, with a single exception, the lymphatic vessels of one or both sides of the uterus, from the cervix to the insertion of the Fallopian tubes, were distended with a puriform fluid, and often with coagulum; the surrounding cellular tissue was infiltrated with a clear or yellowish serum, especially at the sides of the cervix. The affected lymphatics were often traced as far as the ovaries, and in one case a lymphatic filled with pus was followed up to the right kidney. The tubes were frequently enlarged, and contained a mucous or purulent fluid. The fimbriæ were remarkably reddened, even dark-red, always œdematous, and covered with adhering exsudations. The interior surface of the uterus was covered with a reddish-brown or greenish exsudation; in the cervical canal were commonly traces of lacerations, often converted into spreading or deep ulcers; between these was formed a firmly-adhering diphtheritic, yellowish-grey layer, in small or large flakes. Most of the ulcerations resulting from lacerations at the entrance of the vagina quickly became covered with diphtheritic exsudation, which also extended to uninjured parts. This diphtheritic coating was seen by speculum during life.

In all the severe cases, repeated shivering-fits occurred sooner or later.

In many cases a pustular and furunculous inflammation appeared upon the sacrum and around the anus.

In most of the cases (but not even in all the fatal ones) were symptoms of peritonitis, with so much exsudation that the navel in one case was distended like a large apple, and being opened with a lancet, emitted a fluid, yellow, flocculent exsudation, which lasted several weeks. This patient ultimately recovered. In two other cases discharge took place through the large intestine.

The spleen was always found enlarged, its substance breaking up.

From the beginning of December, bronchitis and pleurisy were very common, and pneumonia occasional. In the fatal cases there was a copious fluid effusion in one or both pleuræ, and of commonly a plastic layer on the diseased parts of the lungs.

The heart often contained considerable coagula and masses of fibrin, in other cases some fluid blood.

During and after the epidemic, mastitis was unusually frequent, commonly with suppuration.

The course of the disease was in the severe cases very acute, the deaths happened in from four to thirteen days, once only after four weeks. Thrombosis of the veins and sanious degeneration of the thrombi very rarely complicated the disease, although at the conclusion of the epidemic there were several clear cases of thrombosis in the town. At the height of the epidemic, phlegmasia alba dolens and lobular pneumonia were seen but exceptionally.

2. In reference to *etiology*, the author remarks that from the autumn of 1858, and in the months of December, January, and February there occurred similar, but much less dangerous diphtheritic vaginal inflammations, and at the end of December he treated a woman who had a phagedenic ulcer of the cervix, with profuse bleeding from colpitis and diphtheritic cystitis, attended by marked lymphangioïtis of the left side of the vagina, of the uterus and bladder as high up as the left kidney.

As was usually observed in furious epidemics, not seldom the infants participated in the sickness of the mothers; they were with greater frequency dead-born, or died early, mostly from thrombosis of the umbilical vessels.

The contagious property of this diphtheritic exudation was made probable by the fact that every two or three of the seven cases had been attended by the same practitioners.

3. The author's account of the treatment does not call for analysis.

M. GROUX'S CASE OF CONGENITAL FISSURE OF THE STERNUM,

PERMITTING UNUSUAL OPPORTUNITIES FOR STUDYING THE MOVEMENTS AND SOUNDS OF THE HEART AND LARGE VESSELS, AND ALSO THE MOVEMENTS OF THE CHEST AND LUNGS.

M. E. A. Groux, aged twenty-eight, a native of Hamburg, has for several years been exhibiting the above-named peculiarity of his chest to members of our profession in most of the principal towns of Europe, and of late in some of the towns of America also. He paid a visit to England about five years ago, and a second one at the end of 1857; and within the last few weeks he has been paying a third visit to London. On all these occasions he has allowed close and minute examination of his chest to be made by various members of the faculty, several of whom have written descriptions of the unusual phenomena which presented themselves on observation in the album which he carries with him, and which contains similar autograph descriptions by numerous eminent foreign observers. Brief notices of M. Groux's visit, and of the appearances revealed by the deformity, have appeared in the London medical periodicals; but the most important and minute that we know of as being already published in England are those made by Dr. C. J. B. Williams in connexion with M. Groux's introduction to the Pathological Society of London in November, 1857.* As this case is one of the rarest possible, and as it allows some most interesting references to be made, we shall not apologise to our readers for noticing them here.

We find, on examining an English pamphlet printed at Hamburg, containing a number of extracts from M. Groux's album, as well as some particular notices of the case, that three other cases are on record having more or less resemblance to the present one. Thus Dr. Hughes Bennett, of Edinburgh, is in possession of a specimen which he had removed from a woman showing fissure of the sternum, a margin of bone existing on each side, to which the ribs were attached by their cartilages. Again, Harvey mentions a case in which, from accident and subsequent ulceration, there was such an opening in the left side of the chest that examination of the heart by sight and touch was practicable; and Dr. Lyons, of Dublin, describes the case of a boy in whom, owing to certain of the ribs

* See p. 71 of vol. ix. of the Transactions.

not uniting with the sternum, the movements of the heart and lungs could be seen.

In M. Groux's case the sternum is cleft along its entire length, leaving a conical hollow, with its base upwards, and its apex at the ensiform cartilage, where the two halves of the bone are held together by a strong ligamentous band. The fissure can be greatly widened or diminished by certain muscular movements acting upon the walls of the chest. It is also increased by forced expiration, by which a large bulging tumour, clear-sounding on percussion, and formed by the right lung, is produced at the upper part of the fissure, and diminished by forced inspiration. A very apparent pulsating tumour, which becomes much increased at the end of expiration, is also seen about the middle of the fissure on a line with the fourth rib; and two other pulsating tumours are to be felt in almost a vertical line with that above mentioned, one above and the other below; they are, however, less prominent, the one below being visible to a certain degree.

The question which naturally suggests itself for solution is this: What is it which constitutes the dilatable and pulsating tumour in the fissure? This query has received a variety of solutions by the various observers who in England and abroad have scrutinized the chest. We cannot, of course, detail the conflicting statements given in M. Groux's album; but it is interesting to find that by far the majority concur in looking upon this pulsating swelling as being, in fact, the *right auricle*. Some look upon it as part of the right ventricle, others as the aorta, and others, again, think the tumour is partly the right auricle and partly the right ventricle. The most complete opinions expressed in the extracts from M. Groux's album, constructed prior to his recent visit in this country, are those of Dr. C. J. B. Williams, of London, and Dr. Gairdner, of Edinburgh. We think it will prove serviceable and interesting if we transcribe them *in extenso*.

Dr. Gairdner says as follows:

"The upper *visible* pulsation (A) is auricular—probably of the right auricular appendage—and precedes the arterial pulse which is felt above it by a very appreciable interval. It precedes the apex-beat by an interval appreciable, but not so easily appreciable. Care must be taken to press lightly on the pulse A in making this observation, otherwise the ventricle is felt pulsating below what I take to be the auricle, and is, of course, synchronous with the apex-beat. It precedes the lower visible pulsation (B) by an appreciable interval. The only difficulty here arises from the fact that the movement is in opposite directions. B is rather a movement of retraction than a proper pulsation.

"The pulsation A increases, and the auricle slowly fills under the eye, when M. Groux suspends respiration for a time. This phenomenon, like the welling of water into a basin which fills from below, can be easily distinguished from the protrusion of the lung which takes place when a sudden respiratory movement concurs with a closed glottis. When M. Groux coughs, the lung protrudes, but not the auricle; when he simply ceases to breathe for several seconds, without either expiring or inspiring, the auricle protrudes but not the lung. Percussion also shows the difference between the two. This gradual filling of the right auricle, when respiration is suspended, and the enlargement of its pulsations which follows, is quite in accordance with what is seen in vivisections when partial asphyxia is gradually induced. In forced respiratory movement the glottis closed, the protruded lung conceals the auricle and interferes with the observations of its phenomena. In forced inspiration the heart is drawn back from the thoracic wall. The favourable state, therefore, for noticing the gradual filling of the auricle is intermediate. I presume the movement, which I have called B, the lower visible pulsation, to be in connexion with the systole of the right ventricle. It is only visible or felt in operation because it requires the descent of the diaphragm to bring the heart down to the left costal margin, where this movement is observed, and it is a movement "of retraction because the systole of the ventricle withdraws it from the surface. A similar movement is occasionally seen in men perfectly well formed, perhaps not in perfect health, but without marked disease of heart. During suspended respiration the apex-beat in the usual situation becomes indistinct, and may perhaps finally be lost, though I have generally found it continue. At the same time a movement becomes apparent in the third, and then in the second intercostal space. This phenomenon is very curious, and well worthy the attention of physiologists. It was attributed by some of those who observed it at the Medico-Chirurgical Society here (Edinburgh), to be an actual displacement upwards of the apex. I am rather of the opinion that it is owing to the increasing distension of the right ventricle, which throws back the true apex of the heart from the thoracic walls. In the strictly normal condition of parts the same change takes place so far as the disappearance of the true apex-beat is concerned; but I have not seen the pulsation higher up except in disease. In adherent pericardium and even hypertrophy of the right ventricular, it is not infrequent. In M. Groux, when the heart's action is excited, it can be easily felt on deep pressure in four intercostal spaces simultaneously. With regard to the reduplication of the first sound which I formerly noted, I am still of opinion that it is probably an auricular element, and is dependent on the want of synchronism between the auricular and ventricular contractions. But we are too little sure of the causes of the first sound to allow of our speculating on the matter."

The statements of Dr. Williams will be seen on one or two important points to differ from those of Dr. Gairdner. They are as follow:

"*Motions.* That the visible pulsation in the middle third of the sternal vacuity is chiefly seated in the right auricle appears to me obvious from its resemblance to the same motion observed in the exposed heart of the ass, and from the fact, distinctly perceptible in slow pulsations, that the motion immediately precedes the ventricular systole, the wave of motion beginning with the auricle and rapidly pouring downwards to the ventricle. In quick pulsation the motion of the auricle is felt in the sternal space, and the systole of the ventricle, as felt and heard in its region, seems to be synchronous; but I repeat, in the more deliberate movement, when the pulse ranges about sixty-five per minute, the wave-like progression can be traced from the auricle to the ventricle, the upper margin of which sometimes comes into view at the lower portion of the sternal vacuity. To the rapid systolic movement succeeds a slow dilatation, and that this proceeds from the flow of blood into the auricle from the venous trunks is made more plain by the fact that pressure on the abdomen which follows the blood onwards through the ascending vena cava, accelerates the expansion of the auricle. The aorta can be felt pulsating more deeply seated above the position of the auricle, and a like pulsation of the pulmonary artery may be felt through a portion of the auricle close to the left margin of the fissure.

"*Sounds.*—The sounds accompanying the respective motions can also be distinguished clearly only in the slow pulsations. By aid of a small flexible ear-tube with a narrow pectoral end, I was enabled to hear a distinct sound accompanying the commencement of the auricular contraction. It is faint and short, or flapping, and ends in the less abrupt and more distinct sound of the ventricular systole. When the stethoscope is placed over the ventricle, the flapping sound of the auricle is not heard, but the ventricle swells or rolls out its peculiar sound till it ends with a sharp clack of the diastolic or valvular sound. I infer from the preceding observations, as well as from similar ones made twenty years ago from animals, that each movement of the heart bore its proper sound, and that the reason why the auricular sound is not usually heard is that it is too faint to pass through the intervening lung and wall of the chest. The diastolic valvular or second sound is remarkable for its clearness and loudness in this case, and on one occasion I found it reduplicated or double, and I had the opportunity of verifying the explanation which I gave of this decided diastolic sound more than ten years ago. When the end of the stethoscope was placed over the aorta above the auricle, the diastolic sound was simple, but on carrying the instrument a little downwards and to the left, so as to carry a part of the pulmonary artery, the sound became double, the whole heart's sound being expressed by the syllables *lubb-durrup*, instead of *lubb-dup* as usual, obviously from the two sets of valves closing in succession, not simultaneously. This want of coincidence in the closure of the aortic and pulmonary valves is only occasional, but as it often presents itself as a phenomenon of disease, it is satisfactory thus to be able to trace it to its cause. The only remaining observation which I have to record is that of a short rushing murmur heard in the course of the right carotid artery, which must be produced there, as it is not audible in the aorta or over the course of the innominata."

Such were the phenomena as they appeared to Dr. Williams when M. Groux visited England on a previous occasion; and Dr. Williams has kindly favoured us with the following remarks in connexion with M. Groux's recent visit, when he was again examined by him.

"What (he says) is stated in the former report has been fully confirmed by the test of Dr. Alison's sphygmoscope and Dr. Upham's ingenious application of the electric telegraph. By these it is proved that the motions follow each other in the following succession:—1, auricle, and 2, ventricle (systole); 3, aortic pulse; 4, radial pulse. Each of the three first of these has its proper sound, the distinctness of which is appreciable in the stronger and slower pulsations. 1. A short superficial sound in the auricle, continuously succeeded by—2. The longer and deeper rolling sound of the ventricle. 3. A short deeper sound in the aorta, which may be traced into the chief adjoining structures. Then follows the diastolic second sound, which in M. Groux's case appears to be permanently doubled, for the reason I before explained, the reduplication being chiefly audible when the stethoscope is directed inwards to the left of the fissure, so as to include the pulmonary as well as the aortic valvular sound."

The experiments alluded to above, as contrived by Dr. Upham, of Boston, in America, are partially described in the printed extracts from M. Groux's album, and were exhibited by M. Groux on his own person. We cannot here enter into their detailed description; suffice it to say, that by means of the motion produced by the rising or falling of acidulated fluid within flexible tubes attached to bell-glasses carefully adapted simultaneously to the surface of the pulsating tumour, and over the apex of the heart, the electro-magnetic circuit was broken. By means of Morse's double register, properly adjusted, two sounds were given forth, differing in pitch, and at the same time the motions produced by the breaking of the circuit were recorded on paper in the same way that ordinary telegraphic communications are written. "Then by the intervention of the electric clock, which was also made to mark its seconds on paper, it was easy to measure the time of the pulse-beats themselves, as well as the interval in the pulsation of any two points in the round of the circulation."

Dr. Upham, experimenting in this way, arrived at the following conclusions concerning the question of synchronism or non-synchronism of the various motions of the heart and large vessels. Firstly. That a minute but distinctly appreciable interval of time elapsed between the impulse of the oval tumour seen in the middle of the sternal fissure and the shock of the heart between the fifth and sixth ribs. Secondly. That a slightly increased interval was manifest between the impulse of the tumour and the beat of the aorta at its arch. Thirdly. That a still greater interval was perceptible between the pulsation of the tumour and the radial pulse. Fourthly. That the interval between the apex-beat and the radial pulse was slightly but appreciably less than that between the oval tumour and the radial.

Dr. Upham suggests that possibly the knowledge of these facts may afford additional means to our resources for diagnosis in aneurism and other obscure diseases of the aorta and great vessels in the thorax and abdomen.

MEDICAL INTELLIGENCE.

Army Medical School.

The qualifications, examinations, and subsequent special instruction of candidates for commissions in the medical service of the British and Indian armies, are now, and will be in future, one and the same; and it is this subsequent special instruction which has called forth the existence of the Army Medical School at Fort Pitt, Chatham, where the candidates now spend from four to five months before receiving commissions.

The first session of this school commenced on Tuesday, the 2nd of October last. There were present the Right-Honourable Mr. Sidney Herbert, the Secretary of State for War; J. B. Gibson, M.D., C.B., Director-General of the Army Medical Department; Major-General Eyre, Commanding the Garrison at Chatham; Colonel Twiss, Commanding the Royal Engineers; Dr. Sutherland; Dr. Hume, Deputy Inspector-General and P.M.O. at Chatham; Professors Parkes, Longmore, and Aitken.

The candidates (forty-three in number) were also present, as well as several officers, combatant as well as medical, belonging to the army and the navy.

Thomas Longmore, Esq., Deputy Inspector-General and Professor of Military Surgery, delivered the introductory lecture; and we are extremely sorry that this address has not yet been published. We may probably see it issued from some departmental office long after the special interest of the subjects which it deals with has passed away. The establishment of a fresh element in army medical education in England is an important work, and had Mr. Longmore's lecture been published it would have shown, more clearly than has been yet done, the objects contemplated in the establishment of this school, and the mode of teaching which is being there pursued. Not a little misapprehension has prevailed on these points; and especially as regards the relation of this new institution to our civil schools. Mr. Longmore gave a most interesting historical account of the succession of events which have led to the formation of this school. He explained its plan, and showed the kind of good which might be expected to result, not only to the members of the medical department of the army, but to the State at large from the working of this school. After commenting upon the degraded position of army medical officers a hundred years ago, he showed how very gradually the importance of obtaining well-educated surgeons for the army began to be appreciated.

At the time of the Russian war in 1854-55, it was shown that as far as regards pay and social status the surgeons of the British army were as advantageously placed as the surgeons of foreign armies, with whom they were brought into contact; but that in respect of the opportunities afforded for obtaining special knowledge in military medical and surgical art and duties, the latter had long possessed exclusive advantages in the schools of instruction established by continental sovereigns and governments for the improvement of their military surgeons, and the collections of regular medical reports and observations from army hospitals, which in France were commenced nearly a century ago. The Russian war, however, was destined to lead to important changes, by which the position of the British medical officer has been greatly improved; in fact, a complete reorganization of the army medical department has taken place since then.

Its plan of administration, its relation to other departments, the rules affecting its interior economy, the mode of admission of new members into its ranks, and the preparations for its duties, have all been remodelled; and the establishment of the Army Medical School (temporarily at Chatham), is part of the plan for the more efficient preparation of the army surgeon—a plan by which an opportunity will be afforded to each novice of informing himself minutely regarding the specialties of military medical knowledge—a point of great practical usefulness to him in his subsequent military career. The want of an institution of this nature had been long under the notice of the British Government; indeed, ever since 1798, when the distinguished surgeon, Mr. John Bell, made an earnest appeal on the subject at the time of his going from Edinburgh to Yarmouth to watch the treatment of the wounded sailors after the battle of Camperdown. He proposed one great school of military surgery, and enumerated a list of topics which should be specially studied by naval and army surgeons. This appeal appeared to have had some influence in the subsequent institution of the Military Surgery Chair at Edinburgh, first occupied by Dr. John Thomson, in 1806. Then, also, Dr. Robert Jackson advocated the establishment of an Army Medical Practical School, in connexion with the Invalid Depôt at the Isle of Wight. The details of his plan are almost similar to those which have been now at last adopted through the recommendation of the Royal Commission, presided over by Mr. Sidney Herbert, and appointed to inquire into the sanitary condition of the army, and through the persevering exertions of Miss Nightingale. It was shown that hitherto the studies and knowledge of the probationer for military practice had all tended towards the cure of diseases or the repair of injuries, and the diseases and accidents which he had chiefly considered were those which belong to civil life and an European climate. But the cure of diseases is by no means the chief duty of an army surgeon. He is usually placed in medical charge of a body of men in the prime of life and sound of constitution, and he will best perform his functions who has the least disease to treat, and who keeps his men in the most efficient state of health, ready for any duty. Hence a train of reasoning, as well as habits of thought and action, are required, which are unlike those prevailing in civil life. Hence the necessity of the special study of every topic of sanitary import, and the practical application of the principles of hygiene to the circumstances of bodies of troops and the life of the soldier. The sanitary duty is now a defined and special office of army medical men.

Mr. Longmore then described the arrangements for the work of the school. The practical work of each session will occupy four months, and there are two sessions in each year. In the course hygienic instruction will be given on all the causes which specially influence the health of soldiers; there will be practical instruction in the methods of determining the quality and quantity of his rations, of the fluids he drinks, and of the air he breathes. The principles and methods of ventilation, warming, draining, and construction of hospitals and barracks will be fully explained and illustrated. The geographical distribution of diseases, the history of camp diseases and of army epidemics, all come within the sphere of instruction of the Professor of Hygiene; and occasional visits of inspection to barracks, hospitals, transport ships, will be made whenever an opportunity occurs.

The Professor of Surgery will initiate the candidate into the regular order of duty connected with a military general hospital, and make him familiar with the authorized mode of recording and registering cases, compiling returns, and preparing such reports as are required for the statistical and professional information of the military authorities. Cases will be placed in charge of each candidate for observation and treatment. Physical malformations and defects overlooked in recruiting will be also brought under observation here, while the system of examination of recruits will be part of the daily morning's work; the medical examination of invalids will also be a frequent exercise. Practice in the application of bandages and splints, opportunity for regional dissections with reference to wounds and operations, and for the practice of all surgical operations on the dead body will form part of the practical work which the Professor of Surgery will superintend and direct. It will be his duty also to give lectures on surgical diseases; on gun-shot wounds and other accidents of war; on the means of transport of the sick and wounded in various countries. The pathological and model museum will afford valuable assistance in illustrating these topics. It is also the duty of the Professor of Surgery to instruct the candidate as to the relations of the army surgeons to the officers of other departments of the military service, to explain the regulations authorized for maintaining discipline in the army.

The Professor of Pathology will have to superintend the post-mortem inspections and the subsequent investigations in morbid anatomy. Each candidate will be supplied with one of Smith and Beck's educational microscopes, and instructed in the use of the instrument, and in the mode of preparing objects for microscopic examination, and with such aid he will pass through a course of practical observation on the minute anatomy of morbid tissues. It will be the duty of the professor not only to describe the post-mortem results, but also to give a complete history of each case, by collecting together all the clinical information that can be got from the case-books, and the medical history which will be attached to every soldier.

Opportunities will thus be given for acquiring a considerable amount of practical knowledge, not otherwise easily obtained; and the advantages of these pathological studies will not be confined to the school, but military medical officers generally on duty will be induced, it is hoped, to send home more complete histories of cases under their charge when they know that such information is being made such important use of at home.

The extensive collection of diseased structures in the museum at Fort Pitt will be made use of by the Professor in his lectures and demonstrations. The best modes of making and preserving pathological preparations will be also practically taught. The model museum, which was formed at Dublin by the energetic exertions of Professor Tufnell, has now also been transferred to Chatham; and when this has been sufficiently extended and provided for, so as completely to illustrate the various modes of conveying sick and wounded soldiers adopted by different armies and in different countries, according to peculiarities of climate or surface—all the various articles of camp and hospital equipment—there will be a most extensive means of educating through the eye as well as through the ear, which cannot fail to be followed by much practical benefit.

The course on military medicine will consist also of two parts—practical or clinical instruction in the wards, and systematic lectures on army diseases. It will embrace the various methods of diagnosis and the history and treatment of such diseases as are presented by sick soldiers returning from foreign stations, and also histories of the rise and progress of particular epidemics in British and other armies—of the diseases induced in soldiers by fatigue, exposure, particular duties, and intemperance, and such other causes as may arise in the various colonies where soldiers are required to serve.

Professor Longmore closed his well-spoken and interesting lecture by calling upon the candidates, as well as his brethren in office, to show by their zeal and industry that they do all in their power to discharge that debt of gratitude demanded for the benefits conferred on army medical officers by recent departmental changes; and that the surest way of doing so will be by each, in his particular sphere, contributing to solve the great problem of lessening the rates of sickness and mortality in her Majesty's forces.

At the conclusion of the lecture, Mr. Sidney Herbert addressed the assembly at once clearly and effectively. He pointed out the difference in the position and duties of the army medical officer which the new warrant and regulations had made, and concurred in the opinion so well expressed by Professor Longmore, that the country would expect the medical officers to justify the increased rank and pay which had been conferred upon them by an increased attention to the health of the soldiers. He referred to the excellent accounts which he had received from China, where the appointment of a sanitary officer had been of the greatest service in maintaining the health of the troops, and where a spirit of emulation had sprung up amongst the combatant officers in preserving the health of their men, and in their appreciation and adoption of the advice of their medical officers.

After a few suitable remarks by General Eyre and Dr. Gibson, the Director-General, the meeting broke up; and we have since heard, with pleasure, that the practical work of the school is being most diligently prosecuted. We wish the institution good speed, and hope it may, in the course of time, realize all the good results which may reasonably be anticipated.

The New Sydenham Society.

As this Society has from its extent and importance become of such great interest to the profession at large, we are sure our readers will read with pleasure the following notices issued by the Council:

LIST OF WORKS ALREADY ISSUED.

VOL. I. ON SYPHILIS IN INFANTS. By PAUL DIDAY. Translated by Dr. WHITLEY.

VOL. II. ON THE MORE IMPORTANT DISEASES OF WOMEN AND CHILDREN, with other Papers. By Dr. GOOCH. Reprinted, with a Prefatory Essay by Dr. ROBERT FERGUSON. Woodcuts.

VOL. III. MEMOIRS ON DIPHTHERIA: containing Memoirs by Bretonneau, Trousseau, Daviot, Guersant, Bouchut, &c. Selected and translated by Dr. R. H. SEMPLE.

VOL. IV. ON THE MINUTE STRUCTURE AND FUNCTIONS OF THE SPINAL CORD. By Professor SCHRŒDER VAN DER KOLK:

ON THE MINUTE STRUCTURE AND FUNCTIONS OF THE MEDULLA OBLONGATA, AND ON THE PROXIMATE CAUSE AND RATIONAL TREATMENT OF EPILEPSY. By Professor SCHRŒDER VAN DER KOLK.

Translated by Dr. W. D. MOORE, of Dublin. In one volume, with numerous Lithographs.

VOL. V. EXPERIMENTAL RESEARCHES ON THE EFFECTS OF LOSS OF BLOOD IN INDUCING CONVULSIONS. By Drs. KUSSMAUL and TENNER. Translated by Dr. BRONNER, of Bradford:

ON THE PROCESS OF REPAIR AFTER RESECTION AND EXTIRPATION OF BONES. By Dr. A. WAGNER, of Berlin. Translated by Mr. T. HOLMES:

PROFESSOR GRÆFE'S THREE MEMOIRS ON GLAUCOMA AND ON IRIDECTOMY AS A MEANS OF TREATMENT. Translated by Mr. T. WINDSOR, of Manchester.

In one volume, with numerous woodcuts.

VOL. VI. MEMOIRS ON ABDOMINAL TUMOURS AND INTUMESCENCE. By Dr. BRIGHT. Reprinted from the 'Guy's Hospital Reports,' with a Preface by Dr. BARLOW. Numerous woodcuts.

VOL. VII. A CLINICAL ACCOUNT OF DISEASES OF THE LIVER. By Professor FRERICHS. Vol. I. Translated by Dr. MURCHISON. With numerous Woodcuts.

VOL. VIII. A YEAR-BOOK FOR 1859. The Year-Book is a Register in Condensed Abstract of all Important Communications, whether British or Foreign, in Medicine, Surgery, and their allied Sciences during the year. It consists of five sections:—1. Anatomy and Physiology. Edited by Dr. HARLEY.—2. Medicine. Edited by Dr. HANDFIELD JONES.—3. Surgery. Edited by Mr. HULKE.—4. Diseases of Women and Children. Edited by Dr. GRAILY HEWITT.—5. Forensic Medicine and Toxicology. Edited by Dr. ODLING.

The whole of the above can be obtained by new members, but early application is desirable, as the stock on hand is very limited.

THE ATLAS OF SKIN DISEASES.

It is proposed to issue three Portraits (of life size) each year. Those to be first issued are selected from the magnificent series published by Professor Hebra, of Vienna. The first fasciculus (part of the present year's series) will probably be ready towards the end of December.

WORKS IN PREPARATION.

The following works are in course of preparation for the Society:—

VOGEL AND NEUBAUER ON THE EXAMINATION OF THE URINE: a manual intended for the assistance of the practical physician. Translated by Dr. Markham. With Lithographs and Woodcuts.

A YEAR-BOOK OF MEDICINE AND SURGERY FOR 1860.

FRERICHS' CLINICAL ACCOUNT OF DISEASES OF THE LIVER, VOL. II. Translated by Dr. Murchison. With Woodcuts.

CASPER'S MANUAL OF FORENSIC MEDICINE. Translated by Dr. G. Balfour, of Edinburgh.

CZERMAK'S MONOGRAPH ON THE LARYNGOSCOPE AND ITS PRACTICAL APPLICATIONS. Translated by Dr. G. D. Gibb. With Woodcuts.

PROFESSOR DONDERS ON THE DISEASES OF ACCOMMODATION OF THE EYE. With Notes by the Author. Translated by Dr. W. D. Moore, of Dublin.

SMELLIE'S MIDWIFERY. Reprinted, with Notes and Preface, &c., bringing the Work up to the present standard of knowledge; by Professor Simpson, of Edinburgh.

We are glad to hear that Czermak has kindly offered to revise the coming English translation of his work on the Laryngoscope, and to add any notes or new matter that may appear necessary.

BOOKS RECEIVED FOR REVIEW.

Clinical Researches on Diseases in India. By C. Morehead, M.D. Second Edition. London, 1860. pp. 774.

On Organic Polarity, showing a connexion to exist between Organic Forces and ordinary Polar Forces. By H. F. Baxter, M.R.C.S. pp. 187.

Electro-Physiology and Electro-Therapeutics, showing the best methods for the Medical Uses of Electricity. By Alfred C. Garrett, M.D., Fellow of the Massachusetts Medical Society, Boston. pp. 708.

Analysis of My Sight, with a view to ascertain the Focal Power for Horizontal and Vertical Rays. By T. Wharton Jones, F.R.S. (Reprint from Proceedings of Royal Society, vol. x. p. 380.)

The Invention of Stereoscopic Glasses for Single Pictures, with preliminary Observations on the Stereoscope and on the Physiology of Stereoscopic Vision. By T. Wharton Jones, F.R.S. pp. 31.

On the Nature and Treatment of Diseases of the Heart, &c. &c. By J. Wardrop, M.D. Second Edition. Edinburgh. pp. 667.

On Myalgia, its Nature, Causes, and Treatment; a treatise on Painful and other Affections of the Muscular System which have been frequently mistaken for various Diseases. By T. Inman, M.D., Physician to the Liverpool Infirmary, &c. London, 1860. pp. 307.

On Gout; its History, Causes, and Cure. By W. Gairdner, M.D. Fourth Edition. London. pp. 430.

On Diphtheria. By E. H. Greenhow, M.D., F.R.C.P., Lecturer on Public Health at St. Thomas's Hospital. London, 1860. pp. 274.

On Hæmoptysis as a Symptom. By John Ware, M.D., formerly Hersey Professor of the Theory and Practice of Medicine in Harvard University. (Reprint.) Boston. p. 31.

On the Admission of Air into Serous Cavities. By A. Meadows, M.D. 1860. pp. 16.

Recherches sur le Substitution Graisseus du Rein. Par M. le Docteur E. Godard, &c. Paris, 1859. pp. 31.

Clinical Lectures. By R. B. Todd, M.D., F.R.S. (Second Edition.) Edited by L. S. Beale, M.B., F.R.S., Physician to King's College Hospital, &c. London, 1860. pp. 912.

Clinique Médicale sur les Maladies des Femmes. Par M. G. Burnutz, Médecin de la Pitié, et M. E. Goupil, Médecin du bureau central. Tome premier. Paris, 1860. pp. 591.

Application of the Button-suture to the Treatment of Varicose Dilatation of Veins. By N. Bozeman. New Orleans. pp. 8.

Practical Researches on Stricture of the Rectum. By Jolliffe Tuffnell, F.R.C.S.I., Surgeon to the City of Dublin Hospital. Dublin, 1860. pp. 15.

Traité pratique des Maladies de l'Enfance, fondé sur de nombreuses Observations Cliniques. Par F. Barrier, ex-chirurgien-en-chef de l'Hôtel-Dieu de Lyon, Professeur de Chirurgie, &c. Troisième Edition. Vols. I. and II. Paris and Lyons, 1861. pp. 720 and 736.

Den Syphilitiske Hjernelidelse, Afhandling for den Mediciniske Docktorgrad. Af Valdemar Steenberg, Tredie Læge ved Sindssygeanstalten ved Slesvig, Kjobenharn. Bianco Lunos Bogtrykkeri ved F.S. Muhle, 1860. Small 8vo. pp. 269.

Anatomy of the Arteries of the Human Body, Descriptive and Surgical, with the Descriptive Anatomy of the Heart. By J. H. Power, M.D. Illustrated by B. W. Richardson, F.R.C.S.I. Dublin, Fannin. pp. 374.

The Principles and Practice of Surgery. Wm. Pirrie, F.R.S.E., Professor of Surgery in the University of Aberdeen, &c. Second Edition. London, Churchill, 1860. pp. 871.

A System of Surgery: Pathological, Diagnostic, Therapeutic, and Operative. Illustrated by nine hundred and thirty-six Engravings. By Samuel D. Gross, M.D., Professor of Surgery in the Jefferson Medical College of Philadelphia, &c. Philadelphia, Blanchard and Lea. Vols. I. and II.

The Surgical Diseases of Children. By J. Cooper Forster. London, Parker and Son, 1860. pp. 348.

Lectures on the Surgery of Childhood, delivered at the Hospital for Sick Children. By Athol A. W. Johnson. Reprinted from the British Medical Journal. London, 1860.

Urethro-Vaginal, Vesico-Vaginal, and Recto-Vaginal Fistulas, General Remarks: Report of Cases treated with the Button-suture. (Reprint from the New Orleans Medical and Surgical Journal, 1860.) By N. Bozeman, M.D. New Orleans. pp. 56.

Public Health. The Right Use of Records founded on Local Facts. By H. Wyldbore Rumsey. London, Parker & Son. pp. 22.

New Views on the Causes, Symptoms, and Treatment (chiefly dietetic) on Irritative Congestion of the Wind-pipe, with Remarks on Sea Air. By C. B, Garrett, M.D., Hastings. London, Churchill. pp. 59.

The Climate of Worthing: its Remedial Influence in Disease, especially of the Lungs. By W. G. Barker, M.D. London, Churchill. pp. 72.

On the Successful Treatment of Gonorrhœa and Gleet without Copaiba. By T. Weedon Cooke, Surgeon to the Royal Free Hospital. London, Renshaw, 1861. pp. 32.

The Modern Treatment of Syphilitic Diseases, both Primary and Secondary, comprising the Treatment by a safe and successful method. By Langston Parker, F.R.C.S., late Surgeon to Queen's Hospital, Birmingham. London, Churchill. pp. 403.

Recherches Teratologiques sur l'appareil Seminal de la Homme. Par M. E. Godard. &c. &c. 1859.

A Practical Treatise on Diseases of the Urinary and Generative Organs. By W. Acton. Third Edition. London, Churchill, 1860. pp. 608.

Etudes sur la Menorchidie et la Cryptorchidie chez l'Homme. Par M. E. Godard, ancien Interne des Hôpitaux de Paris, &c. (Extrait des Mémoires de la Société de Biologie, 1856.) Paris, Masson. pp. 180.

The Anatomy and Physiology of the Placenta: the Connexion of the Nervous Centres of Animal and Organic Life. By J. O'Reilly, M.D., Resident Fellow of the New York Academy of Medicine, &c. New York, Hall, Clayton, and Co. pp. 111.

On the Reparative Process in Human Tendons after Subcutaneous Division for the Cure of Deformity. By W. Adams, F.R.C.S., Surgeon to the Royal Orthopædic and Great Northern Hospitals, &c. London, Churchill. pp. 175.

The Principles of Physiology applied to the Preservation of Health and the improvement of Physical and Mental Education. By A. Combe, M.D., F.R.C.S. (Edin.) Fifteenth Edition. Edited by J. Coxe, M.D. Edinburgh and London. pp. 336.

A System of Instruction in Quantitative Chemical Analysis. By Dr. C. R. Fresenius, Professor of Chemistry and Natural Philosophy, Wiesbaden. Third Edition. Edited by J. Lloyd Bullock, F.C.S. London, Churchill, 1860. pp. 687.

Chemistry in its Relation to Physiology and Medicine. By George E. Day, M.D., M.A. Cantab., F.R.S., Professor of Medicine in the University of St. Andrews. London, Baillière. pp. 526.

The Elements of Natural Philosophy: or, an Introduction to the Study of the Physical Sciences. By Golding Bird, M.D., F.R.S., and Charles Brooke, M.A., M.B., F.R.S. Fifth Edition. London, Churchill. pp. 699.

The First Step in Chemistry. A New Method for Teaching the Elements of the Science. By A. Galoway, F.C.S., Professor of Practical Chemistry Museum of Irish Industry, &c. Third Edition. London, Churchill, 1860. pp. 297.

Vestiges of the Natural History of Creation. Eleventh Edition. London, Churchill, 1860. pp. 286 and lxiv.

Electro-Physiology and Electro-Therapeutics, showing the best Methods for the Medical Uses of Electricity. By Alfred C. Barrett, M.D., Fellow of the Massachusetts Medical Society. Boston, Trübner and Field, 1860. pp. 708.

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NOTICE TO READERS.

THE Editor is particularly desirous of having all Reports of Hospitals, Asylums, Sanitary Boards, Scientific Societies, &c., forwarded to him; as also Inaugural Lectures, Theses, Medical and Scientific Addresses, &c.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

APRIL, 1861.

PART FIRST.

Analytical and Critical Reviews.

Review I.

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2. *Johannes Müller.* Eine Gedächtnissrede, gehalten bei der Todtenfeier am 24 Juli, 1858, in der Aula der Universität zu Berlin, von Rudolf Virchow.—*Berlin*, 1858. pp. 48.
John Müller. An Eulogistic Oration pronounced at the Celebration of his Death, on the 24th of July, 1858, in the Hall of the University at Berlin, by Rudolf Virchow.

3. *Éloge d'Orfila*, par P. Bérard, Professeur de Physiologie à la Faculté de Médecine de Paris, Inspecteur-général des Facultés et Écoles Secondaires de Médecine de France, Membre de l'Académie Impériale de Médecine, Officier de la Légion d'Honneur; Prononcé dans la Séance de Rentrée de la Faculté, le 15 Novembre, 1854.—*Paris*, 1854. pp. 56.
An Eulogistic Oration on Orfila, by P. Bérard, Professor of Physiology to the Faculty of Medicine in Paris, &c.

There is nothing so interesting and instructive as the lives and records of great men. They constitute, indeed, the brighter part of that universal history which has been defined to be "philosophy teaching by example." Whether in the political, the literary, the scientific, or even the purely social field, the attribute of greatness is the attribute of beneficent power to improve others, and inspire them with hope and confidence in their destinies. Such greatness is more or less a part of every profession, and especially of that one whose perfection lies in the variety of acquirements necessary for its fullest culture and development. This variety is presented to us in the great names which are prefixed to this article. And without further preface, we invite attention to the appropriate literary conjunction of those two workers and thinkers, who respectively represent the sciences which have made the most rapid strides during the present century.

JOHN MÜLLER was born on the 14th of July, 1801, at Coblentz, on the Rhine—the same city (then under German rule) having produced the immortal Cuvier. His grandfather was a vine-dresser on the Moselle; his father, Mathias Müller, a shoemaker in good circumstances. His mother's name was Maria Theresa Wittmann. John was the eldest of five children, two of which were girls. In common with his brothers and sisters, though in a greater degree, he inherited from his father a striking physiognomy, a stoutly-knit frame, and a dignified presence. From his mother he derived a strong sense of order and arrangement, an active enterprising spirit, and unwearied activity. These characteristics are what achieved the wonderful future of the young Müller, and confirm what history is never weary of repeating, that true greatness descends through the maternal channel. The boy, too, was thoughtful and reflective, thoroughly in earnest about all that he did, and carrying out every undertaking with enduring perseverance. The scene of his childhood was likely to influence his young imagination; for there he saw ruins of castles to which some heroic legend was attached, heard the solemn services of the Catholic religion, "the neighing steed and the shrill trump which make ambition virtue," in the troops of the First Napoleon, then in occupation of Coblentz.

In 1810 the young Müller was sent to an *école secondaire*, which was reorganized by the Prussian Government in 1816, and where, after this latter date, he found Professor Leutzinger, a pupil of Pestalozzi, teacher of mathematics, to whom he acknowledges special obligations. At school he soon became distinguished. Mathematics and drawing were his favourite pursuits; but he was a superior classical scholar, translating and explaining Plato and Aristotle, and writing Latin (it was said) better than German. His rare gifts were especially seen in the independence of all his efforts, the native strength with which he employed any matter given to him, and the activity with which he fed (without satiating) his great desire for knowledge. He greedily read Goethe, who was destined to exercise considerable influence on some of his youthful works. He carefully observed everything in field and forest, collected butterflies and plants, and is said even to have begun the dissection of animals, though he possessed such acute sensitiveness that he could not endure the sight of a spider, even in after years, when he had made important discoveries about Arachnidæ. The elder Müller, lacking either ambition or discernment, wished his son to be a saddler. But the mother, and Herr Johannes Schulze, who had conducted our young scholar's classical studies, prevailed that he might not be so thrown away. On leaving the Gymnasium in the autumn of 1818, he was obliged to serve for a year in the Prussian army, after which he entered the University of Bonn. The splendour of ecclesiastical ritual had at this time evidently worked upon the ardent imagination of the young Müller, and it was not till after great doubt and consideration that he determined to devote himself to the study of medicine. A struggle against youthful prepossessions is betrayed in his subsequent declaration to a friend, with respect to his ultimate choice: "There I know what I have, and whom I serve." And at first the reaction appears to have been excessive, though he afterwards retracted the saying, "What does not fall under the knife, is as nothing," in which he expressed his satisfaction in the certainties of physical science. In 1821 he gained the prize given by the Medical Faculty to the new high school at Bonn, in an essay on the respiration of the fœtus. This subject had been surrounded with obscurity since the time of Harvey, and the young Müller's dissertation is most remarkable when we compare the author's youth with his literary knowledge, his comprehensive treatment of the subject, and his various and bold experiments. In 1822 he published, in the first number of the 'Isis,' a treatise "On the Laws and Numerical Relations of Motion in the Different Classes of Animals, with especial Reference to the Motion of Insects and Polymerea." It is curious that he should have directed his attention to creatures which peculiarly excited his disgust: but doubtless this disgust appeared to him in itself a physiological, or at least a psychical problem not unworthy of solution. In the same year Müller took the affirmative in the question, "De Phoronomiâ Animalium," for his inaugural dissertation, and was created Doctor of Medicine. At this time, however, he was a disciple of the theory of "polar opposites;" and indeed, according to his own confession, his philosophy was a compound of all manner of conceits upon which he afterwards looked back with horror.

As with many illustrious men before him, the studies of our young physiologist were now carried on with great difficulty, by reason of poverty. His father's death left him in great embarrassment, and he was incessantly struggling with want and privation until he had earned for himself a European fame. But these things did not lessen his confidence in the future. His diligence, too, was immense. He mastered every language in which philosophers and naturalists wrote; read from Aristotle to Bacon—from Plato to Giordano Bruno and Spinosa, and was constantly at work with his scalpel and his microscope. Yet he was an open and generous companion, and in every sense meet for the "enjoyment of another's individuality."*

Müller was happy in the possession of one friend who incessantly brought his claims to notice and support before the minister Von Altenstein. This friend was Philipp Joseph Von Rehfues, Extraordinary Government Procurator of the University of Bonn. So zealous indeed was Rehfues in Müller's behalf, that he even proposed to provide him with money for his support out of the funds of the Catholic Theological Faculty, and insisted on the political advantage which would arise from such patronage to a native of Coblentz, a town of only recent Prussian acquisition. These representations met with full sympathy from the minister. Müller received the support he needed,

* Schleiermacher's happy definition of Friendship.

but on condition that he should continue his studies at Berlin instead of Paris, to which capital he had thought of betaking himself. To Berlin he accordingly went in the spring of 1823, and met with the warmest reception from Rudolphi, the Professor of Anatomy and Physiology in that University. Here he had free access not only to the public museums, but to the private library and collections of Rudolphi, who completely settled his pupil's taste for physical science, and gave him, on his return to Bonn in 1824, an English microscope, at that time of great value. During his residence in the Prussian capital, Müller became personally acquainted with the minister Von Altenstein, and (what he prized most of all) with the younger Meckel, then the greatest comparative anatomist of Germany. During this period he published nothing; it was one of strictly intellectual acquirement. On his return to his original university he became a *privat-docens,* was elected member of the Leopold-Caroline Academy, and acted as its secretary until its removal to Breslau in 1830.

In 1826 Müller first attracted the general attention of the learned world by a treatise on "The Comparative Physiology of the Vision of Men and Animals, together with Experiments on the Motions of the Eyes, and on the Human Sight." In close connexion with this followed his work on "Phantastic Visions." Both of these productions showed that their author had not yet passed the "philosophy-of-nature" phase of his educational career. He had, in fact, fallen under the influence of Goethe, and was especially charmed by the poet's "Theory of Colours." The future founder of the experimental physiological movement in Germany now speaks slightingly of experiment, and of Magendie, the French physiologist. Yet the 'Comparative Physiology of Vision' contains a vast number of well-observed and important facts; and Müller's discoveries on the subject of the sight of insects alone make the above work valuable and extraordinary. What especially attracted him, however, to Goethe's 'Theory of Colours,' was the proceeding from subjective appearances. The poet had been the first to instate these in their rights as physiological phenomena. Müller maintained as the leading truth of the physiology of the senses, the doctrine of the specific energies of sensitive substances, which follows from these three facts: 1. That one and the same organs of sensation, in whatever manner excited, always respond in the same manner; 2. That the most different organs of sensation, excited in the same manner, answer each in its own special manner; 3. That each organ of sensation has the power, from specific endowment, of developing, as a fantastic sensible appearance, its own kind of sensation. And this doctrine, which, on the ground of experience, answers to Fichte's subjective idealism, on the ground of speculation, led Müller (verifying his own thesis, "Psychologus nemo nisi Physiologus"), by the path of physiological investigation, into the very heart of the deepest psychological questions.*

* Müller's observations of the sense of vision were conducted unceasingly and injuriously on his own eyes; and he was wont to note any phenomena which occurred as he lay in the darkness, until they and himself were alike lost in dreams. He was accustomed to

Meanwhile, Müller was teaching and lecturing at Bonn with extraordinary energy. From 1825 to 1833 he generally took four different subjects in every academical prospectus, Medicine, Special and Comparative Physiology, Comparative Anatomy, and Latin Disputations. By degrees his lectures extended not only to all the branches of anatomical and physiological science, but far beyond his peculiar province, to general pathology. His success as a lecturer was immense; for, if not by the charm of his style, at least by the manifestation of scientific ardour, he continuously sustained the interest of his audience. His anatomical demonstrations, in great part prepared at his own expense, and the tact and dignity of his manner, attracted to the University a number of students such as Bonn had never before been gifted with. In 1826 the government, anticipating the strictly legal period, made him professor. But no remuneration could be attached to this premature appointment, and Müller was on this account necessitated to essay the responsibilities of private practice. They weighed upon him, however, too heavily, and were essentially uncongenial to his tastes and habits. The death of a friend to whom he was acting in the capacity of physician, proved the climax of the physiologist's career as a medical practitioner. This at least is his own account of the matter; to which it is right to add that there were eighteen physicians engaged in practice at Bonn at this very period, a circumstance which would render the speedy success of a young man extremely problematical. In 1827 Müller married Anna Zeiller, to whom he had already dedicated a poem, promising her, with himself, the dowry of an immortal name. After this event he was obliged to intermit his labours, in consequence of the sufferings which he had brought on by his continual experiments on his own organs of sensation. But the comforts of the married state were greater than its cares. In a little tour which he made with his wife in a "one-horse-shay," he dissipated his malady, and returned home with renewed health and strength. His restoration left him, in more senses than one, a different man. What may be termed the subjective-philosophical period of his life ended, and was succeeded by the objective-physiological period. He now evinced great horror of all transcendentalism, self-contemplation, and indulgence of the fancy. He eschewed speculation, became reserved, dogmatic in assertion, and directed his powers of observation to the manifold objective facts of nature. This is the true beginning of the Johannes Müller who is so celebrated. The hidden glow which dwelt in that nature still shone in his wonderful eyes, and commanded the attention of all who looked upon him. But he was calm, unenthusiastic, subdued, having his volition under complete control. In the next five years, until his settle-

fast, and subject himself to other hurtful physiological experiences, in order to familiarize himself with the phenomena resulting therefrom, in the shape of visions, apparitions, and the curiosities of second-sight. He had by no means absolute power, however, over the pictures arising in the field of vision, and was only once successful in seeing for a moment a bright red. Goethe, on the contrary, could see at will a flower, or the coloured rose window of Gothic architecture; but such an appearance once noticed refused to be fixed, changing like the protean forms of the kaleidoscope. Such was the difference between the creative power of the poet and the investigating power of the physiologist.

ment at Berlin, Müller produced an immense number of treatises on human, comparative, and microscopic anatomy; on zoology, the history of development, and experimental physiology. At first morphology receives most of his attention, and he devotes himself to a favourite subject, the construction of the eyes in invertebrate animals; then to the metamorphoses in the nervous system of animals, and herein to the question of the morphological importance of the gastric cord, deciding that it has its analogue in the spinal cord of vertebrata; an observation afterwards confirmed by Newport's discovery that it was composed of an upper and lower section, the former without, the latter with ganglions.

Müller's most important work, on the 'History of Development,' was published in 1830; and here, for the first time since 1824, he speaks of the general principles which guided him in investigation.* The essential thing, he says, is accurate observation and experiment—experiments yielding always the same result—experiments in which the accidental or empirical is clearly separated from the essential and universal. "If all our experiments (he writes) consisted of such observations, all future theories would be unnecessary: theory would be a plain relation of facts, of which one is the sequence of the other." This is the perfection of inductive science, and reveals to us the earnest disciple of Lord Bacon. In 1830 also appeared our author's celebrated treatise, 'De Glandularum secernentium Structurâ penitiori, earumque primâ Formatione.' Before this time little was known concerning the glands. Müller showed by a series of experiments (wonderful, indeed, if we consider his narrow means and imperfect instruments), that all known glands with outlets are essentially blind surface-involutions; that on the walls of these cæcal involutions is spread out a network of fine vessels; and the manifold internal arrangements of these structures are first of all to be viewed as so many means of effecting the multiplication of secreting surface in a given space, without deriving therefrom the difference of the glandular secretion itself. These verities have received abundant confirmation and development from the researches of Mr. Bowman, Dr. George Johnson, and others. In 1831 Müller confirmed the views of Sir Charles Bell by various experiments, with respect to the *nervus trigeminus*. At the same time he detected in frogs the existence of four bags which pulsate independently of the heart and respiration; they serve in amphibia for the circulation of the lymph, and were denominated "lymph-hearts" by their discoverer.† Panizza's dissections yielded him the same fruits just afterwards, but quite independently of Müller. In order to judge of the latter's labours upon the blood, the then state of knowledge about that fluid must be considered. Hewson, indeed, in England, had described the properties of the blood, and had perfectly understood the process and phenomena of coagulation, so far back as 1760. But his knowledge was so in advance of his period that Ma-

* This treatise contains the author's observations on the formation of the genital organs, another of his favourite subjects. His experiments on the structural tissue of the corpora cavernosa are very curious. It also comprises important discoveries of the primitive kidneys in the amphibia, and of the functions of the "Wolffian bodies."

† Poggendorf's Annal., 1832. See also Müller's Archiv, p. 535. 1835.

gendie in 1817 called this doctrine a chimera; and so gave indirect encouragement to the propagation of false theories by eminent physiologists down to Müller's own immediate day. But Müller's merit is that he discovered Hewson's doctrine, without knowing of its previous existence, supplied it with new support, and imparted to it its first influence on science.*

Other eventful circumstances made the year 1831 memorable to the German physiologist. He met Humboldt and Cuvier in Paris. Müller's pecuniary position in Bonn had somewhat improved, but not sufficiently to justify his refusal of a professorship in Freiburg, offered him in 1832, had he not felt bound by the ties of gratitude to remain under the Prussian Government. Doubtless, also, he was looking to the time when the death of the aged Rudolphi should vacate the chair at Berlin, which was the first in Germany. This event occurred in November, 1832, and Müller's claims to the succession were placed before Von Altenstein, and acknowledged by that minister in an appointment which gave universal satisfaction. The following year, therefore, he removes to Berlin, where many things contribute to make his position a very distinguished one. Cuvier and Meckel had recently died. The 'Archives of Anatomy and Physiology,' which the latter had edited, fell into Müller's hands; and as several other periodicals dropped at this time, he had an ample field for literary achievements. Nor did he fail to take advantage of it. He added the words "for Scientific Medicine" to the title of the 'Archives,' and seized the opportunity to lead the medical world back to physiology as the source of medical knowledge. Science was at this time attracting universal attention in Berlin. Humboldt had settled there after his Siberian journey; and distinguished men in every branch of knowledge gave it a celebrity such as Paris had previously enjoyed. Schlemm, a brother professor, worthily seconded Müller, and became his intimate friend. We find the latter lecturing in winter nine, and afterwards six hours a day, conducting the dissections, and holding the State examinations. In summer he taught physiology six, comparative anatomy four, and pathology three hours daily. His evenings were taken up by examinations. And yet, with this intense work, he published, between his call to Berlin and his death, nine independent works (among which are his greatest); and the medical journals and reports were filled with essays, &c., by him during this entire period. Moreover, the 'Sitzungsberichte' of the Vienna, and the 'Comptes Rendus' of the Paris Academies, as well as many other archives and dictionaries, were enriched by articles from his prolific pen.

The first part of Müller's 'Manual of Physiology' appeared in 1833, and its completion in 1840. This is the work which bears the fullest impress of his ripe manhood—the "objective" period of his life, as the 'Comparative Physiology of Vision' does of the "subjective" phasis of his youth. It is also the work by which he exercised the largest influence on his times, and with which all of us are most familiar. It

* See the Works of William Hewson, edited, with an Introduction and Notes, by George Gulliver. Printed for the Sydenham Society. London, 1846.

was translated into French by Jourdan;* it has been ably and faithfully rendered into our own vernacular by an accomplished physician who graduated at Berlin, and whose recent appointment as Physician to the Queen is an honour to himself and a subject of congratulation to the entire profession.† If this great work is in some sense superseded now, it still holds its ground as the greatest physiological record of the age; great because of its own intrinsic merits, and by reason of the immense impulse which it gave to physiological study in this and every country.‡ M. Du Bois-Reymond proudly says of it that it has stamped physiology as, *κατ' ἐξοχήν*, a German science, notwithstanding that the two greatest facts of physiology—the circulation of the blood, and the different functions of the anterior and posterior roots of the spinal nerves—are of English origin. It is not to be denied that the 'Manual of Physiology' abounds in faults both of arrangement and style. Müller himself, indeed, was fully conscious of them, and called the work, in jest, his "lumber-room of physiology." Some of these faults are inseparable from the nature of the subject, for physiology lacks the exactness of mathematical science, and cannot, like it, proceed from self-evident and already demonstrated axioms. The progress of the 'Manual' itself, too, spread over such a lengthened period, in which scientific knowledge was correcting her antecedents, that contradictions were inevitable. For these reasons the work is deficient in completeness and co-ordination, and rather resembles a loose collection of treatises than a well-arranged guide-book. In these respects it contrasts unfavourably with the 'Elementa'§ of Haller, which is a model of elegance and classical beauty, though the composition and publication of the latter had a limit of not less than nine years. To what, then, does Müller's work owe its great eminence? First, to the extraordinary extent and relative completeness of the author's investigations, and to his skill as a comparative anatomist. Great improvements had at this time taken place in the microscope, and, assisted by Schwann, Müller was able to conduct the most interesting experiments on the reproduction of nerves, on digestion, on the movements of the eyelids in fish, and in various other subjects. These, together with the experiments on animal and vegetable tissues, including cartilage and bone, the spinal system of cartilaginous fishes, the researches of Eulenberg on the elastic tissue, those of Jordan on the (then so-called) contractile cellular tissue of the skin, those of Miescher on the reunion of bone—were all incorporated into the 'Manual.' He did much likewise experimentally to extend and confirm the views of Sir Charles Bell, with respect to the anterior and

* Jean Muller: Manuel de Physiologie. Traduit de l'Allemand sur la quatrième édition (1844), avec des Annotations par A. J. L. Jourdan, &c. Two vols. 8vo. Paris, 1845.

† Elements of Physiology, by J. Müller, M.D., &c. Translated from the German, with Notes, by William Baly, M.D., &c. Second edition. Two vols. London, 1840. With a Supplement in 1848. (Alas! while we are correcting these sheets, we read of Dr. Baly's horrible mutilation and death.)

‡ The writer of this article was a pupil of the late Dr. Todd, and knows personally the estimate which he formed of Müller, and the spirit of emulation inspired by the German physiologist in the breast of his distinguished English cotemporary. Dr. Todd's coadjutor, Mr. Bowman, we believe, fully participates in these sentiments.—Ed.

§ Elementa Physiologiæ Corporis Humani. Eight vols. 4to. 1757–66.

posterior roots of the spinal nerves. A favourite subject with him was the voice, and his treatment of it in the 'Physiology' attracted much attention. In 1839 he published, besides, a separate treatise on the human voice, with remarks on the voice of mammalia, birds, and amphibia. Next followed his experiments on hearing. And for these he was equally well formed by nature, as for those on vision. It is related that his own auditory sense was wonderfully acute, and he had extraordinary power over the external and internal muscles of the ear. The physiology of this sense is in this day much in the same state as Müller left it; but its pathology has been abundantly enriched by modern researches. Still more was the success of the 'Manual' owing to the time at which it (the first part of it, at least) appeared. The ill success of the experiments which followed Galvani's discovery had produced great discouragement among experimentalists. Cuvier had then directed universal attention to the laws of form in animated nature. But no startling theory on "the origin of species" had arisen, and morphology did not easily unite with theoretic experimentalism without the blending wand of Darwin. Thus comparative anatomy was advanced, and theoretic science was defeated, by organic nature. In Germany, too, had been initiated the reign of the false natural philosophy (favoured by the reaction against the Hellenism of Goethe), to which the morphological school in part fell a prey.

Thus the first portion of the present century is a blank leaf in the history of physiology. Even the greatest discovery since Harvey, the differential spinal roots, was put forth theoretically, and years elapsed before Bell himself, Magendie, Béclard, and Müller gave the clearest demonstration of its truth.* But the revolution was gradually being prepared; on all sides more exact investigation was being demanded. Magendie in France, Purkinje in Germany, led the way back to physiological experiment; and now Müller, his earlier opinions being quite changed, seconded their efforts, and stood forth as the great champion of induction in the investigation of nature. Nor was the success of Müller's most comprehensive literary production otherwise than increased by his known character for fearless research and striking accuracy. He is never ashamed to confess ignorance; he despises no mode of inquiry; he has preference for no particular method. Morphology in the widest sense, physics and chemistry, subjective experiences, pathology—all are called to his aid, and lend their contri-

* There has never, we believe, been any question, as to the priority of *conception* or discovery of that physiological truth here alluded to. But the priority of its experimental confirmation has given rise to much dispute. The subject was ably discussed in the British and Foreign Quarterly Review for Jan. 1840, and both sides of the question fairly and impartially stated. M. Dubois-Reymond, in the Éloge before us gives a precedence to Müller—a precedence which is not warranted by recorded facts. M. Béclard (Meckel's Archiv für Anat. und Physiol., 1827) had spoken out with considerable decision before Müller's experiments had lent their undoubted weight to the discovery: "Les expériences de M. Ch. Bell, celles de M. Magendie, et les miennes propres, ont clairement démontré que la racine postérieure des nerfs spinaux est sensoriale, et la racine antérieure motrice." A reference to Sir Charles Bell's first essay shows that he experimentally confirmed (though but partially and approximatively), his own theoretical discovery, before Magendie or any one else had placed his experiments on record. (See Baly's translation of Müller, second edition, vol. i. p. 690.)

butions to the universality of his physiologic record. Thus his book, with which Dr. Baly has familiarized us (and not the least valuable part of which are Dr. Baly's own notes), constituted, so to speak, an epoch; and Müller claims to stand at the very head of the experimental school, because, like most reformers whose conversion from early errors makes their zeal against those errors so intense, he laboured with passionate ardour, and still retained a measure of primitive alloy. For instance, he always remained a "vitalist," and never lost sight of that idea which dignified its disciples with its name, in all his researches into the phenomena of organic nature. That is, he believed in the existence of a simple vital energy quite different from the physical and chemical endowments of organisms, having no fixed seat, divisible into innumerable portions, yet without diminution, vanishing in death, or the appearance of death—"latent," but not destroyed. It may be said, indeed, by some, that the great physiologist made the refutation of this theory easy by the clearness with which he stated it and essayed to define it. The refutation, however, did not satisfy one who had reached an age when conclusions are not easily given up, and who might be excused for still fostering a theory whose entertainment did not militate against experimental exactness, or dwarf the development and fruition of scientific labour. In considering Müller's character as a conductor of experiments (a character whose fearlessness gave it so much weight with his contemporaries), it is hardly possible to do him justice, unless we first divest the period in which we live of the manifold aids by which experimental inquiry is now sustained. But a few years, of a truth, have elapsed since the active experimental period of Müller's life. In those years, however, how enormous have been the strides of the mechanical arts which elucidate science! Nor must we overlook the structure of his own mind. He had not the faculty of mathematical conception and division of problems. He was unacquainted with what we call the æsthetics of experiment. His method, though great, was rough in accessories. He sprang at once to his object, heedless of the little distractions and incompletenesses of the process which rendered his acquisition of that object imperfect.

During the years when Müller was publishing his 'Physiology,' he was likewise engaged in many other works. In 1834 he undertook to supply a yearly report of the progress of anatomical and physiological science, which was by no means a mere chronicle of results, but contained his judgments thereon. He conducted investigations on the structure of tumours, demonstrated their development from cells, and the nearly identical composition of normal and abnormal tissues. About this time, also, he entered into most friendly relations with the Scandinavian anatomists, Eschricht and Retzius, a circumstance which in some measure compensated him for the attacks which he met with in Germany from the jealousy of less successful rivals. This friendship lasted till his death.

The year 1840 is an era in Müller's life, for he then completed his 'Physiology;' and henceforth he was much less occupied with purely physiological subjects. He now stood forth as a pure morphologist, and, indeed, the first of his time. This change in his career was marked by the production of extensive systematic zoological treatises. He wished to find a system which should unite the practical advantages of the artificial with the theoretic advantages of the natural. The joint system of mammalia of Aristotle and Linnæus approached the nearest to his ideal of what a system really should be. He sought for some known or unknown characters that might at once determine, by their relation to the entire organization, the natural relationship of all the creatures in which these characters should betray themselves.

"Comparative anatomy," says Müller, "leads in its complete form to such necessary consequences, that expressions may be found for organizations which are like the expressions of an equation. If these expressions are found, the unknown quantities may in a given case, as in an equation, be reckoned from the known."

The examination of various museums required for Müller's 'Systematic Description of the Plagiostoma' led him, in company with Henle, to Holland and England, in 1837, where he was much fêted. His celebrated discussion on the smooth shark or dog-fish of Aristotle is well known. Aristotle had declared that among the shark genus there were both fish (ovipara) that deposited eggs (spawn) and (vivipara) that bore live young; and among the latter there were some that had the fœtus united with the uterus by a placenta, as with mammalia. Although the Danish anatomist, Stenonis, had made a similar observation on the coast of Tuscany in 1673, yet no one believed in the γαλεὸς λεῖος of Aristotle.* Müller had already observed some years before in the *Carcharias* a fœtal attachment to the uterine walls by a yolk-bag placenta (Dottersack placenta). After much investigation Herr Peters, at last, in 1840, sent from Nice a number of *Mustelus* ova, in which was observed a union of the yolk-bag placenta with the uterus, similar to that obtaining in the *Carcharias*. From this it was evident that there were two kinds of *Mustelus* in the Mediterranean, easily to be confounded with each other, one of which belongs to the live-producing sharks (*Vivipara akotyledona*) *without* placental attachment, the other *with* attachment. The reason of the obscurity in which the subject had hitherto been wrapped was now obvious. One set of observers had found the *Mustelus vulgaris*: Stenonis, and probably Aristotle, the *Mustelus lævis*, or *Pesce palombo*. Müller was now able to thoroughly elucidate the generation of sharks and ray-fish; and, having from the very first regarded Aristotle with peculiar veneration, he was proud of erecting a monument to the accuracy of this early observer.† His next performance was to clear

* "But in those sharks which are smooth and called 'λεῖος,' the ova lie as in the Scyllium between the oviducts. When they separate from the ovary they enter each of the two oviducts, and the embryos are there developed, an umbilical cord connecting them with the walls of the uterine cavity, so that after the ovum (the yolk) is consumed, the embryo appears to have the same relation to the parent as in quadrupeds. A long umbilical cord is connected to the lower part of the uterus by means of a body like a placenta, whilst at its other extremity it is attached to the embryo about the middle, where the liver lies." —Aristotle: Hist. Animal., vol. vi. p. 10.

† Monatsbericht der Akad. der Wissensch. zu Berlin, Aug. 6th, 1840.

up the obscurity which surrounded the *Ganoides;* and his investigations established their proper place among the orders of fossil fishes. Indeed, he now took his position among the first ichthyologists, and began, with Herr Troschel, the 'Horæ Ichthyologicæ,' of which work, however, only three parts appeared between 1845 and 1849. Ornithology in like manner received much of Müller's attention, especially the system of the passerine birds. This was the last great contribution which he made to the knowledge of living vertebrata. He next entered upon palæontology. At this time the 'Hydrarchus,' a gigantic sea-serpent, was being exhibited in Germany. Of the creature's true form the learned were not agreed. By some it had wrongly been placed among the Saurians. Müller, after immense labour, succeeded in recovering the skull, gradually put together the whole skeleton, and described the *Zeuglodon cetoides.* It is to be regretted that he has left no sketch of this "monstrum horrendum, ingens," and the more so as it is known he contemplated a monograph upon the subject.*

Our author continued to keep up his interest in Palæontology; but his subsequent studies were chiefly directed to invertebrate creatures. The description of the *Pentakrinus caput Medusæ;* inquiries into the system of the *Asteriadæ;* the discovery of the *Pluteus paradoxus* (so named by him because of its resemblance to an easel with drapery thrown over it); these and numerous other microscopic observations fully occupied him. We find him at Heligoland, at Ostend, at Marseilles, at Nice, between 1846 and 1849, and always for scientific purposes. The result was that he gave a complete exposition of the *Echinodermata,* and their transformations. On the subject of the change of generation supposed to be involved in these processes, Müller expressed himself with great caution. In order to do full justice to his investigations, it must be remembered that he lived far from the coast, and that the larvæ of the echinodermata are only to be had in certain states of water. The minute structure of this Cycloneurose sub-kingdom was esteemed by the great physiologist himself to be the most difficult part of comparative anatomy; but he succeeded in expounding the whole subject, from fossil remains and living specimens. In the midst of his studies therein occurs the scientific episode of such great interest—the production of *conchæ* in *Holothuriæ.* Müller, in fact, discovered that the *Synapta digitata* gives birth to a *concha,* which he called *Entoconcha mirabilis.* This is much the same as if he had discovered that a mouse had brought forth a butterfly; and of course this revolutionary fact threw the entire world of naturalists into a state of the greatest excitement. But the excitement was by no means joyful; it was the rather painful, disquieting, and humiliating. Müller says be felt as if he could no longer believe the evidence of his own senses. He could not understand this "witchcraft." He felt the ground on which he had been standing all his life shake under

* See Transactions of the Geological Society of London, second series, vol. vi. p. 70, foot-note; also Burmeister's Eigene Untersuchung des Zeuglodon steht unter dem Titel: "Die Literatur über Hydrarchos," in der Halle'schen Allgemeinen Literatur-Zeitung. Juin, 1847, No, 121 ff.

his very feet; and saw no way out of the many difficulties which the new revelation involved. And so, after numerous attempts to arrive at a satisfactory conclusion, he decided that these conchæ must be parasitic. In no other way could he save the principles of zoology. But this compromise could not satisfy him. It seems as if he now became afraid of the subject; and though he went again to Trieste (where he had made the discovery) in 1852, it was only to investigate generally the Echinodermata. He did not answer any of the questions which his discovery respecting the *Synapta* occasioned. It is probable that he purposely avoided this; for he did not feel himself young enough to face the overthrow of all his old principles, a circumstance which must have resulted, had he been compelled to adopt any other explanation of the production of the conchæ than the parasitic theory. In 1854, he closed his investigations on the development of Echinodermata, and they brought him abundant distinction. He received the Copley medal of the Royal Society, and the Cuvier prize of the University of Paris.

It is impossible to give anything approaching to a consecutive detail of Müller's scientific labours and literary productiveness. It is equally impossible to narrate any events concerning him which are not for the more part connected with the conception and execution of his great works. There are a few incidents, however, which we will place before our readers ere we pass our opinion upon his scientific researches in their full comprehensiveness and entirety. Müller's frequent journeys were, with few exceptions, for purely scientific purposes. In 1841 he was invited to succeed Döllinger at Munich, but refused. Three times he was dean of the University of Berlin, and twice rector—the last time in the eventful year 1848. He was conservative in his views, though by no means a politician. He only expected art and science to flourish in peaceful times, and therefore looked with suspicion on changes and the growth of political privileges among the people. A street row, for instance, he regarded much as he would have done an *émeute* in his own library or museum. Such things were not to be tolerated. As a child towards its parent, he felt towards the Government of his country. In the revolution of 1848, Müller had with his own hands to give defensive weapons to the university youth, to moderate their excitement, and to lead them. His eloquence was not of the peculiar kind adapted to such a work, and in the discussions incidental to that dangerous period, he therefore played but a very subordinate part. Some wished, after the example of another German capital, to make the university building the centre of party conflicts. Müller, in his alarm, armed with a sword, kept watch day and night before the door, and by his firm behaviour as a citizen thrust back many who cared nothing for him in his capacity of rector. All agree that he showed great contempt of danger, and behaved with honour and a most exalted sense of duty. This state of things lasted seven months, during which Müller still managed to work and study. The sword did not altogether supersede the scalpel. At the end of his rectorate his health was obviously beginning to suffer, and he was compelled to obtain leave of absence from his professional duties for the winter. So eager was he to be gone that on the very day when his rectorate ceased he left Berlin for the Rhine, afterwards visiting Ostend and Marseilles, where he restored himself, physically and mentally, by inhaling the breezes and contemplating the wonders of the sea. In general he had only had his holidays for sea-studies; but he had managed somehow in eight different journeys to visit all the coast of the North Sea and Baltic; in eleven journeys, likewise, that of the Adriatic and Mediterranean, from Trieste to Messina and Cette. Twice in these wanderings his life was in extreme danger; once, in 1853, when crossing the St. Gothard he was thrown down a precipice by the upsetting of the carriage; once, in 1855, when the steamer in which he had left Christiansand was run into by another vessel, and sank in ten minutes, whereby half of the ninety persons on board perished. Müller managed to float until he was picked up by one of the boats. The horrors of the scene made so deep an impression upon him, that it was not till long afterwards that he could dispel the tragic picture from his mind.

If we consider the great physiologist's performances as a whole we are at once struck with their prolific character. The number of his independent works amounts to twenty; of his lesser treatises and essays to two hundred and fifty. In fact, for something like thirty-seven years he published, one year with another, every seven weeks, a scientific dissertation of from three to five printed sheets, with from one to three plates drawn by himself. And even if he does not surpass all naturalists in the quantity, certainly he does in the extent and "many-sidedness" of his achievements. Haller alone can be compared with him in this respect, and in Haller's time the domain of organic natural science was much less extensive than in Müller's. If it was difficult to detect the thread of unity throughout the latter's works, their universality is, at least, admirable. This universality did not arise from the vanity of showing to the world his unrivalled capacity for multifarious investigation, but rather from the burning impulse of his mind, from his craving for knowledge, from the towering ardour which made him, like a falcon, look down from an eminence on the wide-spread country, and yet lose not the power of centralizing the keenest gaze upon any particular point. He was (as Professor Huxley said of him) not only the Columbus but the Cortez of this new world, who took possession of all its treasures. Or he was like a great conqueror who cannot rest while there remain peoples and kingdoms unsubdued. He appropriated at once, and exacted allegiance from, every hidden territory of science, but so appropriated it as to change it and stamp it with his own characteristic die. More than any naturalist since Haller, Müller understood exactly what had been done up to his own time, and what remained to be done to give completion to but half-discovered truths. His insight, gained by personal experience, into nature, and into the best means of investigating her phenomena in particular fields of science, gave him a most reliable judgment, and therefore made his opinion most valuable. He did not, indeed, keep this commanding position to the end. But who shall blame him if he retreated before the vast flood which himself had conjured up, to ground on which he felt more sure? In spite of the extent and variety of Müller's labours, it cannot be said that he was weak anywhere as an investigator. Accurate knowledge and correct reasoning were the substratum of all his work. The number of positive facts which he brought to light in different territories surpasses representation; and very seldom has any error in fact, or even imperfect observation, been brought to his charge, although (as we have previously intimated) his experiments were frequently at first rough and deficient in symmetric proportions. He knew the great value of objective truth, and therefore had but little faith. If he doubted till he had seen, he acted on this principle equally with respect to his own experiences and those of others. It was his rule to investigate with his own good eyes three times: after the first discovery, he investigated for the second time when he was committing his observations to paper; and for the third time, while his MSS. were in the hands of the printer, to whom Müller's characters and corrections were a complicated affliction. It has been seen that Müller had great capacity for continuous labour, great skill and industry, great method and judgment. It should be added, that he was gifted with a remarkable memory. This faculty, indeed, was so striking (as we believe it is with nearly all great men) that one wonders less at the much he did, in reflecting upon the little time which he could really call his own, amid his incessant lectures.* It is a wonderful curtailment of labour to once observe a fact, and then have it always at command—to know a thing and then remember it for ever. There is only one circumstance which lessens our conception of Müller's productiveness, and that circumstance explains (what we have before indicated) the insular and discursive character of his literary works. He was singularly indifferent to the formal completion of what he had conceived and in a great measure executed. Though interested in general literature and art, and himself a skilful draughtsman of anatomical subjects (having, too, been once subject to Goethe's influence), Müller was not an *artiste* in the highest sense of that expressive word; for he cared only for the essential, and having once secured this he did not trouble himself about the proportional execution and completion of subordinate parts. By this he saved much time, but unquestionably lost some influence. Yet he could certainly write in excellent style, as he showed, for instance, in his 'Report of the Progress of Pathological Anatomy in 1855,' and in passages of his 'Physiology.' But he was often careless in this respect. His great anxiety was to use such expressions as could not be misunderstood, and for this reason he would avoid the use of pronouns, repeating the substantive. In describing forms he not seldom resorted to striking comparisons, partly showing the richness of his imagination, partly his observation of every-day objects around him. Had he lived among a

* Orfila's memory was equally remarkable, as shown in his wonderful accuracy with respect to chemical equivalents, which he could detail one after another without reference to the tables.

people more under the influence of æsthetic impressions, he would, like Cuvier, have become a master of the scientific style. As it is, his introductions for the more part read well, but as soon as he gets into his subject technicalities multiply, foreign words and idioms thicken, and it is obvious that the author's business is to impart as quickly as possible his results with little reference to symmetry or attractiveness.

It has been remarked that, with all his labours, Müller made, properly speaking, no physiological discovery of the first rank; none of those observations which, at once new and of vast import, bear for ever the name of their discoverer. The detection of the lymph-hearts, the branch arteries of the corpora cavernosa of the penis, and the chondrin, are not of this kind. The reflex motions, the functions of the anterior and posterior spinal roots, the constitution of the blood, are not purely his. He did but demonstrate perfectly what had before been demonstrated but imperfectly, and stamp the uncertainties thereof with the positive impress of his experimental genius. Even the development of the echinodermata is more an expansion of the doctrine of change of generation and metamorphose, than a new principle. All this is so far true; but the circumstance detracts but little from our author's great and unquestioned fame. It may be allowed that he generally rather executed what others had imperfectly conceived, than gave birth to fruitful thoughts and theories of his own. Müller's praise is, that he turned to the best account matter which was ready to his hand, utilized what others had discovered, got results therefrom in the shortest time, and then passed on with spirit and unwearied application to some other field of observation. Discoveries of the highest rank may fall to the lot of a comparatively insignificant inquirer. That Müller had not this good fortune is no more his fault than it is the fault of a merchant who has become rich by industry and enterprise, that he has not won the great lottery-prize. But discoveries of the first rank are also made by unremitting search in one direction, and so multiplying the possibilities that some great "find" may offer itself either in the sphere of observation or of thought. That Müller, being what he was, should have missed such a "find" furnishes additional evidence of the truth that no man can be "as deep as he is broad, and as broad as he is deep." If at the time when his productive power was at its maximum, he had limited himself, as he did afterwards, to one field, and (according to Schiller's advice) more concentrated his energy, he would not indeed have coined such masses of gold, but he would have discovered more brilliant jewels. It is not, for instance, to be imagined that, if he had persevered a little longer, he would not have understood the minute structure of the kidneys, of which he had already found the simplest type in the *Myxinoides*, with which Mr. Bowman, who was afterwards successful, was unacquainted.

In thus missing a discovery of the highest mark, Müller presents another striking resemblance to Haller, with whose comprehensive learning, reforming influence, and commanding position, in the middle of the last century, the German physiologist and his surroundings are to be compared. Without being suspected of what Lord Macaulay terms the "furor biographicus," we may affirm that Müller's fame will surpass Haller's in time to come: not because he in a period of greater knowledge was comparatively as learned and universal as Haller in his, but because of his superior judgment. Most of the views advanced and defended by Haller have long since been relinquished: they have been swamped in the flood of sounder science. Not so with Müller. He has generally been right; numbers of his conjectures have since been verified, and the objective portion of his labours has scarcely ever been disputed for its accuracy. For he had learned to think with nature's mind, and to observe with nature's eye. It is more than probable that many triumphs yet await his name in connexion with the subject with which he last busied himself. Such is he by the side of Haller. Compared with the French naturalist Cuvier, we must put into the scale of Müller his variety and richness—into that of Cuvier his simple greatness.

We must not omit to allude to Müller in his special capacity of teacher, in which he exercised so large an influence. He had no natural eloquence, and his inaugural efforts in the professorial chair were by no means of the most promising. Yet his lectures were soon reckoned among the best of the University. His manner was cold, but it was evident that he had a thorough knowledge of his subject, and the coldness did not serve to conceal an undercurrent of real earnestness. His sentences were simple, perspicuous, and unadorned, and so correct, that they might have been printed with little or no alteration. In this respect they contrasted favourably with many of his writings, to the involved and unsymmetrical character of which we have before alluded. He had great control over his class, and his piercing eyes, which ranged over the hearers, would fix themselves upon a stranger, even to his embarrassment. Like the accomplished Professor of Anatomy to our Royal Academy, Müller was a great master of drawing on the board. It was no little pleasure to see him produce some animal form step by step, till it was before you in its full perfection. By this rare gift he more than supplied the want of those designs and illustrations which in France and England are usually hung round the walls of the lecture-room. He had always a narrower circle of pupils who gathered more immediately round him than the larger circle who heard his public lectures. And to that narrower circle, whose members manifested a special interest in scientific study, the Professor's manner was even affectionate and paternal, while to those who were idle and indifferent, his bearing was almost unfriendly and repelling. To feel Müller's eye upon one was the greatest stimulus to exertion. It was a magic influence which carried away all but the most worthless. Delightful were the moments when he relaxed in general conversation and cheerful jest. It is curious that one who bore so great a name and instructed so many students, never sought to fetter them with scholastic individuality. He did not set himself up to be the founder of a school, nor would he resort to the favourite word "disciple." And yet he has been the founder of many schools of investigators of organic nature, who seek to elucidate her wonders with his ardour and spirit. Müller may be said to have created the now fine anatomical museum at Berlin, for he found in it 7000, and left in it upwards of 19,500 preparations. Here he spent much of his time, and laboured abundantly. His zeal accumulated objects for every section, and his arrangement of those objects was perfect.

Like Jacobi, the great German physiologist conveyed to every one the impression that he could have been equally successful in any other field of human activity. The traits of character which gave this universal indication were certainly such as to justify the speculation; for to the wonderful memory before alluded to he had great knowledge of men and manners, great foresight and resolution, a strong sense of order, which, in respect of time, yielded the happy development of punctuality. Müller always succeeded where he *willed:* he would never *will* where he thought he could not succeed.* What struck observers most with regard to him was his great reserve. Except by surprising him at particular moments, one could only see the calm expression of an energy which was tinctured with a sort of melancholy, as if he were conscious, like the son of Peleus, of the little time which he had for achievement. Strange contradictions were united in his inner man. Hard and inconsiderate as he sometimes appeared, he was at others capable of great sensibility. Müller was a tender husband and most indulgent father. In general conversation he was not productive, for he was too much occupied with his own thoughts and with the work which was before him. But in the circle of his family, or of intimate acquaintances, or in his holiday excursions, when he had been unusually successful with the gauze net or the microscope, he would be the most charming companion, and exhibit occasionally quite childlike joyousness.

Müller spent his considerable income with great liberality in the promotion of science and of every noble undertaking. He grudged nothing for his journeys (for they were scientific), books, and the publishing of his works. Such a library as he left behind him has seldom been collected by a private individual. Besides what he himself purchased, it contained a number of valuable presents received by him from all parts of the world. In intercourse with his brother physiologists, Müller has been accused (in his earlier years at least) of an overweening ambition, which made it difficult for him to suffer another's merits side by side with his own. If this be at all true, it should be remembered that such boundless labours as he imposed upon himself could only be carried out by an equally boundless passion, such as science alone, without reference to "le moi," is incapable of inspiring. In his later years, indeed, he gained the mastery over himself in this respect, and from moral considerations. It would then have been truer to say of him, as of Goethe in his age, that he rather overrated the merit of youth. "Envy," he remarked to M. Du Bois-Reymond a few days before his death, "is with me changed into admiration. But that

* Müller presents in this respect a striking similarity of character to Orfila, of whom it is said: "Il a su mener à bonne fin des entreprises difficiles, il n'en a jamais tenté d'irréalisables."

is a height of feeling at which man only arrives gradually." Müller was of middle stature—in youth graceful and thin, though in later years he assumed more aldermanic proportions. His great and noble head was generally hung reflectingly on one side, but in the professorial chair, or in animated conversation, it was held proudly erect. His manners were simple, his temperance extreme. He required no refreshment from his works, for he was never exhausted. To the influence of weather he was perfectly insensible, except when a leaden-grey sky deprived him of light for observation and drawing. He was never ill (he was too busy for that), save just prior to his marriage and at the end of life, and never took cold. He would run and skate in middle life, and appeared, indeed, to have been gifted with eternal youth.

It had formerly been a boast of Müller's, that he could sleep at any moment he wished; but sleeplessness had latterly troubled him for some time. Jaundice and hepatic pains drove him to the use of large doses of opium. Palpitations of the heart (which he fancied was diseased) also now distressed him. Towards the end of the winter of 1856–7, his health received the first manifest shock, and he was compelled by a slow fever of a gastric character to suspend his lectures. He was very anxious about himself, arranged his worldly affairs, and forbade the opening of his body after death. The disease now altered its character, and the sufferer's ankles became subject to an arthritic deposit. Towards the summer, however, he was completely restored; but the symptoms again returned towards the autumn and winter. The large doses of that deceitful medicine which was so hurtful to Haller, had probably been equally injurious in their action upon the modern physiologist's economy, for his digestion became impaired, and he experienced frequent attacks of dizziness. Yet, in spite of these things, the darling microscope was not pushed aside. Matters now became worse, and the calm spirit was overclouded with gloomy forebodings. Müller might be seen sitting at the theatre, almost unconscious of the performance which gladdened the senses of others, or met wandering in remote streets, as if in restlessness and anxiety. The "nobile otium" which he had dreamed of in a little house on the banks of the Rhine was never to be his. He broke down prematurely on his way to that golden land. The Easter vacation of 1858 brought him not the usual happy satisfaction in uninterrupted leisure for his studies. As the summer term approached, he felt the necessity of doing something decided for his health. The chair which he had so long occupied at the University was resigned, and a medical consultation was fixed for the 28th of April. But on the morning of that day he was found dead in his bed. Calmly, peacefully, and alone, he had solved in his own person the greatest of physiological problems. His pupils and friends, after the University custom, accompanied him to the grave, and laid him in his final resting-place. "He lived a man, went from hence a man (as Goethe said of Winkelman); he now enjoys, in the remembrance of posterity, the privilege of appearing eternally great and strong, for in the form in which a man leaves the earth does he wander among the shades below."

We turn the page of this dualistic record.

Nearly half a century before the delivery of the Éloge by M. Bérard prefixed to this article, a young man, gifted with regular features and an intellectual expression, left his native country for Paris, in order to attend the medical lectures of the most renowned professors of that period. Save to himself and the few with whom he was brought in immediate contact, his name was quite unknown. It has now a world-wide reputation, for who has not heard of ORFILA? He was born of honest parents at Mahon, in Minorca, his ancestors having been established in the Balearic islands since the fourteenth century. The events of his youth are full of interest, and he has himself recorded them in an autobiography on which he was engaged at the time of his death, but which is not destined for publication. In this manuscript the illustrious professor, like many distinguished men who have preceded him, dwells with great force upon his youthful antecedents as exercising a very remarkable influence upon his subsequent career. The boy Orfila was placed under the charge of a Franciscan friar, who imparted to him the rudiments of Latin, and, neglecting history, geography, mathematics, and Greek, sedulously endeavoured to give to his young pupil's mind a taste for metaphysical study and disputation. The youth's thesis, in his fourteenth year, at a public trial of scholastic proficiency, was not a little calculated to embarrass many students at a "competitive examination," whether they had or had not been trained in the discipleship of Bishop Berkeley.* Orfila's craving desire, however, for knowledge prevented the restriction of his studies to the exclusive subjects of his Franciscan teacher. From a Languedocian abbot, whose exile to Minorca had been occasioned by the French Revolution, he learned the Gallic, and from an Irish priest the English, tongues. And it is worthy of observation, that with these languages he so contracted the accents of his respective teachers, that he would have passed for a native of Languedoc in Paris, and for an Irishman in London. Nor were these acquisitions sufficient to satisfy our young aspirant. He became so impressed with the importance of mathematics, that he bought books upon this science, and laid himself out to its diligent cultivation. He was indebted to the assistance of a mathematician whom he afterwards discovered in his native island, for a knowledge of logarithms, simple equations, and the elements of geometry. And he gave practical illustration in his own person, by imparting his gradual acquirements to others, of the acknowledged verity, that nothing impresses learning so strongly upon the mind as the process *alios docendi.*

The moment for that great subject of perplexity in every family—the choice of a profession—had now arrived. The elder Orfila drew for his son a charming picture of naval service. The younger Orfila essayed it, went one voyage, and returned home sick and disgusted. To a terrible tempest and a piratical attack, the profession of medicine is probably indebted for the addition to her illustrious children of the son of a Minorca merchant, whose special studies were initiated at

* "Impossibile est simul idem esse et non esse!"

Valentia in the year 1804, at the age of seventeen years and six months. In this field the young Orfila soon showed his rare capacity for study. The text-book on chemistry used at Valentia was by one Macquer, where we learn that air and water are *simple elementary bodies*. Although the professor who taught this science was aware that these compounds were capable of analysis and of reduction to simple elements, he was content to tread in the beaten path of unenlightened times, and require from his pupils an assent to that which did not meet the verified discoveries of the day. But Orfila had already procured the works of Lavoisier, Berthollet, and Fourcroy, and ceased to listen to his University instructors. In fact, the general inefficiency of his *Alma Mater* caused its corporate existence to be menaced with opposition and even suppression. A kind of scientific *soirée* was announced among the pupils by way of warding off the threatened danger, and all the *savants* of Spain were invited to assist at this literary tournay. Orfila was one of those engaged to take part in the discussions, the result of which was that he caused the complete triumph of an University which had taught him nothing, and of the astonished professor of chemistry, who naïvely inquired of his pupil: "But where did you get all your information?" Let us here transcribe a memorable passage from the *procès verbal* of this extraordinary gathering:

> "Matheo Orfila, in the examination which he underwent this evening, for nearly two hours, gave proof of such extensive and profound information on the subject of chemistry and the collateral bearings upon that science; he expounded with so much ability the three subjects assigned to him, showing their application to science and the arts, analysing ancient and modern opinions, and answering with promptitude and correctness all the difficulties proposed to him, that he completely paralysed his competitors, and received the first prize from the judges, with the unanimous acclamation of the assembly."

Nay, the judges even went further: they declared that the prize was not sufficiently worthy of the prizeman's merits, and decreed that the inscription MATHEO ORFILA VICTOR should for ever be preserved in the University, in order to excite a noble spirit of emulation among its students in succeeding ages. The joy of the triumph, however, was on the morrow singularly interrupted. The young Orfila was summoned before the Grand Inquisitor. "You achieved a brilliant triumph yesterday," said that functionary, "and I joined in the applause which was so abundantly bestowed upon you. I love and esteem the studiousness of youth, and I desire to encourage it by all the means at my disposal. Who are you? where do you come from? and what are your intentions as regards the future? But did I understand you right," added the Grand Inquisitor, "when I gathered from your argument that you threw out some surmises in accordance with notions of physics and geology, from certain French authors, as to the world being much older than is generally supposed?" It may be imagined that the young laureate's explanations were not less satisfactory than those of Buffon when the iniquitous Sorbonne tortured him upon his theory of the earth, for the edified questioner thus took leave of him:

"Young man, pursue your studies unmolested; honour Spain, and do not forget that in the present day the Inquisition in this country is neither so officious nor so barbarous as she has the character of being." This anecdote is taken from Orfila's own manuscript, and deserves to be related for the benefit of Exeter Hall and the entire Christian world. It may be mentioned here, too, for the encouragement of those who think highly, and the reproof of those who think lightly, of the value of modern languages, that one of Orfila's great advantages over his colleagues and contemporaries arose from his intimate acquaintance with these media of communication with varied literature and peoples.

Our young *savant* had thus, as it were, reinstated the University of Valentia. But he knew that she had nothing more to teach himself. The University of Barcelona was at that period in great repute, and Orfila becomes a student there, and for the first time has the satisfaction of seeing a public demonstration of *experimental chemistry, practical anatomy*, and *clinical pathology*. Here he remains two years, making the most profitable disposition of his time, and the most sedulous application of his talents. At the end of that time the commercial junta of the town elected him, *à titre de pensionnaire*, to study, first at Madrid and afterwards at Paris, chemistry in its application to the industrial arts. But he obtained permission to proceed at once to France, which he never again quitted, save for brief and temporary purposes of travel. Orfila arrived in Paris on the 9th of July, 1807; and the wondrous country of which that city is the wondrous capital henceforth received him as her adopted child.* So strikingly was his aptitude and real love for work at once manifested, that Vauquelin introduced him into his laboratory, and Fourcroy entrusted him with the preparation of some lectures on organic chemistry. Faithful to that system of teaching others which he believed had been so largely instrumental in teaching himself, he had now an opportunity of instructing a fellow-student, the son of a rich *propriétaire* in the Rue du Bac. The father fitted up a small laboratory, where the young professor gave experimental lectures to the son, and others who cared to witness them. One day, while thus occupied, two persons suddenly entered the room. The astonishment of Orfila upon recognising in them no less illustrious visitors than Vauquelin and Fourcroy may easily be imagined. But his presence of mind did not forsake him. The assembly acknowedged by rising the honour of this addition to their circle; and the lecturer, inviting them to be seated, proceeded in his discourse with the greatest calmness and self-possession.

But the student life was soon subject to the most disastrous interruptions. The long and cruel war between France and Spain was at hand. Murat bombarded Madrid by order of the Emperor; and, some time afterwards, Dupont was defeated at Baylen by General Castenoz. The relations of the young Orfila with his family and the commercial junta of Barcelona were interrupted. Misery threatened:

* Orfila did not really become a naturalized French subject until 1815, upon his appointment to an office which necessitated the taking of that step.

suspicion attached itself to one of Spanish origin. Accompanied by one of his friends, he goes to the prefecture of police to solicit a *carte de sûreté*. The result of his visit is, that after sundry questions and formalities, both are detained as prisoners. The prefect was worse than the Grand Inquisitor; but the favourable impression which he had made upon his teachers secured a liberator for the disciple.

"After a sleepless night," Orfila himself records in his autobiography, "I was conducted into the apartment of M. Vera, where I expected again to be questioned and cross-examined. But no: I am the happiest of men. Vauquelin is there in the dress of the Institute, with his sword at his side, and clothed in his official insignia. 'Je viens réclamer, Monsieur,' says he to Vera; 'il ne troublera jamais l'état: je l'emmène.' 'Be it so,' replies Vera. Vauquelin gives me his hand; I am in his arms; he tenderly presses me to his bosom."

We shall see presently how this excellent man again proved himself *un génie bienfaisant* at a most critical period of his pupil's life. Being free, and unembarrassed by political fears, Orfila now resumes his studies, and receives his degree of doctor in medicine from the Faculty of Paris in the year 1811. The subjects which were most connected with the science of chemistry did not receive his undivided attention, for his examination in pathology obtained the distinction of "highly satisfactory."

But how is our young physician to obtain the means of subsistence? The war with Spain continued, and had done no little damage to the fortunes of the house of Orfila. The father wished his son to return to Mahon, and sent him three hundred francs wherewith to defray the expenses of his journey. The son, though his pockets were empty, sent back the money, and declined to embrace the paternal offer. He had confidence in his own destiny, and a presentiment that favouring breezes would fan his sails, though they might not be under the shelter of his native shores. Nor was that confidence misplaced. In the occupation of teaching, which had hitherto only endued him with knowledge, he is now enabled to supply himself with the means of material life. He opens a course of lectures in the Rue Croix des Petits-Champs, and afterwards in the celebrated Rue du Foin-Saint-Jacques—the cradle of special instruction. Here he gives chemical, medico-legal, and even anatomical lectures, and lays the foundation of a new science—the science of toxicology. For although it is true that before Orfila's time we could recognise by tests and reagents certain poisons when dissolved in distilled water, yet we are indebted to this great practical chemist for the ingenious processes by which they can now be detected, even in the most minute quantities, in any solvent. Since the day when he initiated his elaborate experiments, justice has been doubly armed against secret crime; and many a would-be murderer recoils from the perpetration of that which he can now no longer hope to conceal from the searching scrutiny of analytical science.

As Englishmen, we may be proud to record how faithfully Orfila, like Müller, conformed to the precepts of the illustrious Bacon, and from the very earliest period of his studies preserved and inculcated the habit of submitting every scientific proposition to the test of direct personal experiment. The larger errors of that pseudo-science which then bore the name of toxicology could not escape the severity of the processes by which he gauged them. In illustration of which he himself relates the following circumstance. It was in 1813. He had just shown his audience the precipitates which arsenious acid forms with various reagents. "These precipitates will even be produced if the arsenious acid is mixed with wine, with coffee, or with broth; of which I will give you a practical demonstration." The lecturer held in his hand a cup of pure coffee, into which he introduced the solution of arsenic, and poured the successive reagents into separate portions of this compound fluid. To his horror, the lime-water, which should yield a white precipitate, yielded a purplish-brown; whilst a greyish-olive precipitate was the miserable substitute for the beautiful *Scheele's green* which should follow the addition of the ammonio-sulphate of copper. The embarrassment of the young professor was extreme, and he adjourns the explanation of his failure. On his return home he mixes all sorts of poisons with all sorts of alimentary substances, drinks, and animal secretions; submits them to their ordinary reagents, and proves that the result differs almost invariably from that afforded by these respective drugs when dissolved only in pure water. Yet the poisons existed in the compound mixtures, for he himself placed them there. To discover the means of unmasking this difficulty became to him henceforth the object of his entire thoughts and energies, of which he has left us the enduring results in facts which now make the path of chemical investigation a comparatively easy one. With the true enthusiasm of youth, and the presentiment of conscious genius wherewith to achieve all he aspired to, Orfila goes at once to a bookseller and publisher, and negotiates with him for the sale of two volumes now engaging his attention upon this science as yet in embryo. The publisher (M. Crochard) accepts this singular offer, and (it is reasonable to suppose) made some pecuniary advances towards the prosecution of the work, which appeared in due course, met with immense success, and has already (receiving with each new impress the corrections necessary to bring it up to the science of the day) passed through five editions.

Orfila, like his contemporary Magendie, has been accused of an inordinate sacrifice of animal life in carrying on his experimental inquiries, and met with much censure thereupon. We are disposed to think that the matter has been overstated, and that a very unnecessary squeamishness has essayed to stop legitimate physiological experiments. Far be it from us to advocate unnecessary cruelty; but far be it from us to set up canine existence as a thing so valuable as to forbid its utilization for the purpose of discoveries, either positive or negative, capable of benefiting mankind.

The great advantage of the dog genus to Orfila, in preference to any other, arose from that physiological privilege of vomiting "promptement et facilement" (as M. Bérard expresses it), which renders it meet for the experiments of the toxicologist. There can be no doubt that by the sacrifice of many dogs the great French chemist largely added to our knowledge on the subject of poisoning. Through them he was enabled to examine and analyse matters expelled from the stomach and intestines, or yet contained in the digestive tube. He demonstrated the possibility of extracting poison from the liver, the blood, and the urine, whilst there were no traces of it in the *primæ viæ*. This was the greatest discovery, in a medico-legal point of view, which had yet been made in toxicology—a discovery to which the young professor had devoted six years of his life.

It is known to us, that during the last, and even the earlier part of the present century, the theory of poisonous action was that of propagation of deleterious influence through nervous channels to the nervous centres. Wherever poison was applied to a limited surface, it was immediately taken up and conducted with telegraphic dispatch through nervous filaments to the head quarters of the *vis nervosa*, there paralysing and destroying its manifold functions. Haller, the faithful historian of medical science, had embraced and propagated this theory. It was, indeed, universally recognised as the veritable exposition of the cause of death:—"Neque alia via ad mortem brevior est, venenis vehementer acribus in ventriculum receptis," said the illustrious author of the 'Elementa.' It is true that between the setting of Haller's and the rising of Orfila's star, physiological science pointed out that, independently of the case of local abrasion, the absorption of a poison is the condition of its *action toxique;* in a word, that its deadly action is conveyed to the matter which immediately presides over life through the channel of the blood. Yet for all practical medico-legal purposes the discovery was unavailable until the great man with whose narrative we are now occupied utilized it by his brilliant experiments. Henceforth the detection of secret crime becomes in a fuller sense retrospective. Days, weeks, months after the commission of a murder, and the consignment of the miserable victim to the tomb, science unshrouds the dead, and points to the imperishable evidence of the murderer's guilt in the decomposing elements of mortality. Whilst pursuing these researches, Orfila was led into another path having a most comprehensive connexion with toxicological chemistry and physiology. The elimination of poisons introduced by the process of absorption into our organs, the times of their respective sojourn in the body, the special routes of their exit; such, with its medico-legal inductions, the prognosis, and the therapeutic indications, was the subject of those studies to which he invited the attention of his contemporaries.

Of a truth, the difficulties and complications of toxicology are endless, and such as can only be duly appreciated by one who has the most entire acquaintance with analytical chemistry. For instance, if in a juridical post-mortem examination we discover poison, we have no right to conclude that it has been criminally administered, without the strongest collateral evidence. For may it not have been introduced there after death, with the view of attaching suspicion to an innocent person? And does not our corporeal structure contain, in its natural state, metallic substances? The water which filters through the soils of our burying-grounds, may it not deposit poison in the very bodies which are there undergoing decomposition? But Orfila furnished a solution to these questions and difficulties, and demonstrated the means of distinguishing the metallic substances as they exist in a natural or normal state, from those which would have given rise to metallic poisoning. It was not enough, however, for him to create the medico-legal history of poisoning, though that might have been sufficient for the reputation of a single individual. He traces and lays down the rules to be observed in the exhibition of antidotes. We are wont to overlook the fact, that when this science was initiated, many of its reluctant disciples, *pretendus vitalistes*, believed that the organic action of the stomach would vitiate the power of chemical agents, and neutralize the affinities of one substance for another. So that within the walls of the digestive cavity an alkali would not antagonize that with which, under non-vital conditions, it would form a neutral salt; magnesia would not allay and counteract the horribly corrosive action of sulphuric acid, and tannin would not precipitate tartar emetic. Why the ordinary chemical affinities are so impartially preserved under every condition, that by reason of the fluid of the stomach (water) a poison may be generated from two substances which are previously inert, until their junction is effected, as by water, in an inorganic vessel. Is it not after this manner that the most active of poisons—hydrocyanic acid—is developed from the innocuous isolations of emulsine and amygdaline? The organic ("vital") receiver, and the inorganic, alike bear evidence to the positive and irreversible character of chemical laws. Orfila, too, was the first to advise the administration of albumen in a variety of metallic poisonings, and that by corrosive sublimate in particular. He pointed out, at the same time, that animal charcoal had no real claim to the title of a metallic antidote—its results being of a spurious and deceptive character. He first warned us, also, of the caution necessary in the exhibition of antidotes, so as not to counteract the agency of one poison by a substance which, combining with it, at once destroys its potency, and generates in that very process another poison equally destructive.

Thus far we have alluded to some of the difficulties which Orfila set himself to combat, in creating the special science of toxicology. It is impossible to go more into detail, for that would involve the introduction of almost every chemical substance which is known to us. There is one, however, the most celebrated of all in judicial annals—equally destructive of rats and humanity—which must not be passed over. Arsenic, that drug so extensively used for criminal purposes, engaged the most earnest labours of the great physician; the result of which has been the most complete treatise which ever proceeded from the pen of a toxicologist.* All those (and they were at the time many) who antagonized his opinions and denied the legitimacy of his experiments, were eventually compelled to acknowledge his accuracy and truthfulness and experimental aptitude. The apparatus known

* Toxicologie Générale.

as *Marsh's test*, discovered in 1836, presented in its very simplicity the most serious inconveniences, which various chemists commissioned by the Institute endeavoured to obviate and rectify. There was one difficulty, however, which still perplexed them. In testing by this ingenious mechanism a fluid which contains organic matter, a thick viscous froth almost immediately chokes up the end of the tube from which the gas escapes, and prevents the formation of the characteristic arsenious ring upon the glass or porcelain held for its reception over the lighted orifice. Orfila conceived and executed the brilliant idea of destroying this animal matter by the addition of acetate of potash (and subsequently by acetic or hydrochloric acid). The result was the disappearance of the viscosity, and the disengagement of the hydrogen gas, with the same regularity and precision as obtains from a simple arsenious solution, and the detection of the most minute portion of the poison by its crystalline deposit. He dwelt most forcibly upon the extreme delicacy necessary in conducting this analytical process, and indicated the precautions which alone could ensure success. By reason of too large a flame, and an incorrect angle of incidence in the porcelain held for the reception of the arsenious fumes, the first experiments in the celebrated Lafarge case had been a complete failure, the metallic crystals not having been deposited. But, said Orfila's adversaries, the crystalline deposits which you point to as evidence of the existence of arsenic equally result from the carbonization of organic matter, from the presence of phosphatic salts, or from antimony.* True, indeed; but not less true than that Orfila had himself indicated the apparent similarity of these deposits, and the certain means of distinguishing them from those of pure metallic arsenic.

The immense reputation acquired by this most searching investigator necessitated his being daily called upon to give evidence in difficult criminal cases. His depositions were always listened to with breathless silence by an anxious crowd, who regarded his testimony as the absorbing feature of the trial. But let us recur to the period of Orfila's first special teaching, and first authorship. His 'Toxicologie Générale' was immediately translated in Germany, Italy, England, and the United States of America. The Government of Ferdinand VII., however, now became acquainted with the fact that a young Spaniard was in Paris, laying the foundation of a rapidly increasing and brilliant reputation. Accordingly it strove its utmost to force him from the Gallic shores. A decree inserted in the Spanish Official Gazette summoned Orfila to the chair of Chemistry which Proust had occupied at Madrid. But the French Government, on its part, could not contemplate with indifference the loss which would be occasioned by the

* "The only objection of any practical force is that founded on the presence of *antimony*. There are these differences between the arsenical and antimonial deposits: the deposit of antimony has rarely the bright metallic lustre which that of arsenic commonly presents; by transmitted light it is of a smoky black, while that of arsenic is of a hair-brown colour. Although the antimonial is very similar in colour to the arsenical flame, yet the third property is entirely wanting."—See Professor Taylor's Medical Jurisprudence, fifth edition, p. 72.

withdrawal of their rising professor from the Parisian schools. The ministers of Louis XVIII., therefore, offered him the post of *Médecin par quartier* until a chair worthy of his acceptance should be at their disposal. The coincidence of these two offers somewhat astonished "the rose and expectancy" of the scientific world in Paris, who had asked no favour and presented no petition. But still this was not the first surprise which he had encountered; for prior to this kingly contention the direction of the Italian Opera had proffered him an engagement of the value of 25,000 francs.* But the medicus had a nobler ambition, and unhesitatingly declined the Thespian invitation. Between France and Spain, however, the choice was more difficult. He loved, indeed, the land of his adoption, but Spain was the country of his birth. And the latter consideration would probably have prevailed in determining his future, if Ferdinand's prime minister, imbued with the wretched spirit of *red-tapeism*, had not refused to carry out a university reform which Orfila had submitted to him as the condition of his acceptance. The great toxicologist, therefore, remained in France. At the Peace of 1814 a feeling of great delicacy led him to place at the disposition of his *anciens protecteurs* those scientific treasures which he had accumulated in Paris. But the junta, ruined and disjointed, was unable to give effect to his generosity.

Orfila was now inducted to the chair of Medical Jurisprudence by the Faculty of Medicine under circumstances alike honourable to the electors and the elected. This step necessitated his naturalization as a French subject. His inaugural lecture, in a crowded theatre, on a medico-legal subject which had no connexion with toxicology, was a sufficient reproof to those jealous and bitter enemies who affirmed that legal medicine would now degenerate into a mere history of poisons. From that day, and during every subsequent day of the session—during the four years of his medico-legal professorship, and the twenty-nine years of his tenure of the chair of Chemistry, he was invariably surrounded by a large and eager audience. His success as a lecturer was most remarkable, and lay, not in pompous periods and pretentious discourse, but in a singular adaptive faculty, which enabled him to impart his information to others in forcible and perspicuous language. Moreover, he was careful to illustrate by experiments, whenever it was possible, the subject matter which he laid before his hearers. It is an error to suppose (as many have done) that in the extended field of medico-legal science Ofila was solely occupied with that limited portion which bears directly on toxicology. The publicity accorded to his general discoveries, and the publication by him of an elaborate treatise in three volumes,† shows how carefully he had studied all the branches of this vast and comprehensive subject. Here, again, he strongly inculcates experimental investigation as the only sort of investigation that is reliable. If he desires to know what are the relative

* This offer to a physician may somewhat astonish our readers, even when they are told that Orfila was a most accomplished musician, gifted with a voice of great compass and beauty. He was a proficient with the piano, violin, guitar, and flute.

† Traité de Médecine Légale.

local signs by which it may be determined whether an individual found suspended by the neck, was so suspended before or after death, he hangs up a corpse, and afterwards dissects the parts pressed upon by the cord, notes the kind of extravasation, and lays by his results ready for comparison, at a moment's notice, with the terrible realities of his criminal experience. In the same way he determines the question of the turgescence of the corpora cavernosa of the penis, and the presence of spermatozoa in the urethra. The subject of personal identity also largely engaged Orfila's attention. It is one which will not appear trifling to those who are at all aware of the difficulties of this deceptive process, and the delicacy of the means employed for its detection. To alter the colour of the eyebrows and the cranial hair is an artifice not a little resorted to for criminal concealment. Orfila became such an adept in effecting these changes, as might excite at once the fears and the envy of the *coiffeurs* of the Rue de la Paix or the Burlington Arcade. He could run through the colours of the rainbow, and restore to the most hirsute mortal his natural and primary shading. In his hands the Æthiopian could change his skin and the leopard his spots. So great was his love for scientific investigation, and so anxious was he to clear the rough places of medical jurisprudence, that he was appalled by no difficulties, however arduous or trying. His researches on the question of material decomposition, with a view to laying down some indications for the physician called upon to determine the period which has elapsed between actual death and its judicial discovery, will alone attest the truth of this observation.* A more disgusting office cannot be imagined than that of dissecting and comparing bodies in their varied states of decay, from the period of first entombment to one more or less removed from the investiture of the shroud. Yet Orfila had courage for all this; and he has left behind him a mournful portrait gallery, drawn from nature, of the absolute and relative marches of disorganization.

The circumstance which transferred the Professor of Medical Jurisprudence from that chair to the one of Chemistry was a matter of great regret. A royal decree had dissolved the faculty. It was a part of the plan of those who presided over its reorganization to suppress the chair of Historical Medicine, give to Royer-Collard that of Medical Jurisprudence, and put Orfila into the place of his friend and benefactor, Vauquelin, who, together with Antoine Dubois, De Jussieu, Deyeux, Pelletan, Lallement, and Desgenettes, was entirely excluded. But Orfila, aware of the intentions of the council, seeks his friend and *quondam* instructor, makes him acquainted with the facts, and of his own intention to decline professorial service. His generous nature will not brook the exclusion of his generous friend. "They dare not do it," said Vauquelin. "But they dare," replied Orfila. "In that case," nobly resumed the elder *savant*, "I require of you to accept the chair of Chemistry. Your refusal would not benefit me, and it would

* Orfila published a treatise in 1830, conjointly with M. Leseur, entitled, 'On the Appearance presented by Dead Bodies after Exhumation, Drowning, Suffocation in Cesspools or by Gases.'

deprive the students of that excellent teaching of which your antecedents give such abundant promise."*

Henceforth Orfila appears before us in a new light. With years and labours come honours, and their too frequent accompaniment of painful anxieties. And yet it may, after all, be questioned whether the act of bearing well the anxieties is not the real honour. The Professor of Chemistry becomes Dean of the Faculty. The history of his promotion to that office is not less interesting than his elevation to the chair which he then adorns. The Revolution of 1830 had restored the professors dismissed by the unjust decree of 1822. The celebrated Antoine Dubois, recently and reluctantly elevated to the deaconate, requested Orfila to accompany him to the Minister of the Interior on (as he said) a matter connected with their joint administration. They had scarcely entered the Government chambers when Dubois thus addresses himself to the Minister: "Sir, I am old, and but little anxious for the responsible functions of the deaconate which has been entrusted to me. I beg of you at once to accept my resignation, and to substitute my distinguished colleague, M. Orfila." On the next day the nomination thus made was (to adopt our legal phraseology) signed, sealed, and delivered, such was the confidence of the Minister in Dubois' selection, and in the reputation of Orfila himself. The cares of this new post did not interfere with the professor's labours in the science of his special devotion. Nor did they interrupt the regularity of his teaching; for nothing could have compensated him for the loss of the love and esteem of his pupils—a result which would have followed his less frequent communication with them. Moreover, he knew that he could not better inculcate regularity and exactness in his colleagues, than by setting them an example of the same in his own person. What better sermon, indeed, could he preach than that which the brother of John Wesley pronounced to be incomparable: "the sermon of a consistent life"?

And whilst the novitiate of our dean was occupied in the maintenance of discipline, it was engaged also in rebuilding the material structure of the school. In place of a hideous, was reared a lofty, spacious, and elegant building, wherein clinical instruction was given in surgery and midwifery—the latter being an entirely new feature in the programme of the faculty. New dissecting-rooms were substituted for filthy and unwholesome ones, where the students might work without detriment to their health. To Orfila, likewise, Paris is indebted for that magnificent botanical garden where every plant has its name appended to it for the benefit of inquiring observers. Dupuytren had liberally left the wherewith to found a chair of Pathological Anatomy. Orfila managed to create therefrom not only a pathological chair, but also a pathological museum. Before the com-

* This was the circumstance to which we before alluded as a thing for subsequent narration, occurring at the most critical period of Orfila's life. That Vauquelin's good opinion and prediction were amply justified in the future of his pupil is clear to demonstration. Orfila's 'Elements of Chemistry' has reached its eighth edition. Yet he himself said of it, with characteristic modesty, that it had no other merit (by no means an insignificant one) than that of *co-ordinating* facts.

pletion of this great work he had visited many anatomical galleries, among which the Hunterian Museum in the Royal College of Surgeons had excited his especial admiration. This feeling, combined with that of humiliation at the comparative inferiority of the French galleries, inspired him with an intense desire to rival the collection in Lincoln's-inn-fields. He returns to Paris, and consults the Minister of the Interior upon the subject. That functionary yields to the pressing solicitations of the enthusiastic professor and dean. The necessary arrangements are made, and the work is at once initiated. Never was so complete a change effected in so short a period. The new galleries were not yet completed before the pouring in from all quarters of varied preparations wherewith to enrich them. Orfila had set in motion the most elaborate machinery for achieving this end, and had put himself in communication with the most celebrated anatomists of Europe. Eschricht of Copenhagen, Panizza of Pavia, Hirtl of Vienna (at that time professor at Prague), Erdl of Munich, and (though last not least) our own Professor Owen of London—all were consulted by him, and all, in the true spirit of scientific brotherhood, lent him their assistance. The creation of this museum was, in fact (as M. Bérard describes it), a veritable *tour de force.* In five months the whole was finished, and sixteen thousand specimens were arranged in the new galleries. The faculty expressed by a vote its recognition of the brilliant services of the dean; and M. de Salvandy, the Minister of the Interior, was afterwards desirous of acknowledging them by gifting the anatomical museum with the name of the distinguished man who had thus effected its happy transformation.

We are astounded in the case of Orfila (as we were in that of Müller) as to how he found time for his great and multifarious labours. But the truth is, we suspect, that time is never wanting to those who know how to regulate its employment and economize its disposition. Our professor's methodical habits were most striking. Those who wanted him, and knew his habits, had only to look at their watches, and they at once knew where to find him. He had no bustle, impetuosity, or confusion. But he was calm, systematic, dignified, laborious, and yet with sufficient enthusiasm to ensure rapidity of execution. To these attributes he added a courtesy and politeness which inspired every one who approached him with respect. During his deaconate he had been elected on the General Council of Hospital Administration; and in 1851 he was chosen President of the Academy of Medicine, of which he had been a member since its foundation. In both of these capacities he exercised true wisdom, and displayed a rare spirit of conciliation. He also, during this high official period, conceived and executed the idea of founding a *Société de Prévoyance* for physicians of the department of the Seine. This generous and charitable work was a new development of that anxiety with which he ever watched over the interests of his professional brethren. It was, indeed (as he himself regarded it), the most sacred of his many missions, dignified by the wisdom of age, and rendered holy by the comfort which it imparted to those born to a lot less happy than his own.

But Orfila has not yet reached the highest pinnacle of his honours. He is appointed by the King to succeed Cuvier in the University at the Supreme Council of Public Instruction. Then it was that he commenced organizing preparatory schools of medicine and pharmacy, and increased the value of the doctorate in medicine, by an enlargement of the educational requirements and a general extension of the university curriculum. Nor did he use his influence in effecting these reforms to the exclusive advantage of the schools to which he was himself attached. He instituted new professorial chairs in the Faculties of Strasburg and Montpellier, and elevated to the utmost everything that he could bring within his comprehensive reach.

We have thus familiarized our readers, though briefly and imperfectly, with the leading features of the public life of Orfila. We raise for a moment the curtain of his private life, because we are sure that those who have thus far traced the narrative of this scientific labourer will be interested to see him in the retirement of social relationship. Not that his private life was, in the strictest sense, one of social retirement, as compared with our English notions of domestic quietude. The social life in France is the life of the salons, and here we are invited to see the great toxicologist, rather than by his own fireside. His musical talents (to which we have before alluded), together with his courteous manners and his colloquial powers, procured him from the very first an *entrée* into the best society. He there met with a lady who afterwards became his wife, and whose musical tastes were equal to his own. Such a union ensured the married couple a flattering reception in the leading salons of Paris. In those of the Princesse de Vaudemont and Comtesse de Rumford (the widow of Lavoisier) Orfila met the most illustrious representatives of science, literature, and art. The relations which he there contracted with many of these personages singularly smoothed in after years the difficulties of his administrative life. "He has himself told me," records M. Bérard, "J'ai obtenu plus de décisions avantageuses pour la faculté, j'ai mené à bonne fin plus d'entreprises relatives aux études, dans les salons que dans les bureaux des administrations."* With celebrity and fame came competence. Orfila was in his turn able to *receive.* Whatever he undertook, socially or scientifically, he did well. His *soirées*, therefore, were unusually brilliant, and attended for upwards of twenty years by the most distinguished *artistes* of France and Italy. In fact, his domestic

* We regard this affirmation as worthy of peculiar notice, for we believe that there are well-to-do practitioners in London who decline to move in society, save in a professional capacity, under the impression that they are wedded to an exacting mistress who demands their whole time and attention. There can be no greater delusion; and, as far as we have observed, the victims of it have been men of singularly narrow minds and penurious habits. The best general society is the best general food for an intelligent mind in its necessary hours of relaxation. "But as I grow old, every year convinces me more and more that social intercourse of the right kind is a material aid to success."—('Cornhill Magazine,' No. 12, art. on "Success." M. Bérard very properly comments on this avowal of Orfila in these words: "Cet aveu scandalisera peut-être ces hommes qui cherchent dans un maintien austère et l'ennui qu'ils infligent une sorte d'appoint à une réputation d'administrateur ou de savant; mais les succès dans le monde ne pouvaient être compromettants pour celui que tant de travaux sérieux et utiles recommandaient à l'estime des véritables savants et à la reconnaissance publique."

life was full of happiness, as his public life was crowded with usefulness.

But the hour of adversity was at hand. The Revolution (1848) burst forth. One of the first acts of the Provisional Government was the deposition of the dean. It was the miserable stab of a secret enemy, a letter from whom to a friend, in which he felicitates himself upon the successful overthrow, is in the possession of Orfila's family, and reveals to them the baseness of humanity. This act, too, was followed by a general attack upon his decanal administration. Orfila took but little notice of it. His deeds are now a matter of history, and it is not too much to say that they will excite the gratitude of all lovers of educational science long after the remembrance of this cowardly depreciation of his merits shall have faded from the minds of men. He was himself supported by a dignified consciousness of rectitude, and by a stoical firmness which belongs only to the guileless and the strong. But still the effort was great, and the shock, which he could not conceal from his friends, commensurate. What a comfort to him now was their and his pupils' affection! It touched him with overflowing tenderness, and opened yet more abundantly that generous nature. It led him to anticipate benefits which he had intended only for posthumous execution. He makes over 121,000 francs for the completion of the Anatomical Museum of the Faculty, the institution of an academic prize, and for certain other establishments of superior instruction. From one end of France to the other the profession acknowledged by acclamation this splendid munificence.

"Et cependant," eloquently and feelingly writes M. Bérard, "je ne sais quel triste pressentiment m'assiégeait, lorsque j'entendais Orfila annoncer qu'il donnait *de son vivant* pour surveiller et diriger l'exécution de ses volontés; il me semblait voir, dans ce langage trop confiant, une sorte de défi jeté à la destinée humaine. Helas! la mort devait frapper le donateur avant la réalisation légale du bienfait; ces adresses de félicitation que la province lui faisait parvenir de toutes parts, c'est sur sa tombe qu'il a fallu les déposer."

Orfila had lectured on the very morning of the day on which he took to his bed to rise up no more. It was observed that he was singularly fatigued before the completion of his duty. But he would not relinquish it; he carried himself through to the end. "C'était la mort du soldat sur le champ de bataille." The intelligence that he was ill—that he was in danger—spread through Paris. The doors of his house were besieged by anxious inquirers. Death was not laggard in doing his work. On the morning of the 12th of March, 1853, to him who had unravelled so many hidden things was given the key of yet greater mysteries, and the good and great physician "the clay house of this world forsook, and the house everlasting entered."

Such are the two remarkable men who have contributed so largely to the scientific acquirements of the present century, and who, though dead, will yet speak to us for ever. The reciprocal influences exercised by the sciences which Müller and Orfila respectively represent, give a double importance to physiological and chemical discoveries, and indicate at the same time the peculiar appropriateness of their literary conjunction. And this appropriateness is increased in no small measure by the similarity of the gifts, and conditions, and modes of accomplishing what they did, of the physiologist and the chemist themselves. Born and educated alike under the pressure of circumstances which clog the activity of thousands, they have bequeathed to us the positive results of their achievements, and the priceless benefit of their examples. That they should accomplish much in the paths which they had severally marked out for themselves, both Müller and Orfila had abundant presentiment—a presentiment which furnished the strongest stimulus to exertion, and gave them that self-confidence which is the harbinger of success. Even the scholastic life was fruitful in evidence of a perseverance and an energy which never remain unrewarded. Afterwards comes the period of more complete self-dependence, though at an age when most young men are drawing their supplies from the paternal sources. We have seen how something akin to poverty (that hardest of task-masters) knocked at Müller's door, and how Orfila declined to accept a pecuniary imbursement which he greatly needed, and return to the shelter of his father's roof. We have observed, too, how, even during the entire academic and subsequent career, the whole time of these true *scientiæ discipuli* was given to that which they had severally undertaken. To support one fact by another, to confirm or invalidate previous experiments, to reject the accidental or empirical, and establish the principles of inductive science, became henceforth the occupation of their lives. By a consistent perseverance under all the difficulties which beset them, and an undeviating rectitude of conduct which never stooped to resent the professional jealousy of contemporaries, they preach aloud to many among ourselves, and offer no slight contrast to many who are no longer of this generation. There is little more to be said. Nothing, indeed, save to present our acknowledgments to MM. Du Bois-Reymond, Virchow, and Bérard, for having furnished us with the materials from which this narrative has been chiefly constructed, and to assure our readers of the pleasure which it has afforded us to erect in the pages of a scientific review a monument to the true dignity of Labour.

Review II.

1. *Discours sur le Vitalisme et l'Organicisme et sur les Rapports des Sciences Physiques en Général avec la Médecine: Discours Prononcé à l'Académie Impériale de Médecine, 17 Juillet*, 1860. Par M. Le Professeur Bouillaud.—*Paris*, 1860. 8vo, pp. 75.

A Discourse on Vitalism and Organism, and on the Relations of the Physical Sciences in General with Medicine: a Discourse pronounced at the Imperial Academy of Medicine, 17th *July*, 1860. By Professor Bouillaud.

2. *On the Philosophy of Discovery: Chapters Historical and Critical.* By William Whewell, D.D., Master of Trinity College, Cambridge, and Corresponding Member of the Institute of France. (Including the completion of the third edition of the 'Philosophy of the Inductive Sciences.')—*London*, 1860. Small 8vo, pp. 530.

3. Léconte: *The Correlation of Physical, Chemical, and Vital Forces, and the Conservation of Forces in Vital Phenomena.* (The 'American Journal of Science and Arts,' vol. xxviii. p. 305. 1859.)

4. *Physiological Riddles.* ('Cornhill Magazine,' July, August, September, and October, 1860.)

5. *Address of the President (Sir B. Brodie), November 30th*, 1860. ('Proceedings of the Royal Society,' vol. xi. p. 8. 1861.)

Intending to take the Discourse placed at the top of the list as our guide, as it was the actual cause of our being led into a train of thought that brought the other works mentioned into a common view with it, we will let M. Bouillaud open his harangue in his own manner:

"Gentlemen, before entering upon the great questions that Professor Trousseau has thought proper to moot, on the occasion of the report made by our colleague, M. Devergie, in the name of a commission of which I had the honour of being a member, I ought to declare that this report has been too well defended by its learned author to leave aught else for me to do than simply refer to that defence. M. Devergie is one of those who does not need help.

"That if I mount this tribune on the subject of those high questions which, since the discourse of M. Trousseau, have been debated, it is not that, in certain respects, they have not been sufficiently fathomed by the preceding orators, MM. Poggiale and Piorry, who both have so powerfully captivated the attention of the Academy. But, gentlemen, there remain still in the discourse of our eminent colleague, M. Trousseau, in regard of special nosography, as in regard of general nosography, and of what is still called *medical philosophy*, some assertions which touch me too nearly—I will say more, too contrary to the doctrines which I profess—for me not to feel that, with the consent of the Academy, I am bound to speak.

"Who would have thought, gentlemen, that a report on the perchloride of iron would raise so stormy a discussion, not only in the bosom of the Academy, but in the whole medical press? It is that we had not foreseen that upon that occasion M. Trousseau would formulize one of those professions of medical faith, many articles of which would, if they were accepted, overturn from top to bottom the present constitution of medicine."

Thus M. Bouillaud, whilst pointing in admiration to the fact that the spark which called forth such eloquence and excited such intense intellectual commotion was struck by the perchloride of iron, had nevertheless witnessed too many *émeutes* in Paris—their very crater—not to be entitled to hint that he had not believed the floor of the Academy proof against an eruption. But apart from the materials for ethnological study furnished by the graphic little narrative he gives, the gravest members of our profession all the world over cannot but anticipate that the subject-matter must be of interest to our whole body, where names of such weight as the above in medicine and science appear in the course of the discussion, and which, moreover, results in an essay by a man of M. Bouillaud's reputation under so abstruse and comprehensive a heading as that which lies before us.

The different sections of the medical press had interpreted M. Trousseau's said profession of faith in such devious senses, that he had been claimed by various shades of the rival schools of Paris and Montpellier, of Bichat and Barthez, of organicists and vitalists, as one of their disciples.

> "I'm bird with wings, exclaims the bat;
> I'm mouse, long live my cousin rat."

After some nimble dialectic lunges, M. Bouillaud brings the point of his rapier to his adversary's breast, and summons him to a more explicit avowal, to which, to say the truth, the latter shows no signs of repugnance.

M. Bouillaud: "What is your *real* profession of faith?"

M. Trousseau: "Here it is in two words. I believe that there is in animals no manifestation which does not suppose a *substratum;* that is to say, a tissue or an organ: *I am, then, organicist.*

"I believe, with Descartes, that the free and immaterial principle has in man (and I will add in animals) nothing to do with the nutritive functions, or that which M. Dollfus has so spiritually called the kettle (*pot-au-feu*) of the economy: *I am, then, not animist* in the physiological point of view.

"I believe that living matter, animal or vegetable, has manifestations which are proper to it, which appertain only to it. I call them, for want of better designations, vital forces or vital properties: *I am, then, vitalist.*"

Our author, hoping to have thus gained a clearer notion of the points upon which he might be expected to enlarge, again reminds us that the medical public had been pressing to these *séances*, as the ordinary public to a theatre when some piece eminently dramatic is being performed.

> "Conticuêre omnes, intentique ora tenebant."

He divides his discourse into three parts:

I. *Chlorosis, Anæmia, Specifics.*

M. Trousseau had asked permission of the Academy to compare two diseases *always confounded,* chlorosis, and anæmia from abundant loss of blood.

"A young woman, imprudent [it is M. Trousseau who speaks], and little careful of sacrificing her health for a party of pleasure, takes a cold foot-bath in order to suppress her menses. In a few days she becomes chlorotic, not as in anæmia, because she has lost blood, but *because she has kept too much of it.*"

Such a chlorosis as this M. Bouillaud ridicules as a fabulous complaint. And we quite agree with him when he asserts, that though certain writers, in their desire for generic groupings of disease, may occasionally use language vague enough to give some countenance to M. Trousseau's statement, no such confusion as the one alleged exists in the minds of medical men between these diseases. Hæmorrhagic chlorosis, as well as the cachexia in secondary syphilis and in ague, may all give rise to anæmia, &c.; "but we have so many different maladies (*sui generis*) to deal with, wherein anæmia is merely one of the elements."

It is not, says M. Trousseau, from our pathological researches having taught us that certain morbid chemical changes exist in the last three of the complaints here named that specifics for them have been discovered in preparations of iron, mercury or iodine, and quinine. But these remedies have been hit upon empirically. However, as it does not appear that this clinical teacher absolutely denies that these specifics *may* cure by their chemical action, though the *modus operandi* may not be visible, we will pass over here M. Bouillaud's severe strictures upon the ultra-vitalists, who maintain that they prove efficacious by exciting the living tissues to proper action, and enter upon another of his topics.

II. *The True Relations of Chemistry and the Physical Sciences in general with Medicine.*

"It is not to-day, gentlemen, that, to my knowledge, our eloquent colleague has declared war against that which he calls the chemical treatment of disease. How often has he not spoken to me against the sort of excess with which we study, and we teach in our school the physico-chemical sciences, otherwise called the sciences accessory to medicine!"

We think that it cannot be denied that many of the theories of Liebig and others upon alimentation, respiratory and other elimination from the animal economy, &c. &c., have been shown to be wanting in proof, and that therefore it is wrong to teach them as unquestionable conclusions. But if there is reason to suspect that such theories involve much that is erroneous, it is much fairer to regard them as premature than as tending to mislead. In an article on "Man and Climate," in our last number, it was maintained that the balance of evidence continues, notwithstanding contrary affirmations, in favour of the well-known doctrine of Liebig, that fat is a main source of supply of animal heat; and this point is not only important to the physiologist, but to the practical physician. And even as regards the alleged inacceptability of chemical theories to account for the actions of specifics, we would suggest that these medicines nearly wholly consist of alkaloids—some tertiary compound oxidized—or are metallic, and are therefore prompt at chemical interchange of elements with binary compounds; and that therefore it is very natural to imagine that in the more complex chemistry of the animal frame they may neutralize or eliminate certain principles detrimental to healthy action in this manner, though we may not be able to "catch nature in the fact."

Granting M. Trousseau to be justified in doubting that, when a gouty patient recovers health under a course of mineral waters, this happens from alkalies chemically neutralizing the "uric diathesis," rather than from change of air, scene, and diet, we hardly agree that it is fitting, in consequence of like causes of doubt, to put the services of chemistry to medicine at a low value; for the list of articles in our pharmacopœia alone would show, at a glance, that we are under so great obligations to that science as amply to sustain its credit, though we might altogether fail in tracing the action of these things in the intricate reactions that take place in living organisms.

But M. Bouillaud makes quotations from his 'Essay on Medical Philosophy,' published a quarter of a century ago, showing that the forces of chemical affinity, as well as the physical forces in general, must act in living animals, and thus constitute fundamental studies in the knowledge of physiology and practice of medicine.

III. *On Vitalism, Organicism, and the Forces of Nature in General.*

Space would not permit us to follow M. Bouillaud through his critical history of the rise and progress of the natural sciences. We must confine ourselves to what appears to be his principal aim—to so much as indicates the universality of natural forces governed by laws. In the words of M. Biot, Sir I. Newton was the *creator of natural philosophy.* His thought that the force of gravity which drew bodies to the earth might extend to the moon herself was like an inspiration; and when he finally succeeded in demonstrating that it did do so, only diminished in amount inversely as the square of the distance between the moon and earth, and that it was it that "retained the moon in her orbit against the efforts of the *centrifugal force* which her movements of circulation excite in her," natural philosophy sprung into life. This exposition of M. Bouillaud's can scarcely be called accurate; for, without the force of gravity, the moon would not circulate in an orbit at all—she would move onwards for ever *in a straight line* with a uniform velocity through space. It is the force of gravity that deflects her at every instant from a straight path, and makes her *circulate* about the earth. And the first step in Newton's triumphs was the proof he furnished that the force of gravity, diminished in the ratio above stated, was precisely competent to pull her from the straight into the elliptical path she keeps. This is effected by a central or centripetal force, and centrifugal force is only an imaginary equal opposite force—indeed, but another name for it. Newton showed also that all the bodies of the solar system attract each other directly as their masses, and inversely as the squares of their distances, and that their movements, even to minute oscillations or secular variations, could be calculated from this supposition, and that the tides rise by this force residing in the moon and sun. We may subjoin that later observations upon some of the double stars detect that they move about each other, and that thus there is every reason to conclude that the grand force operates literally wherever there is matter.

Perhaps M. Bouillaud might have fortified his argument of the universality of the natural forces better by adverting to established facts than to Newton's dream, in his Optics of 1675, of an universal ether. It is certain that the heavenly bodies excite sensations in us of light and heat; and since, as was shown by Körner, we cannot calculate accurately the times of the occurrence of the eclipses of Jupiter's satellites except by allowing for the variable distance of the planet from the earth, and assuming that light travels at a certain rate, it appears that the undulations of light are propagated all the way from the planet to us.

Again, at the present time immense interest has been begotten in the scientific mind by lessons of the like universal character furnished by magnetic phenomena. Magnetic storms, or sudden great disturbances in the diurnal movements of the magnetic needle, have been observed to happen *at the same instant* at Toronto, Philadelphia, Hobarton, Pekin, the Kew Observatory—all the world over. These disturbances are accompanied by striking displays of aurora borealis; they run through their variations in a period of ten years, in which period the changes in the amount of spots on the sun's disc—that is, the series of perturbations in that body's luminous atmosphere, as observed independently by Schwabe — are also accomplished. The language of our veteran Brodie's late address to the Royal Society warms when he alludes to these manifestations of magnetism, "that mysterious force which, like the force of gravity, connects us with the sun, and probably with all other heavenly bodies—even those which are at the greatest distance from us."

M. Bouillaud having lingered so long over the universal force of attraction—*felix qui potuit rerum cognoscere causas*—feels obliged to glide very rapidly over the other natural forces. The physical forces, as manifested by "the imponderable bodies, such as light, heat, electricity, and electro-magnetism," and "elastic force or elasticity," as well as the forces of chemical elective affinity, are touched upon. It need not be repeated to our readers how these pervade the world. But we may mention that we have neglected to cite many ingenious comments of M. Bouillaud upon speculations of Descartes upon subjects that could not be successfully handled in his day, when the inductive philosophy had so small holdfast upon philosophers; for though we are under great obligations to the latter for his improvements in geometry, and even under some for his suggestions in natural philosophy (for instance, the law of refraction of light), yet he was a professed *à priori* reasoner, conceiving what physical laws *ought* to exist, rather than experimentalizing to discover those that do; and he actually led a powerful reaction against Bacon's inductive philosophy, which had already obtained influence. We have thought it more profitable to devote our limited notice to mark out the path of scientific discovery, rather than in registering the instances where science has gone astray. Though Bacon and Hooke professed to be *à posteriori* reasoners, their notions were crude or premature in the cases where their names are introduced.

M. Bouillaud now arrives at the *forces of living bodies—vital forces*

—and regrets the complexity of this last of the grand questions under discussion.

We can only succinctly indicate the tenor of his further remarks: Living bodies are made up of inorganic materials constituted into organs, and therefore they must be subject to the action of all the physical forces, though these must be regulated by others peculiar to the several organized structures. In early days it was fancied that a living man differed from his corpse in the possession of some *one* principle; as it was said of Prometheus (we may use the words of Parnell),

> "He carved the turf to mould a manly frame,
> And stole from Jove his animating flame."

We might cite, too, the breath of life in Genesis. Descartes invents or imagines a model of the human frame, in order to get which quickened, he merely wishes the Creator "to excite in its heart one of those fires without light, that he conceived not to be of other nature than that which heats hay put together before being dry;" whilst Stahl assumed the vivifying principle to be an immaterial soul. Some modification of this idea of a *sole* vital force was applied to the cases of the inferior animals and vegetables, if they were deemed worthy of notice. The transparent facts that matter in general is subject to many forces, the difficulty of marking the lapse between life and death, variations in the qualities of a given organic type, &c., compelled a general admission that even where we maintain that vital forces are different in kind from others, we must nevertheless use the plural, and speak of vital *forces* as operating in any individual. Some still adhered more or less strictly to the old idea of unity of vital principle, and discrimination having been made between the organic or vegetable functions of the animal and those that depend upon the will (and emotions?), Professor Lordat, a declared disciple of Barthez, talks of the vital force that rules the former as of an unconscious soul, whilst the real soul is displayed in voluntary actions. In the meanwhile, the nervous system begins to be interpreted, the lower functions have their proper nerves to manage them, and the mind is shown to be proprietor of the cerebrum. The last stroke in such analysis he celebrates by the exclamation, "glory to Gall, who the first so formally called attention to the special faculties of the understanding and the will!" An outburst, by the bye, that reminds us forcibly of fervent eulogies to be met with on the phrenologist in the writings of Auguste Comte, though the latter seems to relish his labours as much for his endeavours to locate the mental faculties in as many proper portions of the cerebral structure, as for defining them. But the views of the leader of the French *positivists* were regarded as having (theologically) a materialistic import, and thus it may be that M. Bouillaud's proclaimed horror of the very word materialism may have rendered him shy of mentioning his name in this and several instances in which the very language of that writer plainly peeps out of the text, in the sketch of the conduct of the natural forces from the simplest—gravitation—to the more concrete ones.

Furthermore, M. Bouillaud insists that psychological inquiries fall particularly within the province of the clinical practitioner, who must be convinced of the nobility of the mind of man, and of the soul manifested by it. He winds up by emphatically pronouncing that, however obscure be the nature of the vital forces "in the immense empire *of medical observation* and *experience*, in spite of the conquests of our predecessors, there remain rich harvests of truth to gather in: *imperium sine fine dedi.*"

Thus we have sat by whilst M. Bouillaud's photographic panorama of the Imperial Academy of Medicine has revolved, and watched the varying traits of emotion and reflection animating or fixing the features of his hearers, among whom are the teachers of the great medical school. Yet had our aim been confined to giving what he mainly aims at doing, a brief summary of the origin and the progress of general science as affecting the position of medicine, we are of opinion that we could have put our hand upon essays on the subject which afford more lucid sketches. Dr. Whewell had written the history of the inductive sciences much more clearly (not only more amply) long ago, and as a new edition of those writings with a new title and much additional matter (more particularly on medicine), has recently appeared, it may not be out of place to glance into this 'History of the Philosophy of Discovery,' to see if it may throw any new light on the grounds of the address we have been listening to. This work is permeated with the doctrine that science advances by a twofold process; by the close observation of facts aided by conceptions to bind them together, the registration and classification of effects and a search after causes. Not only is it necessary to be informed by the senses, but also directly by the mind, by ideas. Without attention and reflection, the accumulation of facts in the memory would make us no wiser. "Plato sought the essence of knowledge in ideas alone;" Aristotle, slighting this source of truth, looked to experience as the beginning of science; "and thus with these two great names began that struggle of opposite opinions which has ever since that time agitated the speculative world, as men have urged the claims of Ideas or of Experience to our respect, and as alternately each of those elements of knowledge has been elevated above its due place, whilst the other has been unduly depressed."

It is observable, however, that no disciple of either extreme opinion can help often using the language of those of the other, as the human mind must respond to its natural tendencies, which agrees with what M. Bouillaud has shown with respect to the vitalist Barthez and the organicist Bichat—namely, how either frequently writes like the other. Jumping from the ancients to the moderns,—Descartes produced great impression in France, especially as an idealist, though he often actually worked from observation; whilst Bacon may be esteemed as the real author of the inductive creed, and Newton and the physicists as its practical exponents. But first Hobbes and then Locke were the fathers of the sensationalists, who referred all knowledge to the senses. This school took strong root in France, until the theory received some modification from M. Lamoriguière ('Essai sur les Facultés de l'Ame'), who assigned *attention* and voluntary activity of the mind as a requisite, so that a reaction ensued to some extent. M. Auguste Comte gave another powerful impulse to the sensational school. He asserts that every man is a *theologian* in infancy, a *metaphysician* in youth, and a physicist in ripe age; that in the second state he searches after the causes of phenomena, and invents mere entities to account for them, and that the physicist or positivist confines himself to connecting phenomena by laws. We have observed that we may detect the secret influence of these lessons in M. Bouillaud's speech. But to return to the reaction against the sensational school; Leibnitz has said, "Nihil est in intellectu quod non prius fuerit in sensu; *nempe*," he added, "*nisi intellectus ipse.*" Finally, Kant propounded that the foundations of our reasonings are to be sought in the mind and not in the phenomena, and thus an extreme opposition to the doctrines of the sensational school first sprung up in Germany, and spread thence. It has been difficult to hold the balance between the two opposite yet correlative modes of gaining knowledge. Whewell declares in his preface, that no 'Cosmos' is complete from which the question of Deity is excluded; so that the general current of Bouillaud's thoughts moves very like his.

Grote* compares the practical turn of Hippocrates in medicine to that of Socrates in ethics; both eschewed the prevalent fashion of trying to hit upon some conception to explain how the Kosmos first began and how it continued to move on. Whewell remarks upon this:

> "There would be no better remedy for this ambitious error of the human mind than to have a definite subject of study, such as the diseases and the health of the human body. Accordingly, we see the study of medicine did draw its cultivators away from this ancient but unprofitable field.
>
> "These," subjoins he, speaking of the medical precepts of Hippocrates, "are marked by the prudence which practical study suggests to a calm and clear-sighted man. They can hardly be said to have opened the way to a science of medicine, for in the sense in which we here use the word *science*—namely, a collection of general truths inferred from facts by successive discoverers—we have even yet no science of medicine."

He has a word of commendation for Galen, as also for Harvey, the discoverer of the circulation of the blood, a contemporary of Bacon's, who, he tells us, put the antithesis between sensations and ideas with great clearness. "Universals are chiefly known to us, for science is begat by reasoning from universals to particulars, yet that very comprehension of universals in the understanding springs from the perception of singulars in our sense;" so that we gather that the medical body have gained some credit out of doors for putting the study of nature on a sound footing.

But once more, let us turn away from our chroniclers and critics, and come back to the word natural force, which has so enchanted the human mind ever since the days of Newton, that it hails the force of gravity as Cowley hails light:

* History of Greece, part 2, chap. 68.

> "Firstborn of Chaos, who so fair didst come
> From the old negro's darksome womb!
> Which when it saw the lovely child,
> The melancholy mass put on kind looks, and smiled!"

Every year since, the persuasion has been spreading wider and sinking deeper into our habits of thought, that not only is the quantity of matter unalterable, but also that of force is so. The Correlation* of Natural Forces is now a phrase of every-day occurrence; not only do we find it maintained that the physical forces are so related that a local change in one must also entail a proportionate change in the others; but even what are designated as vital forces* are assumed to reside within the circle of mutual dependence. Speaking of the whole catalogue of forces, we have the axiom that there is "conservation of force," an invariability of its absolute amount, in accordance with the observation just now made on the ordinary physical forces. This idea is even carried to the operations of the cerebrum in man.

We may even go further than this, and say that there are many writers who either declare, or scarcely conceal that they are tempted to avow, that we may seek in vain for evidence that the total of these forces has ever been otherwise than at present. Lyell, in his 'Principles of Geology,' does little other than affirm as much. Indeed, it is questionable whether he is not under the impression that the whole amount of life, vegetable and animal, has remained substantially the same as far back in time as geology has yet taken us. He elaborately points out how geological changes obliterate the records of life existing further back, which not improbably would have been essentially like those records that remain. Then again we have Darwin and others devoting their investigations to demonstrate that animals and plants may undergo almost indefinite modifications, so that any more modern example may well be a descendant of a very unlike predecessor. Hence the forces above spoken of must combine in action in extraordinary varieties of ways.

Sir B. Brodie congratulates the Royal Society that there is an ever-growing eagerness in general society "to become acquainted with natural phenomena and the laws that govern them," and upon the increasing opportunities of becoming so, "among persons of every class."

Here we would ask M. Trousseau whether, as every movement must have its reaction, he is really desirous in his iterations that too much science is taught in the medical schools, of stemming this flood of research and speculation, which whirls along as swiftly in our medical regions as anywhere? Whether he would be right or wrong in such an impulse, though the mass of the profession would be proud of such a leader as M. Trousseau, it would appear to us to be a desperate undertaking.

But what can there be at the bottom of his thought? Would he chime in, in behalf of patients, with Horace Walpole's compliment, "that after the doctors had talked nonsense for years, while we daily grew worse,

* See Nos. 3, 4, at the head of this Review.

the quacks ventured boldly, and have done us wonderful good"? Are empirics most commonly really justified by the event when they venture? We *have* fallen upon the writings of another man of our profession, one eminent in literature, who would fain stop the panting crowd stumbling over one another's bodies in the rough roads of science,—Dr. J. Brown, the author of the 'Horæ Subsecivæ.' He does not precisely deny that scientific knowledge is to be stored up, but pleads that a physician is more competent for his daily duties by being refined by letters, for being conversant with the thoughts and language of the great classics, for living, through their writings, awhile in the presence of such men as Locke and Sydenham, and becoming inspired with their feelings and tastes. A man, we agree, may improve his own mind and feelings in this way, but it is not so obvious that he would more readily understand the character of morbid action by devoting a large portion of his time to such seductive converse, than by the vulgar, straightforward way of prying into matters-of-fact, and reflecting thereupon. Yet we are willing to make the same sort of defence for Dr. J. Brown's opinions as we tried to do in behalf of chemistry against M. Trousseau's views as to our little obligations to it in medical practice. That Dr. Brown, primed full of such disquisitions as charm the reader in that volume, might prove a restorative in many a languishing patient's chamber, by his very presence, when physic would be useless, and its very mention poisonous, we are as certain of as if we witnessed the effect; and that the story of Rab and his Friends may have called a patient all but dead to life again, is a likelihood that we would not gainsay. So we take for granted, too, as to M. Trousseau's utterances on the subject, that were the whole meaning apparent, we should see that he is conscious of having some aptitude of intellect or disposition which he would find more agreeable to himself to develope further, instead of being compelled to journey onwards under regulations, offshoots of the notions of others, whilst at the same time his patients would be gainers by the liberty he would clothe himself with. Whatsoever may be his notion on this head, we recognise in him a clinical student of ardour second to none. We see no alternative but to move on with the throng.

Review III.

Guy's Hospital Reports. Edited by Samuel Wilks, M.D., and Alfred Poland. Third Series. Vol. VI. pp. 532.

The volume of the 'Guy's Hospital Reports' now before us is inferior to none of its predecessors. It contains seventeen original papers, with nine illustrative lithographic plates, and one woodcut. The first paper is entitled—

I. *On the Surgical Diseases and Injuries of the Nose, Larynx, Thorax with its contents, and of the Organs of Circulation.* By Thomas Bryant.—This is a very lengthened report of numerous cases which have come under notice at Guy's Hospital. The views expressed by the author can scarcely be regarded as original, but refer to many topics of great practical interest. Some interesting observations are made upon that very puzzling and distressing affection, ozæna. It is shown to be due to necrosed bone, to ulceration of the nasal passages, or merely to some constitutional derangement without any local disease. When there is no necrosis or ulceration, Mr. Bryant is of opinion that the offensive smell does not, as is generally believed, arise from any morbid secretion of the part, but that the fetor is the result of the decomposition of the retained mucus. In this case, all the treatment required consists in attention to the general health, the employment of steam inhalations and injections of warm water to remove the discharge, and the occasional use of weak medicated lotions. Ozæna with ulceration is to be treated on similar principles. Ozæna resulting from necrosed bone generally occurs in persons of a strumous constitution, or labouring under constitutional syphilis. In such cases, the appropriate remedies for struma or syphilis are to be combined with tonics and the local treatment already indicated.

Several cases are recorded of foreign bodies in the air passages, of œdema glottidis resulting from swallowing boiling water, of wounds of the throat, fracture of the ribs, &c. The following remarks in reference to the treatment to be pursued in cases of fracture of the ribs, with injury of the lung, may be of some interest at the present time. There are some surgeons who would treat such cases very differently, but perhaps not more successfully:

"Bleeding, as an operation, is now rarely performed; indeed, I believe that at Guy's Hospital it is rarer than any capital operation. In these cases of lacerated lung, however, when urgent dyspnœa makes its appearance, and the powers of the patient do not forbid it, I know of nothing which affords equal benefit, and which to the patient gives greater relief, or to the practitioner greater pleasure. Bleed with no sparing hand; let the blood flow freely in a full stream, and as it flows the symptoms will gradually disappear. When relief has been obtained, immediately arrest the flow. Your aim has been to make an impression through the systemic circulation upon the pulmonary. Watch your patient carefully, and repeat the operation if the symptoms should return, and, if necessary, repeat it a third time."

Although somewhat contrary to the present fashion, the above treatment appears to us to be most judicious, and best calculated to save the life of the patient. The reader is particularly referred to some excellent remarks by Mr. Humphrey, of Cambridge,* on the dangers of excessive stimulation by brandy, wine, beef-tea, &c., in the case of patients suffering from the effects of injuries and surgical operations.

An interesting case is recorded by Mr. Bryant, of laceration of the lung, the result of an accident, but unassociated with any fracture of the ribs.

The paper concludes with an account of a number of cases of wounds of the arteries and aneurism.

* Published in the British Medical Journal for Oct. 27th, 1860.

II. *Catena of Cases illustrating the use of Forceps in Extraction of Cataract.* By JOHN F. FRANCE.—In a previous volume of the 'Guy's Hospital Reports,'* Mr. France recommended the common artery forceps as the best instrument for steadying the globe of the eye, in the operation for extraction of cataract. Twenty-one additional cases are recorded in the present paper, in corroboration of the opinion previously arrived at.

III. *On some Diseases of Children.* By SAMUEL WILKS, M.D.—This is an able report of numerous cases of infantile disease which have come under the author's notice, and is replete with observations of great practical interest. Diseases of the head are first considered. In tubercular meningitis, or acute hydrocephalus, the character of the inflammation is described as of that kind which renders it at once recognisable, even should no tubercles be present. Tubercular deposits, however, are, according to Dr. Wilks, present in the majority of cases, and it is well to know that they are not often seen on the arachnoid, but are to be met with in the pia mater, especially in those parts which dip down between the convolutions of the brain. In cases where no tubercles are present, the appearances are effusion of greenish tenacious lymph at the base of the brain, in the neighbourhood of the optic commissure, and ventricular effusion with softening; and these lesions are, in Dr. Wilks's experience, invariably associated with tubercle in other organs of the body, but more especially in the lungs. The author's belief, indeed, is that tubercular meningitis is an affection mostly secondary to tubercle in the lungs. In the extensive field for observation which he has enjoyed at Guy's Hospital, he has never found tubercular meningitis without tubercle in the lungs. The history and post-mortem appearances of sixteen cases of tubercular meningitis are recorded, and compared with those of eight other cases at a more advanced period of life, to show the identity of the affection at different ages. Dr. Wilks alludes to the difficulty which frequently exists, of diagnosing between tubercular meningitis and typhoïd fever. There are few questions in diagnosis which embarrass the physician more than this. Two points of distinction are indicated, which are not very often referred to. One is the condition of the abdomen, which in tubercular meningitis is contracted, and in fever usually distended and tympanitic. Again, on the attempt being made to raise the patient's night-dress to examine for eruption, if the case be fever, the patient assists in drawing up the clothes; he is in a too senseless condition to heed any disturbances of the kind, and in his partial consciousness attempts to do what he sees is required. But in the case of cerebral disease resistance is made, and there are indications that any movement is annoying.

Tubercular disease of the brain-substance is regarded as a distinct affection from tubercular meningitis. In cases of the latter disease, tubercle is scarcely ever found in the brain itself.

* See British and Foreign Medico-Chirurgical Review, Jan. 1859, p. 168.

Simple acute meningitis is described as totally distinct from tubercular meningitis. In the former, the inflammation is much more acute and rapid, and the whole surface of the brain may be found covered with lymph effused beneath the cerebral arachnoïd. Dr. Wilks also distinguishes between simple *meningitis* and simple *arachnitis*. In the former, it is the cerebral arachnoïd which is inflamed, and the lymph, in place of being thrown out into the cavity of the serous membrane (as occurs in pleurisy, pericarditis and peritonitis, where the membrane is stretched tightly over the investing organs), is deposited in the sub-arachnoïd spaces, in consequence of the convoluted anatomical arrangement of the surface of the brain. True arachnitis, where the lymph is thrown out into the cavity of the arachnoïd, Dr. Wilks believes to result always from inflammation of the parietal layer, as he has never met with an example where it did not depend upon disease of the bone.

Some interesting observations are made upon the subject of chronic hydrocephalus, and one remarkable instance is alluded to of a little child, in whom, during an illness, the head became considerably enlarged, but subsequently declined to its original size. Hydatids of the brain are spoken of as extremely rare, for during the last fifteen years no single case has occurred at Guy's Hospital.

Under the head of laryngeal affections, some important remarks are made upon the subject of croup. Dr. Wilks's observations have led him to believe, that the membranous and catarrhal forms of inflammation of the larynx pass into one another, and are, in fact, identical. The membranous exudation is due not so much to any pathological peculiarity of the disease as to the anatomical structure of the air-tubes in children, and may be produced by any accidental irritation of the mucous surface, such as occurs in children from swallowing boiling water, instances of which are recorded in the paper. During life, Dr. Wilks believes that it is frequently impossible to decide whether a false membrane exists or not.

Tracheotomy, whether performed in croup, or for any object whatever, is shown to be an operation far less devoid of danger than is generally believed. One source of danger from tracheotomy is pointed out, which is not alluded to by authors—namely, emphysema. Three illustrative cases are recorded. In the first two the emphysema was confined to the chest, but in the third it was universal. In these cases pneumothorax appeared to result from air having been drawn into the chest from the neck.

From affections of the larynx, the author passes to pulmonary diseases. Admitting fully the distinction between lobular pneumonia and atalectasis, Dr. Wilks is of opinion that too much has been made of this newly-discovered morbid state, and that a simple collapsed condition in the bronchitis of children is less common than one associated with an inflammatory process.

Dr. Wilks has met with several instances where symptoms resembling those of hooping-cough have depended upon enlargement of the bronchial glands, involving the laryngeal nerves. It is well known, that an attempt has been made to account for the symptoms in all cases of true hooping-cough in this manner; but extended observation has shown that this explanation is not tenable. Indeed, the appearance is so exceptional in true hooping-cough, that Dr. Wilks seems to think that the presence of enlarged bronchial glands after death would render it somewhat doubtful that the affection had been hooping-cough at all.

Two cases of cancrum oris are mentioned, in which the liver was found fatty, which are thought to be of some interest, from the circumstance that the same morbid state of the liver is sometimes discovered in connexion with sloughing and gangrene elsewhere.

Lastly, there are the records of nine fatal cases of burn. In these cases, as well as in twelve others previously recorded,* there was no disease discoverable in the duodenum—a result which is somewhat remarkable when it is recollected that of twelve fatal cases of burn recorded by Mr. Curling in the twenty-fifth volume of the 'Medico-Chirurgical Transactions,' there was either inflammation or ulceration of the duodenum in all, as evidenced by inspection after death, and vomiting and purging during life. Dr. Wilks's experience leads him to think that, in fatal cases of burn, death is due to pulmonary disease, arising from a poisoned condition of the blood, such as occurs in pyæmia.

IV. *The Physiology of Sleep.* By Arthur E. Durham.—The author discusses with considerable ability the changes which the brain undergoes during sleep, and from experiments made by himself upon living animals under the influence of chloroform and other circumstances, he arrives at the following conclusions:

"1. Pressure of distended veins upon the brain is not the cause of sleep, for during sleep the veins are not distended, and when they are, symptoms and appearances arise differing from those which characterize sleep.

"2. During sleep, the brain is in a comparatively bloodless condition; and the blood in the encephalic vessels is not only diminished in quantity, but moves with less rapidity.

"3. The condition of the cerebral circulation during sleep is, from physical causes, that which is most favourable to nutrition of the brain tissue; and, on the other hand, the condition which prevails during waking is associated with mental activity, because it is that which is most favourable to oxidation of the brain-substance, and to various changes in its chemical constitution."

V. *Some Cases of Hydatid Disease.* By S. O. Habershon, M.D.—Thirteen cases are recorded, ten of which were examples of hydatids of the liver, one of the cellular tissue of the bladder, one where a hydatid was situated between the stomach and colon, and one where it existed in the pericardium. In the last-mentioned case the hydatid cyst was quite external to the heart, a fact of some importance in connexion with the doctrine, according to which hydatids of the heart are believed to reach that organ by the vena cava transporting them from the liver. Hydatids of the liver are shown to lead to death in various ways—viz.:

* See British and Foreign Medico-Chirurgical Review, July, 1857, p. 136.

1. By rupture into the pleura.
2. By rupture into the lungs and bronchi.
3. By rupture into the peritoneum.
4. By rupture into the intestine.
5. By rupture into the bile ducts.
6. By pressure on the vena cava.
7. By suppuration of the cyst and pyæmia.
8. By suppuration external to the cyst, consequent on its pressure.
9. By hæmorrhage, as in a case recorded in the 'Pathological Transactions' by Dr. Hillier, in which a cyst extended into the bile ducts, and also into a large hepatic vessel, causing fatal extravasation into the stomach and intestines.

In Dr. Habershon's experience, internal remedies do not appear to be productive of marked beneficial results upon hydatids of the liver, although in some cases diminution of the size of the cyst has followed the use of iodide of potassium. The various operative measures, which have been proposed, are discussed, and it is shown that, although sometimes they have been followed by success, they have frequently imperilled and destroyed the life of the patient. Although the method recommended by Leudet, of drawing off the contents of the cyst, and injecting solutions of iodine, chloride of sodium, and other substances, has been attended with considerable success, Dr. Habershon is inclined to think that the plan of drawing off small quantities of the fluid, so as not to admit any air, is the safest and most deserving of trial. Even this operation, however, he thinks, must only be resorted to after great deliberation.

VI. *Facts and Fallacies connected with the Research for Arsenic and Antimony, with Suggestions for a Method of Separating these Poisons from Organic Matter.* By Alfred S. Taylor, M.D., F.R.S.—This is a very lengthened paper, and is well worthy the attention of every one engaged in toxicological inquiries. Marsh's and Reinsch's methods are first considered in detail. Of the two, Dr. Taylor has found that a quantity of arsenic may be detected by Reinsch's process, which Marsh's process would entirely fail to reveal, even under its most improved modifications; while Reinsch's method is twenty times more delicate in revealing the presence of antimony than that of Marsh, as it is usually employed. There is a latent fallacy, however, connected with the use of Reinsch's process, which, curiously enough, no toxicologist of any repute had the slightest suspicion of prior to the trial of "The Queen *versus* Smethurst." This is due to the existence of arsenic in all the best, and so-called purest, varieties of copper, whether foil, gauze, or wire, in sufficient proportion to give rise, under certain conditions, to fallacious results. The subject of arsenic as an impurity of copper, is dwelt on at considerable length, and the best methods for detecting it are pointed out. In future, Reinsch's test can never be accepted as evidence in medico-legal investigations, unless the copper employed be first shown to contain no arsenic. The method recommended by Dr. Taylor for the detection of arsenic he proposes to call the *distillation process.* It consists—

1. In converting the arsenic into chloride. The suspected substance is treated with concentrated hydrochloric acid, and subjected to distillation. Any arsenic present distils over as chloride.

2. The chloride is converted into hydride by Marsh's process.

3. The gas, which distils over, is made to pass into a tube containing about a drachm of moderately strong solution of nitrate of silver. If arsenic be present, the solution of silver is blackened from the deposition of silver in the metallic form, the arsenic takes the oxygen of the silver salt and becomes *arsenious acid*, while the nitric acid is set free in the liquid.

4. If the hydride which passes over in Marsh's process be collected in strong nitric acid, the arsenic is converted into *arsenic acid*, which may be obtained by evaporating the nitric acid in a sand-bath.

5. By applying a strong heat to the conducting tube in Marsh's process, the hydride is decomposed, and metallic arsenic is deposited in a dark ring or crust on the interior of the glass, at a short distance from the spot which is heated.

For the details of this process the reader must refer to the original account; but it is obvious that it enables us to obtain the arsenic in three different forms—as metallic arsenic, arsenious acid, and arsenic acid. Several analyses are given in the paper, to prove the delicacy of the process.

VII. *Notes on Diabetic Cataract.* By JOHN F. FRANCE.—The author has collected from different sources about twenty cases of double cataract, occurring in patients suffering from diabetes. These cases are of no small interest in connexion with the recent observations of Dr. Weir Mitchell* and Dr. B. W. Richardson,† which show that, in several of the lower animals, the abnormal introduction of sugar into the system, whether by immersion of the body in syrup for a sufficient time for the osmotic process to take place freely, or by injection beneath the integument, is almost certainly followed by the development of lenticular cataract.

VIII. *Remarks on Two Cases of Extra-Uterine Fœtation.* By J. BRAXTON HICKS, M.D.—In one of the cases the fœtus was contained in the left Fallopian tube, and in the other it was imbedded in the wall of the uterus. A careful examination was made in these cases of the decidua-like membrane found in the uterus, and of the membranes enveloping the ovum, and from these examinations the author arrives at the following conclusions:

1. That the decidua-like membrane found lining the interior of the uterus in these cases is, in vascularity and microscopical structure, essentially the same as true decidua.

* American Journal of the Medical Sciences, Jan. 1860.
† Dr. Brown-Séquard's Journal de la Physiologie, &c., July, 1860.

2. That in extra-uterine pregnancies a decidual membrane can be dispensed with.

3. That the delicate membrane-like layer lining the cavity in some cases of extra-uterine fœtation, is composed of elements differing essentially from those of the true decidua, and is derived from plastic lymph, in all probability effused not long before the bursting of the fœtal cavity.

IX. *On Rupture of the Popliteal Artery and Popliteal Aneurism.* By ALFRED POLAND.—In this report, which extends over upwards of a hundred pages, the author has collected the records of sixty-nine cases of rupture of the popliteal artery, which have come under his notice at Guy's Hospital, or which have been published in the various medical journals. These cases are divided into three classes, viz.:

1. Cases of direct rupture, with complete laceration of all the coats of the artery.
2. Cases of direct rupture, with incomplete laceration of the coats.
3. Cases of indirect rupture of a popliteal aneurismal sac.

In the two first classes the coats of the vessel prior to the rupture are healthy; in the last they are diseased. The symptoms, diagnosis, and appropriate treatment for each of these classes of cases are considered at some length.

The following tabular arrangement shows the treatment pursued in the sixty-nine cases, with its results:

1. Cases in which no operation was performed: 6 cases and 2 deaths.
2. Cases in which compression was employed: 4 cases and 2 deaths.
3. Cases in which the popliteal space was laid open by incision, and a ligature placed above and below the rupture: 6 cases and 3 deaths.
4. Cases in which ligature of the femoral was employed: 27 cases and 11 deaths.
5. Cases in which amputation was performed: 38 cases and 15 deaths. Of these, 12 were examples of primary amputation, and of these 5 died; while 26 were secondary amputations, among which there were 14 recoveries, 10 deaths, and in 2 the result is not stated.

X. *On the Transference of Poisons from the Blood to the Alimentary Canal.* By ALFRED S. TAYLOR, M.D.—Physiologists and toxicologists have long recognised the fact that poisons received into the stomach and intestines, find their way by absorption into the blood, and are thence either temporarily transferred to the solid organs, or eliminated by the various excretions. The fact that, conversely, they might find their way into the stomach and intestines from the blood itself, when this fluid is the seat of poisoning, is a truth which is not commonly known, and has hitherto been only doubtfully accepted. Dr. Taylor records some experiments undertaken by himself and Dr. Pavy, which show clearly that arsenic, as well as antimony, may find its way into the stomach and bowels, although not taken by the mouth or injected by the rectum. From this it is argued, that the presence of *mere traces* of either of these poisons in the stomach or bowels is no proof of their *recent* administration. These substances, if administered a fortnight before, might disappear entirely from the stomach, and be partly absorbed into the tissues, from which they may be eliminated by the stomach and intestines, so as to appear again in these organs, quite independently of any fresh administration. Dr. Taylor points out the importance of these facts in reference to the recent case of antimonial poisoning at Liverpool (Regina *v.* Winslow), where antimony was found in the contents of the stomach up to the twelfth day after the last dose, and where, moreover, the elimination of the metal assumed an intermittent character.

XI. *Misplacement and Mobility of the Kidneys, in reference to the Diagnosis of Abdominal Tumours.* By ARTHUR E. DURHAM.—Mr. Durham has collected a number of cases to show the possibility of misplacement and abnormal mobility of the kidneys being mistaken for abdominal tumours. The occasional occurrence of congenital, fixed misplacements of the kidneys is familiar to every one who has had any experience in post-mortem examinations; but there are many who doubt if a fraction of the cases of moveable kidney lately diagnosed during life are really examples of this anomaly. The possibility, however, of the kidney being so moveable as to give rise to all the appearances of a moveable tumour during life, has been proved beyond a doubt by several post-mortem examinations. Mr. Durham has collected from different sources the details of ten such cases. In most cases of moveable kidney the patients have been females. When the kidney is misplaced, the corresponding supra-renal capsule occupies its normal position; and when the kidney is moveable, the supra-renal capsule does not move with it.

XII. *On Hermaphroditism, as illustrated (principally) by Specimens in the Museum of Guy's Hospital.*—By ARTHUR E. DURHAM.—Seven very remarkable cases of hermaphroditism are recorded, in some of which the individuals passed sometimes as males and at other times as females. The author concludes by observing that the study of cases of hermaphroditism affords important aid in the determination of the mutual homological relations which exist between the male and female organs, and submits the following plan as most in accordance with the cases on record:

Ovaries	=	Testes.
Round ligaments	=	Vasa deferentia not pervious.
Fallopian tubes	=	Vesiculæ seminales.
Sinus pocularis and openings into it of the common seminal ducts, in so far as they belong to the vesiculæ seminales	=	Vagina and cavity of uterus.
Clitoris	=	Penis.
Nymphæ	=	Prepuce.
Labia majora	=	Scrotum.

XIII. *An Account of Guy's Hospital Well.* By WILLIAM ODLING, M.D., F.R.S.

XIV. *Contributions to the Practical Surgery of New Growths or Tumours.* Series III. *Cysts* (continued). By JOHN BIRKETT.—This is a continuation of a series of papers by the same author on the pathology of morbid growths, which have appeared in previous volumes of the 'Guy's Hospital Reports,' and to which we have already on several occasions called attention.

Mr. Birkett's paper in the previous volume of the 'Guy's Reports'* was devoted to cysts of the cutaneous covering. The paper now before us treats of cysts of the connective tissue of the body, which are considered under four heads, viz.:

1. Cysts in the neck.
2. Cysts on the spermatic cord.
4. Entozoa cysts.
4. Cysts developed around foreign bodies.

Nine cases of cysts in the neck are recorded, and at the end of the paper there is a table of twenty-six other cases, collected from various sources. These cysts are either unilocular or multilocular. They may be developed at the earliest age, or may form in childhood and adult life. Frequently they attain a large size. Their commencement and subsequent growth are often painless; their origin and cause are most obscure. The elementary tissues of which they are composed, and their contents, are identical with those of the serous membranes. Their contents are sometimes limpid and colourless, and perfectly free from albumen; at other times, the fluid is pale yellow, and coagulates when heated, and in cases of long standing it may contain crystals of cholesterine. The treatment of such cysts is palliative or radical. The palliative treatment consists in removing the serous contents from time to time with a trocar and canula; the radical, in extirpation or obliteration of the cavity of the cyst. Extirpation by dissecting out the cyst is often attended with great difficulty and danger, and is only justifiable after all other measures have failed. Even exciting suppuration of the walls of the cyst, with the object of inducing obliteration, often gives rise to urgent constitutional disturbance; and in order to avoid this, Mr. Birkett recommends that the size of the cyst should first be reduced by repeated emptying, and that then inflammation should be excited by the external application of strong compound tincture of iodine, or tincture of cantharides, in place of injecting a stimulating fluid into the interior of the cyst.

The formation of cysts in the spermatic cord is attributed to the serous canal, which in the fœtus extends along the front of the cord, not being perfectly obliterated, small portions remaining in which the serous membrane preserves its integrity, and which afterwards become distended with serum. Two cases are recorded by way of illustration.

Three cases of entozoa cysts, or cysts containing the cysticercus cellulosæ, occurring on the external parts of the body, are recorded. In

* See British and Foreign Medico-Chirurgical Review, Jan. 1860, p. 55.

one of these the tumour was developed on the thorax, and in two cases in the tongue. Other cases are mentioned where a tumour containing ecchinococci was developed in the female breast. Cysts are sometimes formed around foreign bodies, such as pins or needles, and may occasion no small difficulty in diagnosis. A case is recorded where the foreign body was a small fragment of granite.

XV. & XVI. *On Poisoning by White Precipitate;* by ALFRED S. TAYLOR. *With the Physiological Effects of this Substance on Animals;* by F. W. PAVY.—White precipitate is not generally described as a poison by chemists and toxicologists. A trial took place at the Chelmsford Lent Assizes, in 1850, at which a woman was indicted for administering this substance to her husband with intent to kill him. She owed her acquittal to the lenient assumption that there was no evidence to show that white precipitate was either "a poison" or a "destructive thing."

Dr. Taylor proves that white precipitate is, without doubt, an irritant poison, and he gives the particulars of a trial which occurred during last year (1860), at which a woman was convicted of killing her child by administering it. Thirteen cases are added of poisoning by white precipitate in the human subject; but in all, with one exception, recovery took place.

Dr. Pavy's experiments show that the action of white precipitate on the lower animals is that of a powerful and purely irritant poison. It proved fatal to dogs, rabbits, and mice. The substance is rendered soluble by the acid secretions of the stomach, is absorbed and deposited in various organs. The kidneys are shown to be the chief receptacle of the poison, the liver containing a trace, and the heart none. The intestines retain the poison only in small quantity. Whether the quantity administered was large or small, or whether given in one or several doses, the quantity found in the body of the animal was in each case very small. Dr. Pavy's remarks are accompanied by a plate illustrating the remarkable effects of the administration of white precipitate, in repeated doses, on the structure of the kidney of a rabbit.

XVII. *Case of Ovarian Tumour containing Teeth, Hair, &c.; with Remarks.* By S. JAMES A. SALTER, M.B., F.L.S., &c.—There is nothing very remarkable in this case; but Mr. Salter has submitted the various tissues entering into the composition of the tumour to careful microscopic examination, and has shown that bloodvessels, bone, peritoneum, dentine, crusta petrosa, enamel, tooth-pulp, epidermis, sebaceous follicles, hair, &c., of a perfectly normal character, may exist in these ovarian developments. But the most interesting result of his investigation was the discovery of nerves in the tooth-pulp, nerves having as yet never been recognised in the contents of ovarian tumours, although a case is referred to where Mr. Henry Gray discovered a mass of brain in an ovarian cyst.

Such an aggregation of structures, adventitious to the individual in whom they are found, and situated in the essential generative organ of the female, can have but one meaning. They represent a fresh individual of a succeeding generation, though the anatomical form of that individual is not normal, and the life of the new mass is still dependent on the parent. Mr. Salter discusses the source of these curious developments, and maintains that they are not the result of normal conception, inasmuch as they have been discovered in the bodies of females with unmistakeable proofs of virginity, and even in girls long before the age of puberty. The question as to what calls them forth, Mr. Salter believes to be answered by the analogy which they have to the non-sexual reproduction, which is now known to prevail in many of the lower animals, such, for example, as occurs in the Aphis, the Daphnia, and the common Hive-bee.

It will, perhaps, ultimately turn out that the faculty of *agamogenesis* is latent in all animal life; and that, whereas in many of the lower forms it is the occasional or prevailing method for reproducing the kind, in the higher forms it is inefficient for that purpose, and is only exceptionally manifested as a physiological phenomenon. But however widely, or otherwise, this observation may apply, it seems to me that these ovarian developments in the human ovary are a manifestation of the phenomenon in question; that they are not analogous in any sense to other adventitious growths which the pathological anatomist meets with, but that they are altogether equivalent to *virgin-produced* '*zooïds*,' previous to their detachment from the parent."

Review IV.

1. *Leçons sur la Physiologie et l'Anatomie Comparée de l'Homme et des Animaux, faites à la Faculté des Sciences de Paris.* Par H. Milne-Edwards. Tome quatrième, Première Partie: *De la Circulation du Sang.—Paris*, 1859.

Lectures on the Physiology and Comparative Anatomy of Man and Animals, delivered at the Faculty of Sciences of Paris. By H. Milne-Edwards. Vol. IV., Part I.: *On the Circulation of the Blood.—Paris*, 1859.

2. *Recherches Hydrauliques sur la Circulation du Sang.* Par M. J. Marey. ('Annales des Sciences Naturelles,' Tome viii., 1857.)

Hydraulic Researches on the Circulation of the Blood. By M. J. Marey.

3. *Du Pouls et des Bruits Vasculaires.* Par le Docteur J. Marey. ('Journal de la Physiologie de l'Homme et des Animaux.' Tome deuxième.)

On the Pulse and Vascular Murmurs. By Dr. J. Marey.

4. *Recherches sur l'Etat de la Circulation d'après les Caractères du Pouls, fournis par un Nouveau Sphygmographe.* Par le Docteur J. Marey. ('Journal de la Physiologie de l'Homme et des Animaux.' Tome Troisième.)

Researches on the State of the Circulation, from the Characters of the Pulse, as furnished by a New Sphygmograph. By Dr. J. Marey.

5. *On the Forces of the Circulation.* Croonian Lectures delivered at the Royal College of Physicians. By PATRICK BLACK, M.D. ('The Medical Times and Gazette,' 1855.)

6. *De la Détermination Expérimentale de la Force du Cœur.* Par M. G. COLIN. ('Comptes Rendus de l'Académie des Sciences, 1858.)

Experimental Determination of the Force of the Heart. By M. G. COLIN.

IN a former number of this Journal,* we directed attention to some points in connexion with the circulation of the blood, but we were then unable to proceed further than to a consideration of a few of the more important phenomena which accompany the action of the heart, and the special subjects we discussed were the movements, the rhythmic action, and the causes of the sounds, of that organ. In returning to the subject of the circulation, we do so in order to complete more fully the sketch we then commenced, and because we believe that there remain many other topics in connexion with it which it is desirable to discuss at the present time, and inasmuch as it will afford us an opportunity of laying before our readers some of the results of the recent investigations of physiologists in this most important department of their science. Notwithstanding the labours which have been devoted to this subject, we are compelled to admit, when we come fairly to examine it in all its bearings, that there are many points in connexion with it on which sound principles are yet to be laid down, many facts which require further elucidation. For numerous reasons it is important from time to time to take a review of the various investigations that have been made, to consider any new facts that have been brought forward, any new doctrines that have been advanced, and surveying them from the vantage-ground we already occupy, and in the light of principles already established, to see whether our more enlarged observations and extended researches are paving the way for a more satisfactory explanation of the obscurer parts of the subject we are examining. It is impossible for any one who is engaged in the practice of medicine to do other than recognise the vast importance of a thorough knowledge of the phenomena which accompany the circulation of the blood, and of the principles by which it is regulated. We venture to assert that there are no facts in connexion with the whole range of pathological symptoms, especially those of acute diseases, which have so important a bearing on our diagnosis of the general condition of a patient, and so influence our treatment, as those which relate to the state of the vascular system. We cannot too highly appreciate the labours of the physiologist who endeavours to simplify our views of the circulation, to lay down correct general principles, and by the light he throws on the various processes which take place in the vascular organs in health, to give us an insight into, and enable us to explain, their condition in the various stages of diseased action. It can scarcely be said that there are any points relating to this subject which are not of interest and importance to the practical

* See Journal of April, 1860—art., "Cardiac Physiology."

physician, and which do not demand from him a careful investigation; but whilst we admit this, we must also confess how impossible it is for him practically to study the subject in all its ramifications; and hence we would gladly welcome from time to time, on this and kindred subjects, a *résumé* of the labours in which others have been engaged, with a general outline of the means they have employed, and of the principles they have established.

But if to the practitioner of medicine a knowledge of the principles which regulate the circulation of the blood is of such importance, how much more so does it become to those who are engaged in the duties of teaching in our various medical schools? We may safely say that the advance of rational and scientific medicine dates from the time when Harvey made his great discovery; and the improvements which this century has witnessed, especially in our means of diagnosing the various diseases of the heart and other circulating organs, have been in no small degree brought about by an improved knowledge of the functions of these organs. Of late years, the labours of British physiologists have not been so much directed to this subject as to some other departments of our science, and most of the facts to which we shall have to direct attention in the succeeding pages will be gathered from the writings of those who belong to Continental schools, and who in this, as well as in some other provinces of physiology and pathology, have worked more diligently and successfully than our own countrymen.

Although we may have in the course of this article to refer to some pathological facts which may have a bearing on the principles we may attempt to establish, we do not at all propose to enter into a general consideration of the morbid changes to which the bloodvessels are subject, and more especially shall we avoid any examination of those alterations which take place as the result of inflammatory action. We propose briefly to consider the nature of the capillary circulation, and to offer some remarks on the so-called capillary power, but we do not intend to enter into any questions connected with the pathology of the capillary vessels. Whilst, however, we omit these topics, we propose to refer to some of the modifications which inflammatory diseases, especially of an acute kind, produce in the force and frequency of the action of the heart, and in the tension of the arterial system. We are very strongly impressed with the importance of studying in a more accurate and scientific manner than has been previously done, the varying conditions of the pulse, and the indications they afford. We look upon this as essentially a practical question, and as having a direct bearing on the diagnosis and treatment of many of the more important diseases which come under our notice.

One of the questions which we meet with on the very threshold of the present inquiry is, as to the influence exercised by the heart on the movement of the blood. Are we to regard this organ as the sole propelling power which exists in the vascular system—as abundantly sufficient by the force of its contraction to circulate the blood? Or are we to consider, with some physiologists, that the influence it exerts is but small—that it plays a secondary part in the circulation? Or, again, are we, with a third set of inquirers, to attribute the *chief* force to this organ, admitting the existence of certain "residual phenomena," of certain other powers, which are constantly exerted in producing the onward movement of the blood? The inquiry is one on which a great variety of opinions will be found; and it is impossible to rise from a perusal of the writings of those who have made it the subject of their investigations, and from a consideration of the phenomena themselves, without a strong conviction that, with reference to the relative power exercised by the heart, the arteries, and the capillaries on the movement of the blood, correct principles yet remain to be established; rather, perhaps, from accurate reasoning on the data, both physiological and pathological, already acquired, than from a further repetition of experimental research.

The force exercised by the heart has been the subject of much discussion. To us it appears that the most important point to be established is, not as to the exact force the heart exercises, but whether that force is sufficient to propel the blood throughout the various parts of the vascular system, and bring it back again to the central organ.

We shall not dwell on the means that have been resorted to by different physiologists in estimating the force of the heart's action, which has been at different times considered as 180,000 lbs. and 5 oz. In mentioning the names of Borelli, Keill, Michelotti, Hales, Poiseuille, Magendie, and Volkmann, we shall recal to the minds of those who are familiar with our subject the theories these physiologists advanced. We think that Hales was the first who established, on a sound basis, a theory of the force exercised by the heart. Profiting by the errors into which Keill and Michelotti had fallen, he adopted the principle of measuring the heart's force by the height of the column of blood it would sustain. To effect this, he introduced into one of the arteries near the heart a long glass tube, and he found that the blood rose in the tube to the height of eight to nine feet in horses, to upwards of six feet in sheep, and of four feet in deer; whilst in dogs of large size it rose to the height of six to eight feet, and in those of small size, three to four feet. Hales next proceeded to ascertain the extent of the internal surface of the left ventricle, which he found, according to the plan of measurement he adopted, to be twenty-six square inches in the horse. Multiplying this by the height to which the column of blood rose, he arrived at the conclusion that the force exerted by the left ventricle of the horse, at the moment of contraction, was equal to support a weight of 113 lbs.; and by comparing together his results on various animals with the size of the heart in man, he estimated the force of the left ventricle of the latter at 51 lbs.

We have alluded to the investigations of Hales, the more especially because his conclusions have been somewhat invalidated by those of M. Colin, Professor of Anatomy at the Veterinary College of Alfort. We do not mean that his principles of investigation have been impugned by the latter gentleman, but only his results. M. Colin has found, by the plan he has adopted of taking the measurement of the internal surface of the left ventricle, that the estimate of Hales is too low; that, in fact, in an ordinary-sized horse there is an extent of surface more than double that given by Hales, and consequently there must be an amount of pressure also more than double; the latter M. Colin puts down at 236 lbs. instead of 113 lbs.

We shall not here enter into the consideration as to which is the more correct view of the two just given; but with regard to the plan of measurement, it appears to us that the one adopted by M. Colin ought to lead to a satisfactory result. Hales does not give all the details of his experiment, so that we are scarcely able to speak positively as to its value. His plan was that of pouring wax into the ventricle by means of a gun barrel inserted into a pulmonary vein, and measuring the surface of the wax when it was set. M. Colin injected plaster into the ventricle, and also measured it when it had set. He tells us that he took care to inject the heart whilst it was warm, and before rigor mortis came on. Hales gives no explanation on this point; and if his experiment were made during the time that the heart was under the influence of rigor mortis, we can easily understand that a smaller quantity of material would find its way into the cavity than under ordinary circumstances, and thus vitiate all his conclusions.

We are not inclined to attach very great practical importance to the estimate of the force of the heart given by different physiologists, and we believe that there has been a tendency rather to overrate the power it exercises. The well-known experiment of Dr. Sharpey, to which we shall again refer, proves that a comparatively small pressure is able to complete the circulation in a dead animal; and although it may be said that such an experiment as this does not prove that the same force is equal to the production of the same result in the living animal, yet we are disposed to think that more impediment to the onward flow of a fluid must exist after death than during life. If even we regard, as some physiologists have done, the power of the heart as almost entirely used for the purpose of dilating the arteries, so as to bring into play their elastic reaction, it is quite certain that, in this respect, the living artery would possess a great advantage over the dead, and that its walls would react with greater force on their contents; but if we consider that the power of the heart is partly lost in pushing on the blood, and partly in distending the arterial coats, we still think that the dead tube is placed at a disadvantage, and would require more force to circulate its contents.

A subject which is of more importance for us to consider than the total amount of force exercised by the heart in its contractions, is the pressure with which the blood, propelled by the ventricular systole, enters the arterial system. We shall have to consider this question more fully as regards the different parts of the system when we come to speak of the circulation in the arteries; but inasmuch as the pressure to which we now allude is essentially due to the ventricular contraction, we must take it into consideration at the present time. The study of the variations of which this pressure is susceptible will, we think, tend to throw light on the conditions which regulate its development. Hales, by his experiments, showed the height to which a column of blood would rise near the heart; but M. Poiseuille was the first physiologist who pursued the investigation of the subject in a systematic manner. To measure the force exerted, as we have stated above, physiologists have, of late years, made use of an instrument called a hæmadynomometer, which contains a column of mercury to be acted on by the blood, and the elevations to which the mercury rises in the tube indicate the amount of pressure exerted. The instrument we have just alluded to is much more convenient than the open tube used by Hales, and was first employed by Poiseuille; but, as made by him, it presented some imperfections, which were remedied by Magendie; and a description of it, as thus modified, will be found in a memoir by M. Guettet, in the 'Comptes Rendus de l'Académie des Sciences' for 1850. It now goes by the name of cardiometer.

In the investigation of the pressure exerted by the heart on the column of blood, in order to obtain the greatest accuracy, it would be desirable to insert the instrument into the base of the aorta itself; but such a course is attended with great difficulty, and the plan usually adopted is to operate on one of the carotids at the lower part of the neck. In examining the force of the heart in this manner, a most interesting and important fact comes out, and from the great uniformity of results which has attended the observations of various experimenters, there can be no doubt of the truth of the principle they have established.

It has been found that, in different individuals of the same species of animals, there is great difference with respect to the amount of pressure exerted by the left ventricle, and also in the same individual at different times; but that, comparing animals of different species with each other, a very great similarity and uniformity in this respect pervades all; that, in fact, the height to which the column of mercury will rise in the cardiometer is not regulated by the size of the impelling organ. Thus, M. Poiseuille found that the fluid in the hæmadynamometer rose to about the same level when he submitted to experiment dogs of different size, the hearts of which weighed respectively 83, 120, and 200 grammes. Again, he found that the pressure exerted by a horse's heart, which weighed 6 lbs., did not support a column of mercury notably higher than that which he sometimes found supported by the heart of a small dog. Other physiologists have made similar observations. M. Volkmann, even, has found that the heart of a fowl is capable of raising the mercury almost as high as is usually done by the heart of the horse.

Now, we must not confound the power by which the column of mercury is raised to the level we speak of, with the total force communicated to the blood as it leaves the left ventricle, or as it passes along the arteries. This force will vary in proportion to the size of the aortic orifice, which will bear a definite relation to the size of the heart.

We think that the fact of equal pressure exercised on a given column of blood, whether the heart be large or small, is one which has bearings of much practical importance. The researches we have alluded to show that the force exercised by the central propelling organ, the actual *vis a tergo*, does not vary with reference to each individual particle of blood, whether that blood be circulating in a large horse, and have to find its way *absolutely* to a considerable distance from the centre, or whether it be in the vessels of a small dog or even of a bird. The amount of force expended in the fowl to propel the blood which its ventricle contains, is the same as is expended by the horse to propel an equal quantity of his blood. Both would rise to the same level in an open tube.

Now, can we give any satisfactory explanation of this? Does it indicate that there is an unnecessary amount of power developed in the small animal? or are we to suppose that there are certain other powers which exercise a greater influence in some animals than in others? or is it that we find so little variety in the amount of pressure, because the force is required, equally in the small animal as in the large, to dilate the arterial tubes, and so bring into play their elasticity, in order to convert their intermitting stream into a continuous current? We incline to the latter view of the subject, for we see no satisfactory explanation of it on any other principle. M. Milne-Edwards seems to be of opinion, notwithstanding the evidence to the contrary which the experiments we have quoted appear to convey, that further researches may yet prove that some definite relation does exist between the amount of force communicated to the blood and the size of the impelling organ itself. He says:

"If instead of considering the absolute results produced by the play of the circulating pump, we examine in a comparative manner the force developed by this organ and the resistances which it is called upon to overcome to maintain the course of the blood in the vascular system, we see that there exists between the various mammifers considerable differences. Thus, in the rabbit, the utilized effect of the contractions of the left ventricle only exceeds by very little the resistances which are opposed to the circulating movement, whilst in the horse the excess of force developed by this organ of impulsion is very considerable. It appears to me probable that further researches will bring to light some interesting relations between the cardiac power and the size of the moving forces; but in the present state of science nothing can be affirmed in this respect." (p. 111.)

M. Milne-Edwards seems to base his opinion on the following statement made by M. Claude Bernard—viz., that he found in a rabbit he experimented on, that the cardiometer, inserted into the carotid, stood at ninety-five millimetres during the ventricular diastole, and rose five metres higher during the systole; whilst in a horse he found that the instrument stood during diastole at ninety-five, and rose during systole eighty millimetres higher. Now, it would appear from this experiment that the elastic reaction of the arteries, which is the force which keeps the cardiometer standing at the same level during the ventricular diastole, was the same in both the animals experimented on, but that the total force exerted on the blood by each contraction of the heart, was so much greater in the horse than in the rabbit, that it caused the column of mercury to rise seventy-five millimetres higher in one animal than in another. Such an experiment, considered alone, may favour the supposition of M. Milne-Edwards, but in face of the numerous ones of a contrary tendency to which we have alluded, it cannot be deemed as having much weight.

M. Claude Bernard has endeavoured to establish a distinction between what he calls cardiac pressure, and arterial pressure, and he bases his observations on the fact that, when the cardiometer is inserted into the left ventricle, the measure of the impulsion effected by the contraction of the cavity is at once obtained, and the moment the contraction ceases, the column of mercury falls to its original level; whilst, when the cardiometer is inserted into an artery more or less distant from the heart, the effects are much more complex, and during the diastole of the heart the mercurial column is maintained at a certain height, above which it is raised momentarily at every blow of the cardiac pump. M. Bernard calls the pressure on which the constant elevation depends, the arterial pressure, whilst that which momentarily raises the column to a greater height he calls the cardiac pressure.

As we shall have to dwell at some length on the pressure exercised by the arteries on the blood, in a subsequent part of this article, we shall only remark here that, unless we attribute some other power to the arteries, other than that which is usually attributed to them, we cannot admit that the pressure is due to them in the manner in which it appears to us M. Claude Bernard means. We entirely agree with M. Milne-Edwards, that the pressure is the result of the elastic recoil of the arteries, previously distended by the injected blood; but how is this recoil brought about? It is the result of the impulse of the blood from the left ventricle; the cardiac pump is employed in dilating the arteries, and is therefore the cause of the elastic reaction; so that the pressure which is exerted by the arteries, and which keeps the column of mercury supported in the intervals of the ventricular systoles, is brought about by the cardiac force, and must be considered as a part of the cardiac pressure.

We pause here briefly to consider a principle laid down by Dr. Black, in his Croonian Lectures, with reference to the function which the heart exercises. The following are his remarks:

"What, then, is the heart's function? Is it not to move the blood? No, not primarily. The force of the heart is to the movement of the blood what the muscular effort that winds up a timepiece is to the movement of the several parts. The force of the heart acts only on the elastic properties of the arteries, just as the muscular effort which winds up the clock does not primarily set the various parts in motion, but either raises a weight or acts upon a spring."

Again he says:

"If the heart's primary function be to move the blood through the arteries, then must its power be spent in producing this result, for a force which is productive of motion cannot at the same time be acting on a spring, or if it act on the spring, it cannot at the same time be a cause of motion. Whichever effect you admit, the power must be expended in producing that effect, nor will it remove the difficulty if we admit, as some contend, that the heart's power is divided between the two effects."

Now, we cannot agree either with the position which is here assumed, nor yet with the arguments by which it is attempted to be maintained. We hold that the function of the heart *is* primarily to move the blood, to impel it onwards throughout the vascular system, and that the action of the organ on the arteries is a secondary and not a primary function. We think that the principles which regulate the passage of fluids through elastic tubes out of the body, will apply to the circulation of the blood in the arteries within the body. We cannot see on what principle Dr. Black advocates his view, unless it be that the bloodvessels are constantly tending to contract on their contents and obliterate their calibre; but this appears to us by no means to prove that all the force of the heart is exercised in distending the vessels and bringing into play their elastic spring. As each supply of blood is propelled into the arteries, it must needs happen that the fluid will press equally in all directions, as well forwards as laterally, and thus an onward movement will be directly communicated to the fluid they already contain. We do not see the force of Dr. Black's argument in the latter paragraph we have quoted, that the power of the heart is directed to one object; we believe that the power is divided, in fact, we cannot understand how it can be otherwise than divided. Again, we must join issue with Dr. Black as to his view with regard to the flow of blood in the arteries being equally continuous, as in the capillaries and veins. We think that the experiments we shall hereafter allude to, with reference to the pulsation of the arterial tubes, will abundantly prove the difference of tension which exists in them at different periods, and we cannot see how, with an intermittent supply of blood, and with positive evidence of the existence of unequal pressure, there can possibly be an equably continuous flow. Further, we know that it depends on the condition of the arterial tubes whether the unequal flow of blood shall be sooner or later converted into an equal and uniform stream, for in certain states of the bloodvessels, the rhythmical action becomes perceptible in the capillaries and veins themselves, and these results are similar to others which may be produced in operating on tubes out of the body. We think, therefore, that there is no valid reason for rejecting the generally received opinion, that the flow of blood along the arteries differs in its character from that ordinarily witnessed in the other vessels.

We have not much evidence with reference to the influence produced by height and age on the cardiac force, but some observations made by MM. Poiseuille and Volkmann seem to point out the fact that, for the most part, the pressure is greater in proportion to the size of an individual; and with regard to age, that in adults the force is greater than either in the young or the old. There is another point on which we have experimental evidence of a most conclusive kind, entirely confirmatory of our theoretical views on the subject—viz., with reference to the effect produced on the cardiac pressure by the quantity of circulating fluid. We know the influence produced both on the rapidity and character of the pulse by the abstraction of blood, and the experiments of Hales, and the more recent ones of M. Colin, have shown how the arterial pressure diminishes in proportion as the blood flows from a vessel. The latter author has recently presented to the Academy of Sciences the results of his investigations, and in his essay there is a table, which is given in the work of Milne-Edwards, showing with what regularity the pressure developed in the arteries, by the contraction of the heart, diminishes as the mass of circulating fluid becomes less.

Other experiments show that it is not simply the quantity of blood which influences the cardiac pressure, but also the quality, for if instead of removing the blood, water be injected into the veins, the cardiac pressure immediately falls; and again, if any substance which excites the circulation—as a decoction of coffee—be used, a contrary result is produced, and the pressure increases.

M. Colin has investigated the relation of the cardiac pressure to the general forces of the body, and has shown that the relation between them is very intimate. The following are some of the results he obtained. In a very vigorous horse, the hæmadynamometer rose to the height of 2·70 millimetres; in another, very thin, to 1·85; and lastly, in another, extremely weak, to 1·60.

The principles which regulate the passage of the blood throughout the ramifications of the arterial system we believe are for the most part very well understood, and the means by which the intermittent stream received into these vessels is converted into a continuous current by the elastic reaction of the tubes on their contents, has been so frequently proved by actual demonstration on elastic tubes out of the body, and is capable of so easy explanation on the ordinary principles of hydraulics, as to render it unnecessary that we should dwell on it here. Of late some most valuable experiments have been made by Dr. Marey, bearing on this part of our subject, and we earnestly recommend our readers to peruse the papers of this able physiologist, the titles of some of which we have placed at the head of this article. These experiments of Dr. Marey serve to verify many of the principles on which rest the explanation of various physiological and pathological facts connected with the circulation.

Amongst other interesting facts, Dr. Marey has shown that in proportion as the elasticity of a tube is solicited, so does it become developed; and this is effected either by increasing the force of the impelled current, or the force of the latter remaining the same, by opposing an obstacle to the ready exit of the fluid from the tube, at the extremity opposite to that at which it enters. M. Milne-Edwards thus describes this experiment:

"In accelerating the play of a forcing-pump, or in varying the size of the obstacle which the terminal spout opposes to the escape, the elastic force of the walls of the tubes is seen to increase in proportion as their elasticity has been more solicited; that under the influence of a given effort this elasticity is more solicited according to the intensity of the obstacle to the exit, and that the transformation of the intermittent movement developed by the blows of the piston into a movement of uniform progression, is the more complete as the tube is the more easily dilated, and consequently that the elasticity of its walls has been less brought into play. Lastly, if the calibre or the length of the elastic tube employed in the construction of this hydraulic apparatus is varied, it is equally well shown that, other things being equal, the transformation of the intermittent movement into a uniform movement is the more complete that the parietal surface of the tube, the elasticity of which is brought into play, is more extended." (p. 175.)

We need not dwell on the important bearings which these facts have on the phenomena of the circulation. We can readily understand the influence which must be produced on the flow of blood in the arteries, and on the character of the pulse, when, from capillary congestion, or other cause, an impediment exists to the ready passage of blood into the venous system. We shall have again to allude to this in speaking of the pulse.

There is another fact which has struck us as worthy of observation, and as having a practical bearing, which distinctly comes out from the researches of Dr. Marey, and to which M. Milne-Edwards particularly calls attention. It has been long known that two tubes of a given size, one elastic and the other rigid, will deliver at their orifice the same, or nearly the same quantity of fluid when the current passing through them is determined by a constant pressure; but the case is far otherwise if the force be intermittent. The attention of physicists has not been sufficiently directed to this fact, but it appears from the experiments of Dr. Marey, which are of a most satisfactory kind, that when there is an intermitting impulsion, the pressure being equal in two tubes of the same diameter, one elastic, the other rigid, the quantity of liquid passing through the elastic tube is much greater than through the rigid one. M. Milne-Edwards thus explains this fact with reference to the circulation of the blood:

"The obstacle which is opposed to the free passage of the liquid put in motion by the cardiac pump, results principally from the frictions of the liquid against the walls of the vessels which contain it, and physics teach us that the frictions increase as the square of the velocity of the current. Now, the elasticity of the tube has the effect of retarding the movement given to the blood during the ventricular systole, and of continuing the movement during the period of repose; the displacement of a given volume of liquid is thus effected in a little less than double the time it would employ if the arteries were rigid tubes, and the rapidity of the current being reduced in the same proportion, the resistances must be diminished as the squares of the numbers representing the different velocities. Thus the extensibility and elasticity of the arterial walls play a very important part in the work of the circulation." (p. 176.)

We do not think it necessary to dwell at any length on the subject of the dilatation of the arteries during the systole of the heart. We incline to the opinion that it is impossible on the ordinary principles of hydraulics, to understand how the circulation could be carried on, how vessels resembling the arteries could receive the blood into them at each systole of the heart, without undergoing a dilatation as well as an elongation. Notwithstanding the opinion of Lamure, Arthaud, and Parry, that no such dilatation takes place, we think that the subsequent experiments of Flourens, Poiseuille, and others, have placed the fact beyond all doubt. We think that if any one will, whilst the circulation is vigorously carried on in an animal, place the finger and thumb in contact with the aorta near the heart, and (without compressing the vessel during the time of its systole) keep them in position during the ventricular contraction, he will no longer hesitate as to whether there is a dilatation of the vessel or not. We cannot attach much importance to the fact so strongly insisted on by Parry, that he could not *see* the dilatation even in large animals, and with the aid of a lens, whilst the elongation was distinctly visible. When we consider the shortness of the transverse diameter, as compared with the length, which the eye can embrace, and that the dilatation in the whole is but small, we need not be surprised at the result; and we must not conclude, because we cannot appreciate the dilatation by this means, that it really does not take place, but rather have recourse to other measures of a more delicate character to settle the question; and such it appears to us have been employed by the physiologists above alluded to, one of whom—M. Volkmann—has endeavoured to discover the relations which exist between the elongation and dilatation. His experiments have given the result that the lateral extension considerably exceeds the longitudinal; taking the former as one, the latter would be represented for the same space by 0·45 to 0·83, beyond which it never reached.

Aware that the pressure exercised by the blood as it enters the arterial system is considerable, physiologists have naturally proceeded to ascertain how far that pressure is maintained as the blood advances in the circulating apparatus. Were the arterial system a closed reservoir, it is clear that every fresh supply impelled into it would produce equal pressure in every part, and the manometric column of mercury would rise to the same level into whatever portion of the system it might be placed. But the arteries do not form a closed reservoir, and this fact modifies the phenomena we have just supposed. The investigations of Poiseuille on this subject led him to conclude that the pressure of the blood was the same throughout the whole of the arterial system, but subsequent experiments, performed with instruments of a more delicate character than those used by Poiseuille, have shown the error of his conclusions, and established the fact that the pressure diminishes from the heart towards the periphery of the arteries, and that the phenomena, which accompany the passage of the blood in its vessels, are analogous to those which are witnessed in the flow of water or any other fluid along a tube which is open at its extremity. In one of the works on physiology, a new edition of which has recently appeared, we find the statement of M. Poiseuille brought forward as an established fact, and no mention whatever is made of the able, precise, and more recent observations of M. Volkmann, who thus expresses a law with reference to this subject: "The pressure of the blood is subject to a gradual diminution from the commencement of the arterial to the termination of the venous system, exceptions to this law occurring only at points presenting congestion."

The law thus expressed holds good with respect to all fluids, in their passage through tubes. If, in an hydraulic apparatus, a number of vertical tubes, open at their upper extremity, be connected with a main horizontal tube, and a liquid be passed along this main tube, the liquid will rise in the vertical tubes, to levels diminishing in height, as the tubes are farther from the reservoir by which the whole apparatus is fed. This is supposing that there is a free exit for the fluid at the extremity opposite the reservoir; but if a stop-cock be connected with the extremity, and the size of the orifice of exit be made smaller than that of entrance, then the obstacle to the flow of the liquid will produce a rise in the vertical tubes (or piezometers, as they are called) near the extremity, and if the obstacle be increased, the fluid will stand at the same level in all the tubes.

In investigating the lateral arterial pressure, some physiologists have very closely imitated, in the living animal, the apparatus we have just described, and have introduced a piezometer, or vertical tube, into an artery through a small slit which they have managed to close perfectly, so that the circulation could go on without hæmorrhage. M. Volkmann has made use of another kind of instrument, consisting of a metallic tube, of the calibre of an artery, which is adapted by its two extremities to circular pieces, round which the two ends of the divided vessel are tied, and which has a lateral branch passing off at right angles.

Now, what appears to be the case with regard to the arterial pressure is this: that in the large vessels it is but little modified by their distance from the heart, but that as we approach the termination of the arteries and the commencement of the capillaries, the pressure undergoes a more rapid diminution; and it was from the examination of these smaller vessels that the error of M. Poiseuille was discovered.

But the pressure of the blood in the arteries is constantly undergoing a change, and especially so in the vessels near the heart. As each ventricular systole takes place there is increased pressure, and this is indicated by a rapid rise of the mercury. During diastole of the heart, however, the pressure diminishes, and consequently, the mercury in the manometer falls, but only to a certain level, at which it remains standing till the next ventricular contraction takes place. This difference in the height of the column at these two periods is sometimes very considerable, as we have before stated. Dr. Marey has shown, by a very ingenious experiment, the effect which is produced on the tension of vessels, through which fluid is passing, by a change in their diameter; and has thus illustrated the results which take place in the vascular system of the body, when an enlargement or dilatation of the capillaries is produced. If we have a tube with piezometers connected with it, in which tube there are three portions, of three different diameters, corresponding to arteries, capillaries, and veins, and fluid be injected through the arterial portion towards the venous, it will be found that the tension will be very great in the part corresponding with the arteries, and the level of the piezometers will there be high; but the level will fall rapidly in the capillary portion, and in the succeeding or venous portion the tension will be very feeble. If now the capillary portion be dilated, the other parts remaining the same, the level of the piezometers will immediately change. In fact, the tension will diminish in the first or arterial portion, it will be increased in the capillary portion, and so likewise in the venous.

We wish at the present time to make one or two observations on a subject we have already touched upon—viz., the distinction which has been sought to be made between the cardiac and the arterial pressure. Knowing what we do of the flow of the blood, of its intermittent supply at one extremity, and its constant and uniform stream at the other, and recognising the one as the result of the direct action of the heart, and the other of the elastic reaction of the arteries, we are not surprised that some physiologists, in speaking of the lateral arterial pressure, have alluded to the action of the heart as one force, and that of the vessels themselves as a totally distinct, separate, and independent force. Now, whatever influence the arteries may possess by virtue of their elastic property, in circulating the blood and in producing the arterial pressure, it is quite clear that this force is but a restored one, that it is all derived from the impulse of the heart; that it has been brought into play by the distension which the cardiac pump has produced, and therefore that the continuous flow of blood through the capillaries and into the veins, as well as the power which keeps the manometric column of mercury standing at a certain level, during the ventricular diastole, are both due to the action of the heart.

The influence produced by the respiratory process on the movement of the blood along the arteries, and in modifying the arterial pressure, has received the attention of numerous physiologists, and the general results which have attended their investigations have pointed out that the expiratory act has a tendency to aid the action of the heart in propelling the blood, whilst that of inspiration has an exactly opposite effect. When the chest is contracted during expiration, the pressure which is developed is exerted on the blood contained in the vessels within the chest, and tends to drive out their contents; whilst the inspiratory act removes the pressure, and tends to allow a larger quantity of blood to remain in the vessels. As the action of the respiratory apparatus and the cardiac pump are not synchronous, it will only be at times that the accelerating force produced by the contraction of the thorax, will coincide with the impulse given by the left ventricle, so as to increase its effects. At other times the expiratory pressure will contribute to promote the flow of blood in the arterial system during the diastole of the heart.

The most complete investigations that have been made on this part of our subject are those of M. Ludwig, who has employed for the purpose a peculiar kind of hæmadynamometer, to which he has given the name of kymographion. By placing one of his instruments in contact with the carotid artery, and another in contact with a little flexible sac, containing water, and insinuated beneath the walls of the chest, so as to be subject to the pressure resulting from the expansion of the lungs, he has been able to compare the effect produced on the arterial flow by the combined action of the respiratory process and the cardiac pump. In experimenting on horses, he found that when the respiration was tranquil, no appreciable effect was produced, either by expiration or inspiration, on the course of the blood in the arteries, but when the respiration was laboured, the effect of the systole of the heart was increased if it coincided with an expiratory effort, and during the period this act lasted, the diminution of pressure accompanying the diastole was lessened.

The knowledge of this fact seems to explain some pathological phenomena we occasionally witness; as, for instance, the rupture of an internal aneurism, following violent efforts of coughing or other muscular acts; for if, in any muscular exertion, the glottis be closed, as is very commonly the case, the pressure of the air within the chest on the blood contained in the aorta will be very considerable, and may be quite sufficient to produce a rupture of an aneurismal sac, the walls of which have become thinned.

Many attempts have been made to estimate the rapidity with which the blood flows in the various vessels of the body, but at present it cannot be said that we have attained to anything like scientific accuracy on the subject. The experiments of Keill, Hales, Hering, and more latterly of Poiseuille, must be familiar to all physiologists; and we deem it unnecessary to dwell on them. We must not, however, pass over the interesting researches of MM. Volkmann and Vierordt, which are of a more recent date. Physiologists have had recourse to microscopic observation to ascertain the rapidity of the circulation in the capillary vessels; but in dealing with vessels which are not transparent, or are placed in non-transparent tissues, any means of this kind utterly fails. M. Volkmann was the first to introduce a new method of measuring the velocity of the current of blood in the arterial tubes, and the instrument he has used for this purpose he calls a hæmadrometer. It consists of a small metallic tube, which is adapted to the two ends of an artery divided transversely, and which communicates laterally with a tube of glass bent in the form of the letter U. By means of stop-cocks the blood may be made, at the will of the experimenter, to traverse the glass tube, and the period it occupies in passing from one end of the vessel (the length of which is known) to the other, gives the velocity of the current. From the various observations made by M. Volkmann he deduces the following conclusions:

"The velocity of the sanguineous current is enormously greater than that observed in the capillaries.

"The velocity is greater in the arteries lying near, than at a distance from, the heart.

"The mean velocity of the blood in the carotid of the mammals hitherto experimented on is 300 millimetres (about twelve inches) per second."

The results obtained by Volkmann, with reference to the velocity of the arterial current, accord very closely with those M. Vierordt has arrived at by means of a process, essentially the same, but differing somewhat in its details, and as his investigations seem to be the latest on this subject, we shall give the description of the instrument he has used as we find it in the work of Milne-Edwards. He calls the apparatus a hæmatachometer, which

"Consists of a little narrow box with transparent walls, which has at its two ends orifices destined to be adapted to the two extremities of an artery divided transversely; it can thus be traversed by the current of blood which flows in this vessel, and it encloses a small pendulum, the free extremity of which, struck by the current, is raised more or less, according to the rapidity with which the latter is endowed. The opposite extremity of the rod of the pendulum sets in motion a needle, which, with the aid of various intermediate organs, moves in a corresponding manner a pencil, before which turns with an uniform movement a cylinder carrying a slip of paper, very much as the registering kymograph of M. Ludwig. This instrument is very sensitive, and after having been properly graduated, indicates with much precision the rapidity of the current which traverses the box." (p. 244.)

The estimate given by M. Volkmann of the quantity of blood expelled from the left ventricle at each beat is very large—6·2 oz.; and taking the duration of the pulse at 0·85 second, he calculates the period of the circuit to be 67·5 seconds.

This estimate shows a duration much greater than that which the experiments of Hering and Poiseuille would seem to indicate, but we think it is doubtful how far the method of experimenting of these latter physiologists, which consisted in the injection of prussiate of potash, &c., can be relied on. The experiments alluded to below show the influence produced on the rapidity of a current by the introduction of different materials into it, and until we know the power of diffusion which salts like prussiate of potash and others possess, we cannot tell how far their detection at a part of the system, distant from that at which they were inserted, after the lapse of only a few seconds, may be due to the influence of this power of diffusion, rather than to the mechanical conveyance by the blood itself.

MM. Volkmann and Poiseuille have found some singular results on the rapidity of the circulation, by the injection of certain materials into the blood, and the same results have followed the introduction of the materials into a fluid circulating in artificial capillary tubes, and in the vessels of a dead animal. Thus it was found that a solution of nitrate of potash increased the rapidity of the circulation, whilst alcohol diminished it; and this latter substance had the most powerful effect of any in retarding the flow of blood. In horses, when the normal velocity of the circulation was from twenty-five to thirty seconds, the injection of alcohol reduced it to from forty to forty-five seconds. We have been accustomed to consider alcohol as a powerful stimulant to the circulation, and undoubtedly it does in many cases increase the frequency of the heart's beat, but this increased frequency affords no proof, *per se*, of increased rapidity of the circulation, for this depends not only on the frequency of the heart's contractions, but also on the quantity of blood which the ventricle at each beat expels.

Now, considering the facts we have mentioned above, that, whether we inject alcohol into the vessels of a living animal, or into the vessels of a dead one in which we are producing an artificial circulation, or again, into inert tubes where we are performing a similar process, the result is a retardation of the flow of the circulating material, we cannot come to any other conclusion than that the effect is due to certain hydrodynamic influences, and it becomes important to consider the physiological action of alcohol in the light of this principle, and to endeavour to ascertain whether any of the effects it produces, when taken into the system in the ordinary way, may be explained by a reference to it.

We quit here the subject of the rapidity of the circulation, to examine briefly a principle with reference to the force of the heart and of the arteries in circulating the blood, which was advocated by one of our own most distinguished neuro-physiologists, Sir Charles Bell. He was led to suppose that, in the instance of the fluid blood and the solid tubes through which it passes, there was an absence, a negation of the ordinary attraction which universally exists with respect to dead matter; and that the circulating organs did not possess the force necessary to press on the blood, provided that the latter meets with the ordinary resistance to its progress, which in the presence of a mutual attraction between this fluid and the solids of the body we should naturally expect. He endeavoured to explain the arrest which takes place in the flow of blood through a divided artery, on the principle that, under these altered circumstances, the law of attraction comes into play, producing coagulation of the blood and a plugging of the vessel.

Although the principle thus advocated by Sir C. Bell has not received the sanction and support of subsequent physiologists, we think it worth while to dwell on it for a few moments. We believe that the power of the heart has been by many physiologists very much overestimated; and again, that the resistance which the blood meets with in its progress has by others been equally over-estimated. The experiment to which we have alluded, which shows that the arterial pressure diminishes in proportion as the vessel examined is distant from the heart, proves sufficiently clearly that an impediment does exist, and this must be in the mutual attraction between the arterial walls and their contents. Were this attraction removed, as Sir C. Bell believed it was, no impediment would exist to the onward flow of the blood, no difference would be recognisable in the arterial pressure in the various tubes, and the blood would rise to the same level in the piezometers, whether placed at a distance from, or near to, the heart. This, however, we do not find; and we are therefore for this, amongst other reasons, compelled to reject the hypothesis of Sir C. Bell. We are of opinion, however, that in the arterial system the resistance to the course of the blood, produced by friction of the latter against the walls of the tubes, is reduced to a minimum—that from the perfectly smooth character of the inner surface of the arteries, but little opposition is presented to the circulating current.

In considering the passage of the blood through the arteries we naturally refer to the subject of the pulse; a phenomenon of a highly complex character; not made up of a simple impulse from the heart, although essentially depending on it; not due to any active contraction, nor even to any active force of the vessel itself, although its character is in great measure due to the condition of the arterial coats.

It has never yet received that scientific consideration to which its importance entitles it.

We think that the study of the pulse offers a field of labour that will admit of successful cultivation; the establishment of a few sound principles on the subject would enable us to communicate to others a knowledge which can now be only acquired by prolonged clinical experience.

Next to the question of the sounds of the heart, the nature of the pulse, and the causes which produce it, have probably given rise to more theories than any other phenomenon in connexion with the circulation. Thus Galen and Harvey attributed it solely to the dilatation of the arteries. Weitbrecht and Bichat maintained that it was due to the displacement of the vessel. Arthaud thought that it resulted from the effort of the blood against the obstacle produced by the depression of the artery. Parry considered it due to the elongation of the vessel. M. Flourens looked upon it as a complicated phenomenon combined of all the elements which concur to produce a pulsation of the artery, an opinion shared in by Haller and Soemmering; Weber and others have attributed it to an undulation of the sanguineous fluid. The most recent experimenter on the subject is Dr. Marey, who has endeavoured to show experimentally the part which is due to the depression of the vessel by the finger, and to explain the mechanism of the movement.

Although Galen and Harvey attributed the pulsation in the arteries to their dilatation, the opinions they held of the cause of the dilatation, and of the way in which the pulse was produced, were diametrically opposed. Galen considered that the dilatation of the arteries coincided with that of the heart, and he believed that the pulse was due to a force residing in the coats of the artery, and communicated to them from the heart itself. The experiment he made use of to prove his point was this: having exposed an artery, he opened it longitudinally, and having placed in it a pervious tube, he tied the vessel in the tube; he found that the pulsation ceased beyond the ligature. Harvey pointed out some of the sources of error in this experiment, but there is one which he seems to have overlooked—viz.; the fact that there is great probability of the blood coagulating in the tube. Harvey further pointed out the fallacy of Galen's statement which was then universally prevalent, that the dilatation of the arteries was coincident with that of the heart, and experimentally proved that the contrary was the case, and that the systole of the ventricles takes place at the same time as the diastole of the arteries.

If, however, we are to believe the authenticity of a work which was translated by M. Daremberg, and published for the first time in 1846, the truth with respect to the fact just stated was known long before the time of Galen. The work to which we refer is attributed to Rufus of Ephesus, and is a 'Treatise on the Pulse,' written in Greek.

A knowledge of the influence produced by different kinds of tubes on the character of the impulsion felt in them, when liquids are propelled along their course, serves to explain some of the varieties of the pulse we meet with under different conditions of the system, the contractions of the central propelling organ remaining the same. Thus, if the walls of a tube are depressible without being sensibly elastic, the pulsation is short, if the impelling blow is sharp and short; it is strong, if the blow is powerful, and it is distinctly felt at the same time in the entire length of the tube. But if a tube with elastic walls is used, the pulsation is modified in proportion to the distance of the vessel from the impelling organ; it becomes most distinct, or in other words reaches its maximum (a point we shall have to explain further on) later, as the distance is greater. If we examine the effects which are produced on the arteries, when conditions more or less similar to those we have above described, exist in them, we find the same characteristics in their beats as we meet with in the pulsations of inert tubes. Thus, if there be extreme tension in the arterial system, bringing the vessels more into the condition of hard, rigid tubes, the pulse in the extremities will be felt almost exactly synchronously with that in the vessels near the heart. Whilst, on the other hand, if there be diminished arterial tension, the pulsation in the extreme vessels will be apparently longer delayed than usual. The other characteristics of the pulse which are due to the yielding or unyielding condition of the arterial walls, we shall have to allude to farther on.

An attempt has been made by some physiologists of Continental schools, and especially recently by Dr. Marey, to deal with the question of the pulse in a scientific manner; we shall lay before our readers some of the chief points to which he has directed his attention, and the conclusions to which he has arrived.

Dr. Marey first attacks the view which is held by most German physiologists, and amongst others by Volkmann, that the flow of blood through the arteries takes place in a series of waves, which become obliterated in proportion as they advance towards the periphery of the vessels. This wave-theory has been supposed to account for the delay which is believed by most authors to take place in the pulsation of an artery, at a distance from the heart, as compared with one near it. The existence of a sensible interval between, for instance, the pulsation in the aorta, and that in the dorsal artery of the foot, was, we believe, first remarked about the middle of the last century, by Weitbrecht and Senac. Haller threw some doubt on the fact, but the observations of Rochoux, Carlisle, Weber, Hamernik, and others have now established beyond all doubt that there does exist, at any rate, an *apparent* delay. M. Weber has estimated the difference between the time of the ventricular systole, and the pulsation in the arteries of the foot, at one-seventh of a second. Dr. Marey denies that any *real* delay takes place, and states that the pulsation *begins* at the same time throughout the whole arterial system. He says that physiologists have fallen into the mistake of supposing that there is a real delay in the pulse in different arteries, in consequence of the fact that, although the pulsation begins at the same time alike in all, it does not reach its maximum as soon in the distant vessels as in those near the heart. It is this maximum pulsation, or in other words, the condition of greatest tension, which is perceptible to the touch, and which varies in different parts of the arterial system. By way of illustration, Dr. Marey makes use of an instrument he calls a sphygmograph, which was first invented by M. Vierordt. Every one is aware that the pulsation of the artery in the ham is capable of producing a movement of the leg and foot, when one leg is made to rest on the opposite knee. Without dwelling on the cause of the phenomenon, it is to the observation of it that is due the invention of the sphygmograph, which consists of a lever which is raised by the pulsating vessel, one end of the lever being kept in contact by graduated pressure with the vessel itself, the other being so arranged that it shall trace on a cylinder, which revolves by the aid of clockwork, the curves produced by the lever as it rises and falls during the diastole and systole of the vessel. The pulsations may be amplified to any extent the experimenter may desire by elongating the lever. Dr. Marey has introduced some modifications into this instrument, which have made its indications more accurate.

When the action of the heart is imitated out of the body, and a liquid is injected at regular intervals into an elastic tube, which is so curved as to be brought into relation with three levers, or sphygmographs, fixed on an upright support, there being a length of tube equal to a metre, or somewhat more than a yard, between the points at which the levers are brought in contact with the tube, it is found that at each injection the levers are thrown into action, and if the rotating cylinder be at work, they will mark on it certain curves, as they rise and fall, and thus indicate the exact period at which pulsation begins, the time at which it reaches its maximum, and its period of decline. The paper which receives the markings of the sphygmograph is graduated by means of vertical and horizontal lines, the former serving for the valuation of the duration of the movements, the latter of their extent. A paper of this kind, which has been marked by the acting sphygmograph, presents us with a line for each lever, consisting of a series of curves, which will vary both in their height and length, in proportion as the pulsations which produce the movements of the lever are strong or feeble, prolonged or rapid.

If we inspect a paper which has been marked in the way we have just described by an hydraulic apparatus similar to the one alluded to above, we observe the fact stated by Dr. Marey:

"That *all the pulsations commence at the same time*, only they are not complete at the same instant. It follows from this *that there is no real delay in the pulse*, but only a delay for the maximum of the pulsation. Now as, according to the delicacy of our touch, we perceive the arterial pulsation at a moment more or less near to its commencement, all observers do not find an equal delay, because they have not a tactile sensibility equally developed, and that the delay is not a real quantity."*

We think there can be no doubt that the same phenomena which are witnessed out of the body, in the apparatus used by Dr. Marey for his experiments, take place in the arterial tubes, and that there is no delay throughout these vessels in the commencement of the pulse, but only a delay in the period at which the pulse reaches its maximum. It becomes important to consider how this result is effected, and the

* Journal de la Physiologie, &c., tome ii. p. 267.

following explanation is given by Dr. Marey. As the ventricle propels at each contraction a quantity of blood into the arteries, a part of the force thus employed is expended in pushing onwards the column of blood, and a part in dilating the arterial tubes. It is quite clear that this force will become diminished in intensity, the farther we pass from the impelling organ, and that the shock, simultaneously communicated throughout the whole of the arterial ramifications, will be the more feeble, in proportion as the point at which it is examined is more distant from the centre; and as the direct shock is diminished, so will the dilating power of the current be less, until at length both will become infinitely small.

The direct impulse being thus reduced, its power of producing a pulsation perceptible to the touch will be reduced also, and hence, Dr. Marey says, it happens that, under ordinary circumstances, we do not perceive it, for, as he endeavours to show, the pulse in the more distant vessels, although it owes its commencement to the impulse from the heart, yet is indebted for its maximum intensity to the elastic reaction of the coats of the arteries which precede it. At the moment the blood is impelled by the left ventricle into the arteries, the tension of these vessels is less and less, in proportion as they are more distant from the heart; but a modification of this condition immediately ensues: the first part of the arteries has reached at once its maximum of tension, and immediately begins to react, the force of its reaction is communicated to the blood, which is thus pressed onwards; and in proportion as the vessel is situated at a distance, and so receives but little of the force of the impulsion, so will it receive more of the restored force from the reaction of the vessels which precede it—which vessels will continue to send on the blood until they have got rid of all that their dilatation contained, or, if the entrance to the tube is closed, till there exist a perfect equilibrium of tension in all the parts. Dr. Marey thinks that this serves to explain the peculiarity of the curves formed by the sphygmograph in the hydraulic apparatus he has used. Near the impelling organ the ascent of the lever is rapid, and the descent slow, whilst at a distance from it the ascent increases in duration in proportion to the distance. The height to which the lever rises shows the force of the tension on the tube, and it is found to reach a higher level the nearer it is placed to the impelling organ.

The theory of the pulse which was advanced by E. H. Weber is probably well known to our readers, but as it bears a strong resemblance in some points to, although essentially differing in others from, that of Dr. Marey, we think it right briefly to allude to it. According to Weber's view, the blood impelled by the ventricle into the aorta first distends the vessels which are nearest the heart; these by their elasticity react on their contents, and cause the distension of the next portion of the arterial system, which in turn performs the same function, and thus the blood is forced on from part to part, so that a certain interval elapses before the kind of undulation of the blood, which is thus produced, reaches the most distant part of the system.

The pulse is, in this view, considered as the result of an undulation produced by the pressure of the blood on the aorta, and thence propagated along the walls of the arteries, and along the blood, by the elastic reaction of the vessels.

An objection was long ago raised to the wave theory as advanced by Weber and others, and in the 'Medical Gazette' for 1845 a communication on this subject will be found from an anonymous correspondent, and another from Dr. Thomas Williams, which merit our attention at the present time in connexion with the theory of Dr. Marey we have just considered. Dr. Williams claims, with reference to the theory advanced in the communications, a priority of authorship.

In combating the wave-theory of the pulse, the anonymous author points out the fact that the pulsation in all the arteries, even the most remote, is perceived previous to the occurrence of the second sound of the heart, or in other words, before the closure of the semilunar valves, and he maintains that if the pulse were due to the elastic reaction of successive portions of the arterial tubes, it ought, except in those vessels whose pulse is dependent on the first wave of blood, to follow the second sound of the heart, for no arterial contraction could exert any force in aid of the circulation until after the closure of the aortic valves. He admits that a delay takes place in the pulse of the vessels of the foot, as compared with the carotid, of one-sixth to one-seventh of a second, and the theory he advances of the pulse is as follows:

"1st. The cause of the pulsation felt in the arteries is their progressive and universal distension by the blood expelled by the left ventricle.

"2nd. This distension is due to the direct force of the left ventricle; not to that force transmitted by the successive undulatory contractions of the arterial walls.

"3rd. This distension still partakes of the character of one long wave, in so far that the first part of the arterial tube is necessarily dilated before the middle and last parts; but still the dilatation of these middle and last portions is not due to the contraction of those portions of the arterial tube which are nearer the heart than they are."

We are quite satisfied that the statement made by the author of the above theory, with reference to the pulsation in the extreme arteries being perceptible before the occurrence of the second sound of the heart is correct; but the question appears to us to be, whether it has reached its maximum before the closure of the valves is accomplished. We are of opinion that the fact, as stated by Dr. Marey, of the simultaneous commencement of a pulsation in the whole length of an elongated tube, must be considered as applying to the arterial system, and it appears to us to afford convincing evidence, the most complete that has ever been given, of the incorrectness of the wave-theory as advanced by Weber.

There can be no doubt, from the experiments of Dr. Marey, that when fluid is entering an elastic tube by intermittent pressure, some parts of the tube have passed their period of greatest pulsation, and are reacting on their contents, whilst others have not reached their maximum tension, and thus the orifice of entrance to the tube being closed so as to prevent any backward flow, the elastic reaction is constantly pressing the fluid onwards towards the open extremity. If it could be proved that, with regard to the more distant arteries, they do not reach their maximum tension, until coincidently with the occlusion of the semilunar valves, the theory of Dr. Marey would, we think, be unexceptionable; for it seems to us that the elastic reaction must influence the distant arteries immediately it commences, and therefore, before the valves are shut; the reaction which causes the valves to shut will also produce a pressure of the blood into the distant vessels. We are, however, disposed to think that even in the extreme arteries, their maximum distension is reached, and their reaction has commenced before the aortic valves have closed. This question might be decided if we could place an instrument so as to indicate the period of closure of the valves, and another to show the time at which the distant arteries reach their greatest pulsation, for it may perhaps be said that palpation on the one hand and the ear on the other, scarcely constitute a perfectly safe tribunal to which to appeal for a satisfactory solution of this question.

We quite agree with Dr. Thomas Williams in the paper we have referred to, that it is impossible to assign a limit to the immediate current propelled by the heart, or in other words, to *the first wave* supposed to exist by those who admit the undulation theory; and this, we think, affords a strong argument against Weber's view. We believe in the simultaneous shock communicated throughout the entire arterial system, but we confess we see a difficulty in admitting the whole of Dr. Marey's conclusions.

The preceding remarks will show how much the character of the pulse depends on the distance of the vessel, in which it is felt, from the central propelling organ, and on this point Dr. Marey lays down the following laws:

1. The amplitude of the pulsation decreases in proportion as we pass from the orifice at which the fluid enters.

2. The duration of the increase of tension increases in proportion as we approach the orifice at which the fluid escapes.

Experiments on the effects produced by the elasticity of vessels on the character of their pulsation, serve further to explain many of the conditions of the pulse which are met with. The following experiment on this point may be quoted:

"If during winter we operate on a tube of caoutchouc, it will have, in consequence of its being hardened by the cold, very little elasticity, and the pulsation will preserve its initial character of short expansion to a considerable distance. If now we pass hot water into our apparatus, we shall see the pulsations become modified by degrees, and when the tube shall have resumed its suppleness, they will have the characters we observe in a point far distant from the orifice of entrance."*

An effect of a similar kind may be produced by increasing the surface of the tubes by an elastic dilatation, and if such dilatation is sufficiently large, it may obliterate the pulsation beyond it, as is sometimes seen in cases of aneurism, in which the loss of pulsation could not have been due to the blocking up of the vessel with which the aneurism was connected, and must therefore have resulted from the

* Journal de la Physiologie, &c., tome ii. p. 279.

force of the heart having expended itself in the dilatation of the cavity, whilst the fact that the circulation continued negatively proves the effect that is due to the elastic coats of the arteries.

The condition of the arteries which most influences the character of the pulse is their tension. We shall endeavour to point out a few general principles which we think may be safely laid down.

There are two kinds of causes which may produce variations in the arterial tension. A greater or less afflux of blood from the heart, or an increase or diminution of the obstacles to the flow of blood through the capillaries: Dr. Marey has alluded in one of his papers to the great difficulty which exists, in the present state of science, in deciding whether the circulation is being rapidly carried on or not; for a rapid action of the heart does not always indicate a rapid circulation. To this we have already alluded in speaking of the velocity of the blood.

Let us consider for a moment what is the condition of a bloodvessel when it is in a state of extreme tension, and what is the kind of pulse which results therefrom. The vessel under these circumstances is stretched beyond its ordinary calibre, and consequently there is but little opportunity for elastic reaction. It rises but little under the finger during its diastole, and falls but little during its systole. If the sphygmograph be applied to an artery in such a condition as this, it also will rise and fall but little, and the curves marked by the lever will be small; in fact, a constant high level will be maintained, above which the rise at each pulsation will be but little marked. Results, similar to these, Dr. Marey has obtained by producing a condition of extreme tension in elastic tubes out of the body, and also by so interfering with the circulation in his own arm, as to cause a similar state in the radial artery.

Again, let us examine briefly into the condition of the arteries under a condition of feeble tension, and in what manner this is indicated by the sphygmograph. We have here the tubes not over-distended by their contents, with a yielding condition of their walls. It will necessarily happen that each impulse of blood from the heart will give rise to a sudden dilatation, and an almost equally sudden reaction of the vessels, and if the sphygmograph be applied to them, it will mark a rapid and almost vertical ascent of the lever, and a descent somewhat more prolonged, but still rapid.

It is obvious at once that the action of the heart will be materially modified by the tension of the arterial system. If the tension be great, increased labour will be thrown on the central propelling organ, its contractions will be more powerful and more prolonged; whilst, on the other hand, a feeble tension, inasmuch as it offers but little resistance to the entrance of blood, and allows of easy distension of the arterial tubes, will call for less force on the part of the heart, the contractions of which will be frequent and rapid.

Most of our readers will probably be familiar with the phenomenon of the dicrotic pulse—a pulse in which there is a second feeble beat following the first. Various opinions have been expressed with reference to its cause. We believe it is of more frequent occurrence than is generally supposed, and it is important not only that its mode of production should be understood, but also the condition of the circulating organs, and of the system at large, which it indicates.

M. Beau has pointed out a clinical fact which we can ourselves confirm, that there is a constant absence of dicrotism in the femoral artery and the vessels of the lower limbs, even when it is well-marked in those of the arm and head; and this fact opposes a very strong argument against most of the theories which have been advanced of the production of the phenomenon.

Dr. Marey is of opinion that *the dicrotism in the radial is due to the echo of a wave of blood, which is reflected at the lower bifurcation of the aorta by the spur of the two iliac arteries.* He endeavours to prove his theory by an appeal to experiments made in an apparatus resembling somewhat the aorta, and having branches to correspond with the iliac vessels and with those of the upper extremity.

We have no doubt that the phenomenon we have considered is due to a reflux or "echo," as Dr. Marey has it, of the sanguineous wave, but we are not quite in accord with Dr. Marey as to the mode of its production. The conditions which are necessary to produce it in the most marked manner are those of feeble tension of the arterial system, and it is essentially a pulse of debility. Our own view is, that it results from a reflux of blood produced by the closure of the semilunar valves, and not from the "echo" reflected at the bifurcation of the aorta. When there is feeble tension of the arterial system, it is by no means difficult to understand how the sudden closure of the aortic valves would produce an impulse on the blood—a shock, in fact, which would be more or less perceptible in the vessels arising from the arch of the aorta. The reason we do not feel the impulse in the vessels of the lower extremity we believe to be in consequence of the curve the aorta itself takes, which it appears to us would be likely to prevent the transmission of so feeble a shock, unless in very extreme cases.

We cannot stop now to consider the important bearings which the facts we have detailed with reference to the arterial tension, have in a pathological and practical point of view, but we think that, the general relations of that tension to the general condition of the system, may be gathered from the phenomena we have described and the principles we have endeavoured to lay down.

We need not dwell at any length on that property of the arteries for which they are indebted to the muscular fibres found in their middle coat. The opinion most generally entertained of their action by physiologists is, that they exercise no power on the onward flow of the blood, and that their function is to regulate the calibre of the vessels to their contents. This is a view entirely opposed to the opinion of Sir Charles Bell, who attempted to prove that the arteries were the principal agents in moving the blood. We shall allude by and bye to some of the arguments he adduced in support of his position, but we must now take into consideration some of the effects produced in the arterial system by certain influences which may be brought to bear on it, as the facts we shall bring forward will serve to illustrate the function of these vessels, and to explain some of the phenomena we meet with in various abnormal conditions of the vascular organs, and will furnish us with arguments in opposition to the opinions of Sir C. Bell.

Anatomical research has pointed out that the muscularity of the arterial tunics is greater in the smaller than in the larger arteries. Observations have further shown that the contractions of these muscular fibres of the arteries are regulated and controlled by the action of the nervous system.

The absolute proof of the influence which the nervous system exercises over the bloodvessels has only been furnished of late years by clear and incontestable experiments. These experiments, which so clearly establish the reflex function of the vaso-motor nerves, are so well known that we need not enter into details with reference to them.

We must, however, allude to a highly-interesting observation which was made by Claude Bernard, as not only does it show the influence exercised by the nervous system on the bloodvessels, but also throws light on the nature of the venous pulse—a phenomenon we occasionally meet with. The following experiment was made on a dog. The submaxillary gland having been exposed,

"The glandular branch coming from the great sympathetic is ligatured; the venous blood of the gland becomes redder, and flows more abundantly; no saliva is excreted; the sympathetic branch is galvanized; the blood of the gland becomes quite black, flows less abundantly, and at last ceases entirely to run. The animal is left at rest; the blood of the gland becomes red. The glandular branch of the gustatory is tied and galvanized; the saliva flows in waves, the venous blood preserves the same bright red colour it previously had, but it flows in much greater quantity, and *presents a jerking stream synchronous with the pulse.*"*

Considering the effect we may produce on distant parts by applications of cold, &c., to different portions of the surface of the body, it becomes a very important question, whether we cannot make a more scientific and definite use of the principle of reflex action of bloodvessels which has been established. It is important to ascertain what part of the surface of the body would transmit, to any given internal organ, a reflex action, in the most direct and influential manner; or whether it is a matter of no importance on what part of the surface we act. We are disposed to believe that with regard to internal organs, there are portions of the skin, from which an influence is more readily and powerfully transmitted to them than from others—that, in fact, there are parts which, for reflex purposes, correspond with each other; and we believe that for the most part they are those which are situated on the same level with the body.

Now, we are of opinion that the recognition of this principle serves to explain that which, when we look simply at the anatomical arrangement of parts, we cannot satisfactorily account for—viz., the effect produced by local measures, as, for instance, the abstraction of blood, the application of blisters or other counter-irritants, on internal organs. We have always had a difficulty with respect to these two points—viz., how the abstraction of blood from the surface of the chest can benefit an inflamed lung, or how the blistering of the surface of the body can have a similar result. We believe that, both by the one

* Journal de la Physiologie, &c., tom. i. p. 241.

process and the other, but more especially by that of blistering—of the influence of which we suppose no one will entertain any doubt—an impression may be made on the nerves of the skin, which, being transmitted to the spinal cord, is reflected to the inflamed organ, and produces an alteration in the condition of its vessels.

When we consider the various facts brought to light by experiment, with reference to the influence exerted, by an injury to the nervous system, on the bloodvessels of an organ, it appears to us that no difficulty need be experienced in accounting for some of the conditions we meet with, in connexion with the arterial system leading to a part which is the seat of inflammatory action, especially when we bring to bear on the subject the facts we have previously considered, with reference to the manner in which the walls of an elastic tube influence the character of the pulsations of a fluid transmitted through it. We need not quote the experiments which have been made to prove that an increased flow of blood takes place in the arteries leading to an inflamed part; we refer briefly to the explanation of the fact which was advanced by Sir Charles Bell, and which he thought tended to prove the point for which he argued—viz., the power of the arteries by their muscular contraction to propel onwards the blood. Recognising the increased pulsation, the undoubted fulness of the pulse, and the fact that more blood was actually passing through the arteries, many pathologists, like Sir C. Bell, had a difficulty in explaining the phenomena, without allowing to the artery itself some local independent action. As we do not now recognise, in the dilated condition of the capillaries of an inflamed part, an over-action of those vessels, so pathologists have come to consider the condition of the arteries which lead to such a part, as the result of a want of tonicity and of healthy reaction of their coats to the pressure produced by the impelled blood. Admitting the disturbance of that tonic contraction of the vessels, which is due to the action of the nervous system, we readily understand how the artery will yield to the force of the current, and we are prepared for that fulness of the pulse we so constantly meet with, which may be altogether independent of any general disturbance of the circulation; for the action of the central propelling organ remaining the same, the local phenomena will vary according to the local circumstances. But there is another point to which we must here allude—that in the vessels leading to an inflamed part we shall find the character of the pulse vary in proportion as there is a greater or less impediment in the capillary circulation. In proportion as the circulation through the capillaries is impeded, or, in other words, in proportion as the tension in these vessels becomes diminished, so will the tension in the arterial tubes be increased, and the pulse be more resisting to the finger.

We pass now to consider briefly the subject of the capillary circulation and the so-called capillary power. In the fifteenth and sixteenth volumes of this Review (1855) will be found an article by Mr. Savory, in which the whole subject is fully and ably reviewed; and as we think he has brought forward almost all the arguments which can be adduced either in favour of, or in opposition to, the theory that the capillaries exert some active force in carrying on the circulation, we shall not enter at any length into the discussion of the question, which still remains *sub judice.* For our own part, we are disposed to agree with Milne-Edwards, that the capillary circulation does not depend on any peculiar power which the vessels themselves possess, but that the movements of the heart determine the passage of the blood alike in this portion of the system as in the arteries themselves. So long as nutrition and secretion are carried on, there is an exosmosis of some of the contents of the capillary vessels. This will have a tendency to empty the vessels, and such an effect must act in promoting the flow of blood to them, in order that the place of those parts of it which have been removed may be supplied. We look upon the changes which take place in the capillaries themselves, and in the arteries leading to them, by which an increased quantity of blood is made to reach the several parts, under the conditions we have alluded to, as primarily produced by nervous influence, which, just as we see in many of the experiments we have referred to, results in a larger nutrition or a more abundant secretion, processes that, as they extract a larger quantity of material from the blood, so produce a greater demand for it. But we see in this no active capillary force. No doubt there are mutual attractions between the different ingredients of the blood and the various tissues and secreting cells, affinities peculiar to each part; but this is not active capillary circulating power; this does not drive on the blood.

Attractive as this subject is, we must not prolong our consideration of it here, but refer our readers to the various works we have quoted and alluded to for a full examination of the question. We think that in the present state of science in general, and of our knowledge of the functions of the vascular system in particular, we are perfectly justified in giving our adhesion to the hypothesis expressed in the following guarded language of Mr. Paget; but beyond this we cannot go: "We have facts enough to justify such an hypothesis, as that there may be some mutual relation between the blood and its vessels, or the parts around them, which, being natural, permits the most easy transit of the blood, but being disturbed, increases the hindrances to its passage."

There are several other topics in connexion with our subject to which we should desire to refer, but want of space now prevents us. Our object has been, in this article, to place before our readers some of the more salient and important points of the circulation, and some of the principal facts which recent observers have brought forward, as well as to indicate to them the sources from which further information may be derived. Although we have given a prominent position to mechanical experiments, and have dwelt much on the mechanical condition of the bloodvessels, we would guard ourselves and our readers against anything like a purely mechanical view of our subject. In a question so complex as that of the circulation of the blood, we must bring to bear on it all our knowledge of the influence of physical laws, and of the manner in which they may he modified by vital action. We must not be led away by the reasoning of the pure mathematician, nor follow the dictates of the simply mechanical philosopher; nor must we, on the other hand, reject their teaching or their principles in any single portion of the subject—we must endeavour, on the basis of structural knowledge, a correct appreciation of experimental inquiry, and a wise interpretation of pathological phenomena, to build up a sound and comprehensive theory.

Review V.

1. *Lehrbuch der Kinderkrankheiten.* Von Dr. Alfred Vogel, Privat docent. und II vorstand der Reiner'schen Anstalt in München. Mit 6 lithographirten Tafeln.—*Erlangen*, 1860. ss. 592.

A Manual of the Diseases of Children. By Dr. Alfred Vogel. —*Erlangen*, 1860.

2. *Lehrbuch der Kinderkrankheiten.* Von Dr. Carl Gerhardt, Privat docent. in Würzburg.—*Tübingen*, 1861. ss. 501.

A Manual of the Diseases of Children. By Dr. Carl Gerhardt.—*Tübingen*, 1861.

3. *Traité Pratique des Maladies de l'Enfance, fondé sur des nombreuses Observations Cliniques.* Par F. Barrier, D.M.P., ex-Chirurgien en Chef de l'Hôtel-Dieu de Lyon, &c. Troisième édition, revue et augmentée.—*Paris*, 1861. pp. 720, 736.

A Practical Treatise upon the Diseases of Childhood, based upon Clinical Experience. By F. Barrier, M.D., late Senior Surgeon of the Hotel Dieu, Lyons.—*Paris*, 1861.

4. *Beiträge zur Kenntniss der Physikalischen und Chemischen Constitution Wachsender und Rachitischer Knochen der Ersten Kindheit.* Von Alexander Friedleben, M.D., zu Frankfort-am-Main.—*Wien*, 1860. ss. 109.

Contributions towards a Knowledge of the Physical and Chemical Constitution of the Growing and Rachitic Bones of early Childhood. By Alexander Friedleben, M.D.—*Vienna*, 1860.

5. *Der Typhus in Kindlichen Alter.* Von Dr. Joseph Bierbaum, Practischem Arzte, &c.—*Leipzig*, 1860. ss. 193.

Typhus as it occurs in Childhood. By Dr. Joseph Bierbaum.—*Leipsic*, 1860.

6. *Historische und Kritische Untersuchungen über das Behorchen des Schädels bei Kindern.* Von Dr. F. Rilliet, früherem Director des Hospitals zu Genf. ('Journal für Kinderkrankheiten,' Band xxxv. s. 1.—*Erlangen*, 1860.

Historical and Critical Investigations concerning Auscultation of the Cranium in Children. By Dr. F. Rilliet. ('Journal for the Diseases of Children,' vol. xxxv. p. 1.)—*Erlangen*, 1860.

7. *Die Syphiliden im Kindes Alter.* Nach Beobachtungen von den DD. FRIEDINGER MAYR and ZEISSL. Seperat Abdruck aus dem Jahrbuche für Kinderheilkunde. Mit fünf auf der Buchdruckspresse ausgeführten Farbendrücken.—*Wien,* 1858. ss. 12.

The 'Syphilides' in Childhood, after FRIEDLEBEN MAYR and ZEISSL.—*Vienna,* 1858.

WHATEVER might once have been the case, we certainly have not in these days any lack of works, good, bad, and indifferent, upon the diseases of children in the chief three languages of Europe. In the French language are to be found some of the most popular *systematic* treatises upon pædiatrics, amongst them and at the head of all being the classical work of MM. Rilliet and Barthez. Our German neighbours have been particularly fruitful of separate tracts and memoirs, and to them we are also indebted for that interesting and useful serial —the 'Journal for the Diseases of Children;' whilst in our own tongue we have but to mention the names of West, Condie, Meigs, and Churchill to recal the interest which has of late years been taken in a once much neglected branch of practical medicine. Whilst most of the French pathologists have been dissatisfied with any labour short of grasping the entire field of infantile pathology in their well known treatises, the German investigators have in recent times rather sought to enrich our professional literature by monographs and memoirs upon special diseases. When we have named Bednar and Hennig's works we have referred, we believe, to all the late systematic labours of our German brethren. The former writer, too, it should be remembered, views disease chiefly from a pathologico-anatomical stand-point, whilst the latter has bestowed upon us but a brief compilation. That our neighbours should have thought there was yet room amongst them for a concise systematic treatise which would view disease rather from a symptomatic and clinical stand-point than from a pathologico-anatomical one, we are, therefore, not surprised. Bednar's work, however valuable in its way, is certainly not one which we should put, as a bedside guide, into the hands of the student. Hennig's, also, leaves much to be desired. There can be no doubt that the labour of merely compiling text-books and systems from the industry of others is a less worthy avocation than extending science by researches in a less wide but more original path of inquiry. But some one must take occasionally for us a "bird's-eye view" of the whole of our possessions; we must have some easily accessible *résumé* of the results of years of work. The student must also be provided with succinct treatises which compass the whole field of any special branch of knowledge. It is the rule, perhaps, that the experimental and clinical worker in medicine is a writer only of separate essays and memoirs, and it is but rarely that we can obtain from him a systematic work. But we are occasionally successful; and not in any branch of medicine have we been more so than in pædiatrics, in which the most practical men of their time and the authors of some of the best memoirs of their day, have presented us with such a view of the entire field of their speciality as to place their work far beyond the reach of rivalry and dispute. We need scarcely say that MM. Rilliet and Barthez are now before our minds. But France cannot expect such good chance often, and Germany has evidently not yet been born to it. We say this advisedly, for we have the latest German Compendiums before us. Both these works may be said to be *compilations by gentlemen who have paid some attention to the diseases of children.* Both authors are "privat-docents;" one being at Munich, the other at Würzburg. Now we do sincerely hope that the literary chivalry of these two gentlemen will satisfy the class ambition of all other "privat-docents" *quoad* pædiatrics for some time to come. We have had a fear lest their whole body might turn loose upon this department of medicine, each individual endeavouring to work himself into note by offering some ingenious *réchauffé* of Rilliet and Barthez, Bednar, Mauthner, Bouchat, Weber, &c. &c., to our now somewhat fastidious appetites. "Privat-docents" must of course, similar to a well known class amongst ourselves, make themselves famous. They must—or at least they fancy they must—write something to arrest attention. We do hope, however, that they will have mercy upon the diseases of children. We do not intend by these remarks any disparagement to the labours of Dr. Alfred Vogel. Within the limits of six hundred not overloaded pages he has managed to give a good *résumé* of the more important facts and doctrines of which pædiatrics are composed in the present day. His 'Manual' is fairly cognisant of recent research, &c.; it is written in a clear and scientific manner, and is quite free from that windiness and repetition which constitute the bane of the writings of so many German authors. Dr. Vogel tells us that it is his wish to offer his work as an assistance to students who may be reading up at home the cases they have witnessed in the "ambulatorium" during the day. The book fulfils this intention very fairly, and we are not sorry to have made its acquaintanceship, as we have found it both handy and trustworthy to refer to upon an emergency. Dr. Gerhardt's treatise is much less to our taste, and we shall be surprised if it is not left far behind in the race which it has to run along with the text-book of Dr. Vogel for public approval. The more noticeable of Dr. Gerhardt's pages are those furnishing directions to beginners conducting the necroscopic examinations of young children.

Besides these new German 'Compendia,' we have a fresh issue of the once often-quoted work of M. Barrier. Fifteen years have slipped away since the appearance of the second edition—years, too, which have been (at least in our opinion) very fruitful of progress in the pathology of infancy and childhood. But it would seem that M. Barrier is not so clearly of this way of thinking, and in consequence, his new edition will not make as much way against other works which have taken the field since he first appeared in it as he might desire. His poor opinion of the labours of others, and the good one of his own exertions, have been the means of preventing this new issue of the 'Practical Treatise' being all that it should and might be. M. Barrier tells the reader that

"Although fifteen years separate us from the preceding edition, the present one differs but little from it; whether because during this period the acquisitions of science have not been very important, or because the reception given to our labours upon two separate occasions have permitted us to think that their intention and scope have been favourably appreciated." (Preface.)

Such being the case, the result is not surprising. M. Barrier's new edition must be pronounced not sufficiently *au niveau* with the more advanced pathology of the day. Though the author assures us that he has revised it "with much care," it is quite clear that he has felt himself bound, in reference only to a few important topics, to glance at such salient doctrines as have made too great alterations since M. Barrier first came before us to be ignored *in toto*.

M. Barrier continues to hold the views originally promulgated by the French school concerning the nature and frequency of the so-called "lobular pneumonia," or that form of pulmonary induration so often associated with bronchitis, in children from one to five or six years of age. The later views of Jörg, Legendre, and Bailly are referred to (p. 185); but it is maintained that, although the condition resulting from collapse of the air-cells may perhaps have been mistaken for the effects of pneumonia, yet as, according to the observations of the two latter pathologists, such condition is in the great majority of instances developed concurrently with a lobular pneumonia, the views commonly held regarding the anatomic character and frequency of this malady require no fundamental alteration. It is true, that in one class of cases MM. Legendre and Bailly associate the collapse of the air-cells with a "catarrhal pneumonia;" but that is certainly not all that M. Barrier connotes in his "lobular pneumonia." The latter thinks it probable that at least a portion of the cases of what has been termed "carnification" of the lungs of infants may be explained by the doctrines of MM. Legendre and Bailly. Nevertheless it will be preferable, before deciding even this, to await further inquiry, seeing that, whilst the air-cells of the lung in the *état fœtal* are dilatable by insufflation, in the carnified tissue they appear obliterated. "If the latter be really distinct from the *état fœtal*, it is then without doubt nothing but an induration by which inflammation has terminated. But it is far from being a true cure of this process, since the carnified tissue appears unfit for respiration, and insusceptible of reverting to the normal state." (Vol. i., p. 187.)

M. Barrier, though not adopting the anatomical views of Legendre and Bailly, Rufz, Hasse, West, and others, explanatory of the lesions found in the pulmonary parenchyma, almost as much necessitates, however, the presence of a bronchitis for their production as do those pathologists. He does not appear, indeed, to stop far short of the generalizations of Burnet and De la Berge, who affirm that "lobular pneumonia" *always* takes from a bronchitis its *point de départ*. M. Barrier writes:—"Out of 61 cases, 53 furnished proofs of the pre-existence of bronchial catarrh; . . . . and if in the 8 remaining cases the antecedent bronchitis could not be demonstrated for want of evidence, its existence was at least very probable." (p. 207.)

M. Barrier takes but a cursory notice of the modern doctrines of vegetable parasitism relative to "muguet," and omits altogether their recent application to the pathogeny of bucco-pharyngeal and laryngeal diphtheria. Indeed, he declares himself (vol. i., p. 712) as "not altogether able to appreciate exactly the value" of these views. "Muguet" M. Barrier affirms to be simply an inflammation of the mucous membrane of the alimentary canal, accompanied by an exfoliation of the epithelium (analogous to that of the epidermis which takes place during the first weeks of life), and by an exudation of a plastic layer. The cause of the malady is the irritation arising from unsuitable milk or improper artificial diet. These act upon the delicate and sensitive alimentary mucous membrane, in an analogous manner to cold air upon the bronchio-pulmonary lining, in producing the pneumonia of early life previously spoken of. The author denies that "muguet" (vol. i., p. 701) can be propagated by contact or inoculation.

The subject of "Thrush" generally is clearly, though necessarily concisely, treated by Dr. Vogel, who believes in the inoculability of that form of it we are now alluding to. We were surprised not to find any mention of the cardiac symptoms and lesions which are now well known to be sufficiently often associated with chorea as to lead most pathologists to believe in a causal connexion between the two diseases. At length we arrived at the following passage, which cleared up the mystery:

> "In recent times M. Sée, followed by M. Botal, has supposed that not only is there often coincidence of chorea with rheumatism, but moreover an ætiological connexion between these two affections. We regard these assertions as very exaggerated. In the first place, the coincidence is rare enough, unless we consider several morbid conditions as rheumatism, and which they certainly are not. Farther, if rheumatism sometimes precede chorea, this is an insufficient reason for regarding the latter as a form of rheumatism, and for establishing an identity of nature between the two maladies." (Vol. ii. p. 323.)

Dr. Vogel appears to agree with M. Barrier upon this point. Without arguing for what M. Barrier terms "an identity of nature between the two maladies," we have no hesitation in expressing a belief in some intimate pathologic union between chorea and rheumatism. Our own personal experience points to this, and it is fully borne out by that admirable digest of 309 cases of chorea, by Dr. Hughes and Mr. Barton, to be met with in 'Guy's Hospital Reports' for 1846 and 1855. In the last analysis of 209 examples of this disease, either a cardiac murmur, or the previous existence of rheumatism, or both, are mentioned as having been inquired after or searched for in 104 instances. The authors observe upon this, "that of the 104 cases, . . . there were only 15 cases in which the patients were both free from cardiac murmur and had not suffered from a previous attack of rheumatism." There is surely something more than an accidental coincidence in this.

It has been for some time established by anatomical and physiological inquiries that a continuous osteogenetic process goes on during *rickets*. There is deposited here, as in health, layer upon layer of new matter, the deeper-seated of which layers are constantly disappearing through resorption. The difference between normal and rachitic osseous growth lies in the want of the usual deposit of earthy salts in the newer layers. The latter hence remain soft and somewhat puffy. At one time, it is true, it was believed that the bones *became soft* rather than that they *never became hard* in rachitis, but this idea we imagined had been for some time abandoned. Yet here we have Dr. Friedleben (p. 97) taking credit to himself for enunciating the now very common doctrine. Whatever other merit is his due, he cannot lay fair claim to that suggestion. The true *softening* which occasionally accompanies the rachitic growth is to be explained by the occurrence of some secondary process superadded to, and not necessarily involved in, the rachitic action. During the continuance of the latter, there is proceeding without doubt resorption of the older and underlying ossific layers, but this is a normal act, a necessary process in the growth of healthy bone, and not a true part of the rachitic process. So far as this morbid state is concerned, we may venture the opinion that Dr. Friedleben's remarks upon the act of resorption are of considerable value. He endeavours to show that however actively this process may go on during rickets, it would be a serious error to assume that it constitutes the pathogeny of the affection. Repeating the experiments of Chossat, the author fed pigeons upon peas and corn so prepared that they did not contain any or but a *minimum* of earthy salts. This source of nourishment failing the birds, the latter after some time died from diarrhœa and progressive emaciation. Upon examining their bones, it was found that resorption had been greatly exaggerated. The amount of earthy salts was reduced to nearly half that of healthy bone, the fat was increased, the specific gravity was lowered, and the quantity of carbonic acid was almost one-fourth less than that obtainable from the ordinary osseous structure of healthy birds. The bones were likewise thin and brittle, but not bendable. In fine, there was ample evidence of resorption having been increased, of new osteogenesis having been arrested, and of the bones having become fragile, but no proof of their having become soft so as to simulate rickets. Some difference of opinion has existed as to whether the cartilage of ossification in rachitic bones yielded gluten or chondrin. Von Bibra, Marchand, and Lehmann failed to procure gluten from such bones, the latter chemist, moreover, obtaining a highly-gelatinizing substance, offering some of the reactions of chondrin. Upon the other hand, Schlossberger obtained from rachitic cranial and thigh-bones perfect gluten. Our author met with the same success, and observes that—

> "We can arrive at no other conclusion than that the organic basis of rachitic bone, both during the continuance and after the cessation of the diseased process, had not undergone any change in its chemical reactions. . . . . . the unossified cartilage contained more water." (p. 94.)

A not unimportant item of Dr. Friedleben's investigations is that which tends to correct somewhat the views which have been held regarding "cranio-tabes" or "soft occiput," since Elsässer drew attention to it some few years ago. Most pathologists who have alluded to this condition, have on the one hand regarded all cases of soft and yielding cranial bones in the young child as examples of rickets, and on the other hand, they have looked upon cranial rickets as peculiar to children still at the breast. According to our author, both these generalizations are wrong. In the first place, Dr. Friedleben maintains that the *soft state* alone of the cranial bones, together with their thinned condition, are not necessarily proofs of cranial rickets. In the second place he asserts, in conformity with his main doctrine, that when the bones *are* soft, thin, and rickety, such softness and thinness are not essential to the rachitic process. This is a diseased process, and one apart from that which produces the conditions in question, and which are the results of a normal or physiologic action—viz., that of *resorption*. The author admits that the diseased state being added to the physiologic one of ordinary osteogenesis, produces "a thinner, softer, and more yielding" bone than usual, from the disease necessarily being accompanied by a more watery condition of the cartilaginous, &c., matrix, than is common to healthy ossific tissue. But

"Great care must be taken not at once to assume without further investigation, that every instance (even in strong, well-nourished, thriving children showing no other sign of diseased action) in which thinning of the cranial bones is easily perceived by the touch to be present, is of a rachitic nature. Closer observation will easily prove that without any assistance from art, change of diet, or of nursing, in the course of a few weeks, or it may sometimes be of months, all the former soft and yielding spots have become hard and firm. We have in such cases, as we have before shown, to deal only with the normal development of the cranial growth which the careful and continuous observation of children of all ages has shown to be sometimes more, sometimes less advanced in progress. Many of Elsässer's earlier cases, as also not a few of his followers, cannot therefore be admitted as examples of disease." (p. 106.)

It has been the opinion of Elsässer, Vogel, Virchow, and others, that the very thin or attenuated spots to be met with here and there in the soft skull of extreme "cranio-tabes" were due to pressure of the brain upon the skull. Upon this opinion Dr. Friedleben observes,

"That they originate from 'resorption' there cannot be a doubt, but not, as Virchow maintains, from resorption of new ossific deposit, for they become formed before a trace can be found of new deposit at the occiput. The plane of their ossific plates runs quite even with that of the inner older layers. Besides, this circumstance clearly opposes the notion of (at all events a partial) cerebral pressure—viz., even in the most marked example of completed rickets accompanied by quite membranous gaps at the occiput, neither projections of these gaps outwards beyond the level of the dura mater, nor an imbedding of the cerebral convolutions in them are ever to be seen." (p. 106.)

Further, were these attenuated spots due to pressure from within the skull, the internal periosteum must be affected. Such result has not been alluded to by any pathologist, according to our author. He however admits that the examination of dried preparations alone might lead to the belief that the pressure in question really had been exerted, but yet maintains that the scrutiny of fresh specimens, and the comparative examination of healthy living sucklings, will readily dissipate the idea. Dr. Friedleben is a total disbeliever in the production of laryngismus, or "Tetanus apnoicus infantum" from pressure upon the brain, through the attenuated spots, by a force from without. We cannot follow the author in his scepticism, having had to treat a child in whom occasional tonic convulsions were seen, and which, so far as we could make out, were produced by pressure inwards of the occipital bone when lifting the head in nursing, &c. As regards the essential nature of what we may call the rachitic *nisus*, Dr. Friedleben attempts a farther step in generalization than has yet been made, though he admits he cannot bring forward such positive proof of the truth of his doctrine as is demanded by exact science. According to him, we must recognise, as a fundamental character of all rachitic tissue, its succulent and swollen state, due to great increase of water in the cartilaginous and ossific layers. In the second place, careful inquiry will go to show that some pulmonary disease, or some disturbance of the respiratory function, precedes the outbreak of the rachitic disorder. From these latter follow collapse of the air-cells (or atelectasis or induration) of the lung tissue, and with it such additional and continued embargo upon respiration as to prevent those healthy and natural changes ensuing which are necessary to the normal production of new tissue. The evil is farther added to by the defective hygiène and nourishment to which the child is subjected. The result finally appears in the abnormal condition (before alluded to) of the cartilaginous matrix, and in consequence of which there cannot take place any union between the earthy salts and the cartilaginous molecules. To say, then, that rachitism is only a want of earthy salts in the bones is neither a sufficient nor a proper explanation of that state, according to Dr. Friedleben. There is an absence of these salts, it is true; but the question is, Why are they absent? It was once generally held that these salts are from an excess of acid either too rapidly removed from the body to be plastically employed, or that they are immediately re-absorbed after having been so employed, and find their way out of the system through the urine, in which they appear as an excess of phosphates. This opinion Friedleben strongly opposes. He maintains, from direct observation, that the reaction of both healthy and rachitic bones is never acid, but generally slightly alkaline. Independently of this, it is evident from Lehmann's researches, which afford an increased per-centage of carbonic acid from rachitic bones, that the presence of a free acid in them is more than problematic—an acid, too, be it remembered, of the name of which we are yet ignorant. Friedleben maintains also that an increased excretion of phosphatic salts by the urine is not a necessary accompaniment of rickets, and that even when it is present it is probably dependent upon some other pathologic state or condition of the patient.

As far back as 1853, MM. Rilliet and Barthez drew attention in the second volume of their classical treatise upon the diseases of children to the import of the *bruit de souffle encéphalique* as affording assistance in the differential diagnosis between doubtful cases of rachitism and chronic hydrocephalus. If the enlarged cranium be accompanied by the "bruit," the example is most probably one of rickets, the cerebral disease not being associated with the sound in question. But the general subject of cerebral auscultation they did not then touch upon. This had already been brought before the profession by two American physicians—viz., Dr. Fisher, of Boston, and Dr. Whitney, of Newton, Massachusetts.* In this country, Dr. Richard Smyth first made allusion to it;† but both here and elsewhere it was a subject which failed to excite any particular interest. Within the last two or three years, however, it has obtained some attention. At the suggestion of Hennig, the Director of the "Polyclinic" for children at Leipzic, Wirthgen made some investigations connected with it, and wrote an inaugural dissertation, 'De Strepitu qui in Capite Auscultando Auditur,' Lips., 1855. Soon after this appeared, Hennig himself published a paper in 'Vierordt's Archiv'—"Ueber die bei Kindern am Kopfe und am Obertheile des Rückgrates wahrnehmbaren Geräusche." This was followed by a communication of M. Roger to the Parisian Academy of Medicine, in October, 1859. Finally, M. Rilliet has again directed his attention to "cerebral auscultation," and has deemed it advisable to combine the results of his own experience with that of the observers before mentioned, attempting to draw some general conclusions which may be held as trustworthy for the future. Of the paper in which these are contained we purpose to give a short analysis.

Leaving out of consideration for the moment the later observations of M. Rilliet, we may state that previous inquirers seemed to agree in the following conclusions:—(*a.*) If the ear be applied to the head of a young child over the unossified fontanelle, a peculiar blowing sound may be heard, and which may be termed the "bruit de souffle céphalique." (*b.*) This sound is not to be heard previous to the commencement of the first dentition, and scarcely ever after a child is much above three years of age. (*c.*) The closure of the fontanelle is the anatomical cause of its disappearance. (*d.*) The sound or "bruit" is an intermitting one, and isochronous with the stroke of the heart. (*e.*) It is to be heard more plainly over the anterior fontanelle than anywhere else. (*f.*) Its origin must be sought for in the arterial system, since the sound corresponds with the systole of the heart and the diastole of the cerebral arteries. (*g.*) The sound can be intensified by increased energy of the heart's contractions, and by a state of hydræmia. (*h.*) It can be diminished by physiological and pathological states of depression of the system. (*i.*) The latter (*g.* and *h.*) explain the modifications which the sound undergoes in its appearance, course, disappearance, and reappearance.

Upon two important points the American and German observers differ. The latter assert that the cephalic "bruit" may be heard in children who are quite healthy, and indeed regard it up to a certain point as a proof of health and strength. The former maintain that

* American Journal of the Medical Sciences for 1838, 1843.

† Miscellaneous Contributions to Pathology and Therapeutics. London, 1844.

in full health the sound is not to be heard. The German pathologists further consider any pressure upon, or distension of, the brain, diminishes the intensity of the "bruit;" whilst the American writers take an opposite view, and regard the presence of it as indicative of increased cerebral tension. Now, as M. Rilliet observes, the question as to whether the sound is to be heard in a perfectly healthy infant would appear to be at first sight capable of easy solution, by taking a sufficient number of sound children of from five to twenty-four months of age, and examining them with this specific object. But, in the first place, can a child whose fontanelle remains unclosed at two years of age be regarded as perfectly healthy? In the second place, to consider an infant as unhealthy simply because the cephalic "bruit" is audible, would of course be begging the question. According to Dr. Whitehead, of Manchester,* who has paid much attention to the subject of infantile growth and development, the average age at which the fontanelle becomes "closed" in children having a *good* development is fourteen months and a half; whilst in some cases of *bad* development it was found still open as late as from three to four years and a half. On the other hand, according to M. H. Roger, the period of "ossification" of the anterior fontanelle lies between the age of fifteen months, "when the ossification is very rare," and the age of three years and a half, when "it is always met with." "Between these two extremes, it may be reckoned that the most usual period of occlusion is between the second and third years."† Much of the discrepancy here apparent no doubt depends upon the difference between a closure by *perfect ossification*, a state only to be decided by postmortem examination, and by that from dense membrane—in which case it may appear clinically that ossification has quite ensued, though as yet it is unfinished. M. Rilliet observes:

"If further inquiry substantiate the opinions of Wirthgen, the general systemic changes seen in rachitis must cease to be regarded as the actual producing cause of the cephalic blowing sound, and be rather viewed as only contributing to render it stronger and more easily appreciable. Anæmia, as respects its influence, must be looked upon in the same light. . . . But these opinions require to be substantiated by further research; and we are at present justified in considering the pathologic condition of the blood as the chief cause of the augmentation, if not of the origin, of the sound." (pp. 12, 13.)

Relative to the second point at issue, the French writer partly agrees with the German pathologists.

"In fact," he says, "I believe that the cerebral tension or pressure is not the cause of the development, but rather of the disappearance of the sound. Nevertheless, I do not share the ideas of MM. Hennig and Wirthgen, that such tension must be of—if I may so express it—an acute character. One of a chronic kind can destroy the blowing sound; this fact M. Barthez, as well as myself, has witnessed in chronic hydrocephalus. . . . Our own opinion—that is, the opinion of M. Barthez and of myself—has always remained the same, and we still adhere to the belief that the blowing cephalic sound does not exist in cases of chronic hydrocephalus." (pp. 14, 17.)

* Third Report of the Clinical Hospital, Manchester, 1859.
† L'Union Médicale, Nov. 26th, 1859.

The grand conclusion of M. Rilliet's inquiries is, that the chief value of cerebral auscultation is for the differentiation between rachitis and chronic hydrocephalus. Fisher, Wirthgen, and Hennig maintain that the "bruit" exists in the latter disease; whilst Roger,* agreeing at first with Rilliet, that it was not to be met with in it, appears to have now a different opinion; for he says—

"Wanting in the greater number of cases, wanting in meningitis, in concussion, &c., the abnormal 'bruit' has only presented itself in some children suffering from chronic hydrocephalus; but it has not been sufficiently often present to fairly permit me to regard it as a sign of effusion into the brain. In fact, we cannot determine upon the existence of any cerebral affection, either from its presence or absence."

M. Rilliet's Memoir terminates with the following paragraph:

"Yet one remark in conclusion. In accounting for the presence or absence of the encephalic murmur, we should not forget that a double series of facts must be taken into consideration. First, those circumstances operating causally in its production and intensification, and those which tend to suppress and to remove it; or, in other words, the conditions for the development and augmentation of the 'bruit,' and those for its transference and propagation are not identical. If both kinds be present, the sound will be at its greatest intensity. That is the case in rickety children whose heads have large circumference. But if the conditions necessary for the strengthening of the sound be very prominent, whilst, on the contrary, those which oppose its conduction be slight, the former may quite overcome the latter, and the 'bruit' become so loud as to override the hindrances to its transference. Probably we may in this way explain those exceptional instances in which the sound has been heard in children with *congenital* chronic hydrocephalus." (p. 23.)

Not long since we passed in review† the question of typhoïd fever as it occurs in the child. It was then shown that this fever was by no means infrequently to be met with in early life, that in very many cases it was easily diagnosable, though so constantly confounded with other affections, and included under "infantile, remittent, gastric fevers," &c., but that there do occur instances in which it is almost impossible for the diagnosis to be made absolute until the time has passed for treatment. These latter cases being such as where the diagnosis lies between follicular enteritis, with a pyrexia of a low kind, and the fever before us. In discussing the question, we preserved the clinical distinction between typhoïd and typhus fevers, confining the argument to the former malady. We then took the memoir of Friederick for our starting-point. Now, Dr. Bierbaum tempts us to a short *excursus*, though not, we are sorry to say, for the purpose of praise. In the first place, he refuses to acknowledge any difference between typhoïd and typhus fevers, and tells us that they are only different grades of development of the same affection. This would be all very well if it were clear that Dr. Bierbaum had studied both fevers. Having done so, he would have as fair a right to an opinion upon this litigated question as would anybody else. But it is evident to us that the author has not a clear and definite knowledge of what we see and talk of *here* as typhus

* Traité d'Auscultation, second edition. 1854.
† See vol. xxii. p. 147, of this Journal.

fever, and that and that alone can be compared with typhoïd fever in the point at issue. We are told, for instance, that the *typhus fever* of other writers is synonymous with our author's *grave typhoïd*, and when we come to look at the symptoms of the latter we find amongst them diarrhœa and sanguinolent stools, meteorism, pain upon pressure over the cæcal region, accompanied by an exanthem of rose-red spots! Whether typhus and typhoïd fevers be essentially distinct diseases, is a question which is still *sub judice* amongst ourselves. The dispute would be easily settled, however, if the arguers for their distinction had not in their eye a typhus marked by somewhat different characters to those attached to this disease by Dr. Bierbaum. Any one reading the new "typhuslehre," and not well acquainted with the modern pathologic doctrines concerning the two maladies, would be seriously misled as to the true nature of the matter in dispute. When Dr. Bierbaum states (p. 164), that "typhoïd fever is less dangerous than typhus," it must not be supposed that his "typhus" and "cerebral typhus" are synonymous with what we call typhus here, and of which some of the foreign writers have a proper knowledge. Such a statement as the above means nothing more than the truism, that mild typhoïd, is less dangerous than grave typhoïd fever, and that typhoïd with early head symptoms is more likely to run on to a fatal result than when they are absent. Although the author describes a "pectoral typhus," a "laryngo-typhus," a "pharyngo-typhus," as well as an "abdominal" and "cerebral typhus," we have failed to discern any delineation of typhus properly so called. Dr. Bierbaum does allude, it is true, at page 30, to a "petechial typhus," but as he appears to dismiss it from all further consideration, we scarcely know what relation this variety holds to the argument. Had the author treated of a typhous process as manifesting itself under the two generally clinically different forms of typhus and typhoïd fevers, but maintained that the latter were yet not essentially distinct, however apparently so, we could have followed him. Had he at the same time described both forms under the term "typhus," making our typhoïd synonymous with his abdominal typhus, and our typhus synonymous with his cerebral or petechial typhus, after the manner of his countrymen, we could have seen our way clear through his essay. As it is, however, we are like Cassio, we "remember a mass of things, but nothing distinctly." Both Vogel and Gerhard describe the continued idiopathic fever of children under the term of "abdominal typhus," and this is well known of course to be synonymous with our own "typhoïd," as well as with that of the French pathologists. Not one of the writers whose works are now before us, from Barrier to Bierbaum, give us any information regarding what we call "typhus" in early life. We question very much whether many of their brethren could give us more. Upon reading Dr. Hauner's Report* of the cases admitted into the Hospital for Children at Munich, we came upon a paragraph which led us at at first to suppose that we had met with some information upon this interesting topic. But no: the more we read, the less we

* Journal für Kinderkrankheiten, Band xxv. p. 120. Erlangen, 1860.

were enlightened. That true typhus does occur *here* in early life we know from personal experience, and in the lectures of Dr. Jenner reference will be found to thirteen cases in which typhus attacked individuals from four to fifteen years of age. We were surprised to find M. Barrier, in his article upon typhoïd fever, touch only superficially upon the important question of the differential diagnosis between that disease and the follicular enteritis of very young children.

It is well known that there are one or two presses at Vienna which produce very fine specimens of letter-press printing, as also of woodcuts and coloured illustrations. The comparatively great costliness of medical books accompanied by the latter has hitherto stood much in the way of the wide sale of certain works which it is important should have a large circulation. In treatises upon dermatology and syphilis, *e.g.*, it is particularly important that the reader should have given him what we may term correct physiognomical sketches of the affections alluded to by the author. Without them diagnosis is next to impossible, and without good illustrations there cannot be formed a good idea of the diseases. Mere verbal description is next to useless for conveying any profitable information to the novice. Even the modern process of chromo-lithography (oil-colour-printing) has scarcely helped to diminish the want. In the first place, the press-work is difficult and tedious, and in the second place, the stones are generally worn out after 1500 impressions have been taken. Hence this art-process, in its better aspect, has been limited to the multiplication of separate oil and water colour pictures, or to the formation of illustrative atlases appended to costly works upon art, archæology, natural science, and occasionally upon medicine. A necessary circumstance also of this mode of illustration has hitherto been that the drawing or cut is not incorporated as it were with the text. It has been by the means only of the woodcut and the printer's press that a pictorial illustration could be worked off in the same impression immediately with the type; at least, so that both print and illustration should be satisfactory and agreeable.

Attempts have recently been made to overcome some of the drawbacks we have alluded to, and to the ingenuity of H. Knöfler, the director of the xylographic *atélier* of the well-known typographic and art establishment of Zamarski and Dittmarsch, of Vienna, we are indebted for a great step being made towards the desired result. We have lying before us the 'Syphiliden im Kindesalter,' published more as a prospectus of what can now be effected in the way of cheap coloured illustration than as anything else. Several vignette specimens are cut in boxwood, and afterwards printed off with from three to five tints of colour, by the printing-press directly with the type. The blocks permit of more than 100,000 good impressions being taken off, are safe from the influence of the acids present in the colours employed, and are likewise applicable to the process of stereotyping. So facile is the application of the new method, that more than 1000 impressions can be worked off by a hand-press during the day, and the inventors look forward to further improvements, which will perhaps permit of quintuple this quantity being produced The process admits of the finest lines of the woodcutter being seen, the work is clear and clean, and quite free from anything like blotchiness. The first illustration in the pamphlet represents "roseola syphilitica" in its transition stage to "psoriasis" upon the face of a child. The second, the same form of disease upon the buttocks and nates, accompanied by "mucous tubercle" of the anus. The third, a pustular syphilide upon part of the trunk and the whole of the lower extremities; whilst the fourth coloured cut exhibits the whole infant affected by a congenital pustular syphilide. Whilst admitting that great progress has been made, we must yet remark that in the specimens before us, the shadows have all a most unnatural green tone about them, and there is too much of crude positive colour about the cuts generally. The "mucous tubercles" in Cut II. might be anything. The third illustration is the best. The cost of this pamphlet here is eighteenpence. Of its text there is not much to be said. In the first volume of the 'Jahrbuch für Kinderheilkunde' (Wien, 1857), may be found two essays upon the 'Congenital Syphilis of New-born Children and Sucklings,' by one of the authors whose names are appended to the present tract —viz., Dr. Zeissl, and to which the reader desirous of knowing the opinions of the Viennese school upon this subject may refer with advantage.

Review VI.

A System of Surgery; Pathological, Diagnostic, Therapeutic, and Operative. By Samuel D. Gross, M.D., Professor of Surgery in the Jefferson Medical College of Philadelphia. Two Vols.—*Philadelphia*, 1859.

If the reader were in danger of forgetting that science is progressive, and that surgery—as we understand it, of little more than a hundred years' growth—has advanced in his own day with at least equal strides with other departments of human knowledge, the accumulating literature of the Profession would recal him to a just sense of the position in which modern surgery stands. For much of this we are indebted to those who have already passed away; some, to whom our obligations are not less, are still among us.

The old treatises on surgery, the work of authors too numerous to mention here, were good in their way; they were faithful records by the writers of their own practice and observation. Often, no doubt, the writer of earlier times took from other sources, as modern authors do, facts unsupported by authenticity; but a careful examination will show, that he distinguishes honestly between his own work and what he has acquired at second hand. What is practical and true holds good.

These more laborious compilations of a past generation have been superseded by monographs, and by abridgments and handbooks, which have been called into existence by the wants of the day. In them the student finds what he wants; and the practitioner uses the book of his student-life. The monograph, the rival in popular favour, shows the class of experience of the individual who writes. Each artisan in his specialty accumulates experience, and the result is shown in many valuable works.

Books, it must be acknowledged, are made in many different ways. Not many years ago we heard two members of the Profession discussing the merits of a volume which had been lately published. "Why," said one of them, "the man has written the book from two cases." "Most men," replied the author's friend, "write from a single case."

A 'System of Surgery' is necessarily to a certain extent a compilation. The author of such a work, like a lecturer, endeavours to collect all the scattered knowledge that has been obtained, and incorporating with it the additional information gained by his own observations, to arrange and display the whole in a clear and readily accessible form. For such a work Dr. Gross is eminently fitted.

After being for several years Professor of Surgery in the University of Louisville, and surgeon to the hospital there, enjoying at the same time a high reputation, with extensive practice, as a consulting and operating surgeon throughout an area of many hundred miles, he has lately been appointed to the chair of surgery in Jefferson Medical College, in Philadelphia, in succession to the late Professor Mütter. He has published shorter treatises on different subjects of surgery, and has contributed some valuable papers to the periodical publications in America. This, his largest work, is founded upon the lectures he has delivered as professor of surgery; "it should be regarded," he says, "as embodying the results of a large personal, if not a ripe experience, of extensive reading, and of much reflection." Upon subjects where his personal experience has been limited, he gives a clear account of what his predecessors have done; and his criticisms upon new or untried modes of practice are candid and satisfactory to an inquirer.

Of a work so comprehensive our pages do not allow us space to give more than an outline, after omitting many of the subjects which the specialities of the day have withdrawn from the province of the general professional critic.

The work is contained in two large and closely-printed volumes, of nearly 1200 pages each, and is intended by the author to "embrace the whole domain of surgery—to exhibit an outline of the existing state of the science upon every topic of which it treats." He has devoted a larger space than customary in similar works of preceding writers to the consideration of general inflammation and its results, the great principles of surgery; this occupies the first five chapters. Under the head of "General Surgery," the next six chapters comprise new formations, wounds, and morbid states of the constitution, or "poisons" as liable to occur in every region of the body. Directing special attention to the discrimination of diseases, he allots one chapter to general diagnosis. To operations, the modes of performing them, and the instruments to be used, the rest of the first part of the book is given. "Special Surgery" forms the second part, comprising the diseases and injuries occurring in particular organs, textures, and regions. The remainder of the first volume, and seventeen chapters of the second, are devoted to this comprehensive subject. At the end of the second volume are three chapters of what may be called *paralipomena*—some operations and local affections, the description of which has not been included in former pages.

Without attempting to define within strictly philosophical terms what constitutes *inflammation*, what are its limits, or how the early stages of a state which he assumes to be familiar to his readers are to be appropriately allotted to irritation, sympathy, and idiosyncrasy, or to congestion, he considers its causes, symptoms, and effects, and points to the treatment of these as the chief subject of study to the practitioner. The modes of termination, he says, amount only to two—one being in health, by "delitescence;" the other in the death of the part, by ulceration and gangrene. All deposits and all changes of structure are to be viewed simply as so many products or results of inflammation, without necessarily involving a suspension of the process itself.

In speaking of *hereditary disease* among predisposing causes, he says: "There are family diseases, just as there are family likenesses, manners, and peculiarities; and what is remarkable, they are more liable to be communicated by the mother than the father, as if it were her special prerogative to impress her vices, as well as her virtues, upon her descendants." Possibly disease transmissible from either parent will be more likely to be developed in those of the children who partake most of the *vis insita*, as of the physiognomy of that parent; but we do not know that the observation of our readers will be found to confirm this statement of the author.

While reading different parts of the work—those relating to different diseases as he successively treats of them—it has seemed to us that the author maintains with too great favour the practice of *abstracting blood*, as a powerful means for removing the activity of disease. Former writers have often told us that the diseases affecting our brethren in North America are of a more inflammatory type, are more sthenic, and require for their subjection a freer use of the lancet, and a more liberal use of leeches. The frequency with which allusion is made to taking away blood seems to show that this practice is more prevalent there than here. The stirring active habits of the people (to say nothing of the air they breathe, or of the cheapness of alcohol) must have had some influence upon the material of disease, and upon the frequency of it. We might think there was nothing moderate in American life. Taking root in a new soil, the human plant rapidly grows to maturity, and rapidly passes into decay. The consulting practitioner often retires upon a competence at a time of life when, in the old country, he has scarcely begun to fix himself firmly in the estimation of the public and his professional brethren, or to get a footing within the circle of remunerative practice; and the highest officers of a kindred profession are superannuated before the grand climacteric brings the evidence of a decay, which here we do not acknowledge as accompanying it.

The author must be allowed to speak for himself upon the subject of general bloodletting:

"A great change has come over the profession within the last fifteen years, and is steadily gaining ground, subverting all our preconceived notions upon the subject, and rendering it very questionable, in the opinion of many, whether bloodletting is really ever required as an antiphlogistic. Whether this change has been the result of a modification of the type of disease, of a more improved method of treatment with other remedies, or simply of the whim and caprice of a few prominent and influential practitioners, from whom the rest of the profession have imbibed their views, I am unable to assert; but the fact does not admit of a doubt that more quarts of blood were formerly spilt than ounces are spilt now. Bleeding is no longer the fashion: the operation is denounced by every one. Public sentiment has got to an extreme upon the subject, and we may therefore soon look for a reaction in favour of the opposite opinion. For myself, I cannot but regret this state of things, because I feel satisfied that it does not rest upon a just and proper basis. If we formerly bled too much, too frequently, too copiously, and too indiscriminately, it is equally certain—at least to my mind—that the operation is not enough resorted to at the present day. Many a deformed limb, blind eye, enlarged spleen, and crippled lung, bear testimony in every community to the justice of this remark."

In the pages following this extract, his remarks on bloodletting, the cases requiring it, and the classes of subjects that will bear it, or otherwise, will be found to be judicious, as well as the precautions to be taken in having recourse to this, which "may justly be regarded as standing at the very head of the list of the constitutional remedies for inflammation."

The *juvantia* and *lædentia* of treatment are pointed out with praiseworthy minuteness. He advises attention to the temperature of the patient's room, and to ventilation; he insists strongly on cleanliness, a frequent change of bedclothes and body-linen, and washing or sponging of the patient's person, as means of subduing inflammation—matters not receiving (he thinks) sufficient attention from practitioners. "Even the arrangement of the furniture should be attended to, on the principle that an agreeable impression, of whatever kind, is more conducive to comfort and recovery than one of an opposite character." If the wall-papers could be changed to colours and patterns more pleasing, with as little trouble as the bed-hangings and the chair-covers, it might cheer the sufferer onward in his progress to health. Who has not heard of a patient complaining that he sees faces on the walls, or that the figures are starting from their places? "No persons should be permitted to be about the patient, except such as are absolutely necessary to nurse him. Many a patient is killed by the kindness of his friends and relatives." Indeed, unreflecting, misjudging kindness is often in its effects far worse than the want of it.

In the treatment he recommends for inflammation, and the fever attending it, his directions under every head are wonderfully minute, his practice inclining rather to a *polypharmacy*, constitutionally as well as locally. His list of local applications is long, and the potential ingredients are active; and we think he trenches too much on what would be thought the province of the pure physician in England. But perhaps practice is tending in the West to a single faculty as rapidly as it seems to be with us.

The diagnosis of *chronic abscess*, however easy in theory, is not always established without mistake, as the experience of too many of the best surgeons testifies. Indeed, we doubt whether there are many operators who would not acknowledge that once, or even more than once, they have found themselves doing something more than making a mere puncture for the evacuation of the contents.

"The only two affections with which it is at all likely to be confounded are encephaloid or aneurism; but from these it may usually be easily distinguished by the history of the case, the fluctuating character of the swelling, and, if necessary, the use of the exploring needle."

The history, if it could be always had, might aid the diagnosis. The patient seldom has the amount of observation necessary to guide the surgeon unerringly, and if the fluctuation of purulent matter could be always felt, there would be no need of a puncture with the needle—an instrument of which he tells us in another place that "a large volume might be filled, if one had time, with a rehearsal of the mischief that has been committed by it in the hands of careless and unscrupulous practitioners."

Upon the subject of *malignant tumours*, Dr. Gross has nothing to add to our knowledge of this "subject, unfortunately sufficiently obscure." More was hoped from the microscope than it has done for the advancement of surgical pathology: "it is often a valuable auxiliary, but nothing more." It comes too late to aid the diagnosis. His treatment, general and local, dietetic and palliative, in the occult and in the ulcerative stage, is judicious, and the reader will be pleased in receiving from the other side of the Atlantic a condemnation of all caustic applications. Of congelation he does not express an opinion. We confess to a wish to see a larger and more extensive trial of cold as a surgical agent, before it is allowed to pass from professional memory. In regard to extirpation, he says:

"The cases which have done best in my own hands, after operation, were females with scirrhous breasts, which, after having been long in a quiescent state, at length assumed a threatening ulcerative tendency, or which had actually, in a slight degree, yielded to this process."

His description of the *scrofulous ulcer*, the surface, the secretion, the edge, and the parts around, is too long to extract. It is a clear and accurate description of an ulcer strongly marked, of very frequent occurrence, of which specimens are always to be found in every ward of a hospital; and, common as it is, worthy of close study, being one with which the ulcers of other specific diseases are often confounded, and from which they are sometimes with difficulty distinguishable.

The subject of *metallic sutures*, now exciting attention among us, comes under his consideration when treating of wounds. He says, in his own limited experience, he has not realized from them the great advantages which other surgeons have been led from their own observation to ascribe to them. "The introduction of the wire is easy enough, but its withdrawal is often attended with great inconvenience and even risk of tearing the imperfectly-united edges of the wound completely asunder. Nor am I satisfied that it is less irritating than the properly-prepared silk or linen thread." The inconvenience we have ourselves found in withdrawing it has arisen from the less yielding character of the wire, and the removal, especially in highly sensitive parts, as the lip, is often attended with very great pain to the patients.

The following piece of natural history may be new to some of our readers:

"It is supposed that many of our Indian tribes poison their arrows, so as to inflict a more deadly wound; but I am informed by an old pupil—Dr. W. F. Edgar, of the United States army—that this practice is peculiar to the savages inhabiting the mountainous regions watered by Pitt River, one of the north branches of the Sacramento. These people, it is said, use the poison of the rattlesnake, by grinding the dried head of the reptile into an impalpable powder, which is then applied by means of the putrid blood and flesh of the dog to the point of the weapon, the wound of which proves speedily mortal."

For another extract ("Tooth-wounds") we make no apology, as being an example of injury more frequently arising in the American mode of life than in ours. "Wounds inflicted by the bite of the human subject," he tells us, "are by no means uncommon, and from the danger which so often attends them, are worthy of more attention than they have hitherto received. I have seen quite a number of cases of severe suffering occasioned by wounds received upon the fingers in the act of striking persons upon the mouth." He relates several cases; one of them in "a distinguished jurist," in whom erysipelas followed; the hand was amputated, disease reappeared in the stump, and necessitated the removal of the arm. "In several," he adds, "I have experienced much trouble in saving life and limb."

The word *syphilis* he applies, as other writers do, to all forms of disease—*penal* diseases, they might be called—supposed to have their origin from impure sexual intercourse. He does not distinguish with more accuracy than many of his predecessors, the sequelæ of the different primary sores which are followed by secondary affections; the characteristic eruption, for instance, loss of hair, and nocturnal pains of the hard primary sore, which does not produce pus, from the protean secondary symptoms of the phagedænic poison, less regular in their course, and more formidable in their ravages. A chapter on venereal diseases in a comprehensive work on surgery would not be complete without some reference to John Hunter. Accurate observer as he was, Hunter no doubt confounded together several primary sores of different specific character. The more accurate observation of the present day endeavours to establish a separate diagnosis; and, before attempting a cure by a single remedy, or by a routine administration of remedies of acknowledged virtue, should be able to point out with accuracy the sequelæ of each, the course of the original sore, and, where there are such, the appearances and order of the several sequelæ.

The twelfth chapter, on "General Diagnosis," comprises general directions for the examination of the different organs, and the mode in which it should be conducted. It will repay the reader for a careful and attentive perusal.

The different methods, still in vogue, of *amputating the extremities*, and the *mortality* after the operation, occupy some space in the first volume. Our author describes the different methods, on which so much discussion has taken place with so little approach to uniformity of opinion, but makes no estimate of their comparative value as therapeutic agents. In his own practice he prefers the operation by flaps, which he thinks more easy of performance.

The circular operation, as now performed by double incision, is that which for the longest period of time has received the general approbation of surgeons. By it a smaller amount of injury is inflicted on the soft parts, and it makes a smaller wound. Generally it is attended with less flow of blood, the mouths of the divided vessels contract more perfectly, and a smaller number of vessels require ligature. But it requires the exercise of a little more judgment on the part of the surgeon—a nicer calculation in admeasurement, to secure an efficient covering for the end of the bone. It certainly is not according to our experience, what some instrument-makers tell us, as quoted by Dr. Gross, that stumps made by the circular operation seldom answer well for the adaptation of an artificial substitute. If it was, we should be tempted, when the difficulty arose, to seek the aid of others who had the faculty of being able to fit an artificial substitute to the stump presented to them.

The operation by flaps is done more readily; it takes some few seconds less in time, and there is less danger of the inexperienced operator failing to secure a sufficiency of soft parts for protection of the end of the bone—the one thing above all others required for the future comfort of the patient and the adaptation of an artificial limb. "Many amputations have I seen villainously done," said a late inspector of army hospitals; "but they all turned out well if only the bone was sawn high enough."

Dr. Gross remarks:—"It has long been known that the danger of the operation is greater in proportion to its proximity to the trunk, and the size of the limb." We are inclined to think that the danger to the patient arises rather from the injury or disease, for which the operation is desired, having its seat so near the trunk. If it should ever become the fashion—and who shall say it will not?—to amputate at the hip-joint for disease of the knee, we venture to say that the mortality following that operation will be much less than in the cases to which the operation is now for the most part restricted—those, namely, in which the disease cannot be removed by any operation through the thigh itself, and where the amputation is, often of necessity, done through unsound parts.

In the great operations, where an opportunity is afforded by the publicity attending cases occurring in the public hospitals, it might be worth while to institute an inquiry into the amount and causes of the mortality arising among cases in similar circumstances under the hands of different surgeons of the same institution, where there exists the same atmosphere, the same general routine of domestic economy, the same nurses, the same dressers and pupils, the same governors controlling, or may be helping, the *adjuvantia.* The field of inquiry has been too restricted. Fuller authentic reports from the larger hospitals should be periodically laid before the Profession. We wish to see the causes of death after these operations investigated with the zeal and industry and the success with which the mortality in hernia has been investigated, in the belief that it may equally lead to improvements in practice.

The chapter on "Excision of Bones and Joints" is short. It would seem that the knowledge of this branch of surgery is not in an advanced stage. "In this country," Dr. Gross remarks, "excision of the joints has hitherto been greatly neglected, both in hospital and private practice."

To an American author one looks naturally for a full account of *anæsthesia:* it is due to American surgery that we should acknowledge our obligation for this great and wonderful innovation. An interesting history is given of the unhappy originator, Dr. Wells, whose first experiment was made on himself with nitrous oxide gas in 1844. The knowledge that inhaling the gas rendered the patient insensible to the pain of a surgical operation had been publicly taught in one, at least, of the medical schools in London nearly thirty years before this. But we were told no more than that it had been "tried;" so had other means. Large doses of opium had been given by practitioners,—rather, we suspect, with the view of overcoming the expected resistance of the patient in a given case, than with the intention of relieving the pain of the operation. Dr. Gross went beyond this. He was in the habit of employing it, either alone, or with antimony, for many years in almost every case that fell into his hands. "I became very fond of the practice, and never, so far as I could determine, experienced any bad effects from it. On the contrary, I know that it was commonly productive of great benefit, not only blunting sensibility, but preventing shock, and consequently severe reaction."

The effect of drunkenness in deadening pain had been observed. Many an amputation has been done during drunkenness, and the patient has known nothing of it. But experience testified that it was far from being universally so. In one of the midland counties, a family of "bone-setters," who had great reputation in restoring contracted joints, used to make the patients drunk before applying forcible extension to break down the adhesions: and it has been known that midwives have had recourse to the same practice to relieve the pains of labour. Drunkenness was too uncertain in its action as an anæsthetic to be admissible as an agent of acknowledged efficacy.

The apparent removal of consciousness by mesmeric "passes" (whatever its success with the "mild Hindoo") failed, with us, to temper the sharpness of a serious operation: the single case brought publicly before the profession having been afterwards acknowledged to be an imposture. But that the imagination *can* be so far abstracted as to allow some degree of pain to be inflicted unfelt, the well-authenticated case of the late Lord Metcalfe will show. Besides, many of us, since the introduction of ether and chloroform, have seen a patient *quieted* during the latter part of an operation by breathing through the empty sponge of the inhaler.

"The progress of civilization and the improvements in the arts and sciences," Dr. Gross observes, "have greatly multiplied the frequency and severity of *burns and scalds*. [*e.g.* "On our Western waters, where steamboat explosions are of almost daily occurrence, many persons are annually destroyed by the effects of hot water."] I am satisfied that few practitioners understand their character, or treat them with the success of which they are capable." The subject is discussed fully, with the different treatment he considers applicable to the injuries in different degrees of severity, and in the various stages of their progress.

Tetanus has been found fatal under Dr. Gross's care. "In an experience of thirty-one years I have seen but two cases where the patient escaped with his life; and then only after a protracted and painful struggle." In an acute disease so formidable and so fatal, when the anæsthetic effects of ether and chloroform were fully established, surgeons naturally looked to inhalation as the one remedy yet untried, and from which benefit might be hoped. The experience of each individual surgeon in the thirteen years which have passed since its introduction is necessarily limited; and none can have had many opportunities of watching its effects. It does not appear that Dr. Gross has himself used it. He says, notwithstanding the reports published by different observers, "it is certain it has generally signally failed to cure." Under our own observation, it has seemed to us that the violence of the spasm and the paroxysms of pain have been lessened; but the attacks returned at shorter intervals, and the patient has become sooner exhausted by their repetition. The euthanasia was successful.

The subject of *aneurism* is treated at length; Dr. Gross gives a valuable analysis of our present knowledge of the disease. From his report, spontaneous aneurism seems to be of rare occurrence among our kinsmen in North America; and his own opportunities of personal observation—large as his experience has been in public and private practice—have consequently been limited. He gives the preference in general to the treatment by instrumental compression: but he considers that digital compression should be tried more extensively than has yet been done, in aneurisms of the extremities. He has not had an opportunity of trying manipulation. The directions for performing the operations for tying the different arteries are not different from what will be found in standard works of surgical anatomy; but they are given in the language of a surgeon familiar from frequent practice with the anatomy of the living subject.

The principles of treatment in *diseases* and *injuries of the joints* are briefly, but clearly stated. Perfect repose during inflammation is of the utmost importance: "the rest must be absolute and unconditional." If matter should form within a joint, it must be evacuated: "the incision need not, nay must not, be direct, but subcutaneous, small, not large; and when this precaution is observed, and the orifice is immediately closed to prevent admittance of the air, nothing but good can result from it. . . . . The opening is of course made at a dependent part, and is repeated from time to time, until the matter ceases to accumulate: the joint being well supported in the interval by the bandage, or by a roller and adhesive strips." In chronic disease Dr. Gross has found the mildest modes of counter-irritation sufficient in his own practice to promote a cure. He has not been so bold as to use injections. Constitutional treatment of course is not to be neglected.

Dislocations occupy more than a hundred pages. The importance of correct and early diagnosis is pointed out, and the necessity for making a thorough examination of the parts concerned, under the influence of anæsthetics, if necessary. The chief obstacle to the reduction, he thinks, does not arise from the resistance offered by the muscles, but from the prominences of the bones and the ligaments; "the resistance is most striking in the ginglymoïd articulations, owing to the greater complexity of their structure and their larger size, but more especially to the greater number and bulk of the neighbouring prominences and depressions, thus permitting the displaced bone to become more readily interlocked with the fixed one." In dislocation of the hip, "the chief impediment to restoration is not, as was formerly supposed, the contraction of the muscles that are affected by the accident, but the indirect action of the muscles that are put upon the stretch by the malposition of the dislocated bone; and the operation may always be safely, certainly, and expeditiously performed, simply by manual effort, without any apparatus, pulleys, or, in short, any extraneous aid whatever."

We pass from the diseases of the bones and their appendages, with which the second volume opens, to the subject of *fractures*, which he regards as the most trying and difficult branch of surgery to practise successfully.

> "As for myself, I never treat a case of fracture, however simple, without a feeling of the deepest anxiety in regard to its ultimate issue. I cannot retire at night, or rise in the morning, without a sense of discomfort, so long as I am conscious that, despite my most assiduous attention and my best-directed efforts, my patient is likely to become deformed and lamed for life."

Perhaps his anxiety may arise in part from his knowledge of the frequency with which law-suits are waged about charges of malpractice. Indeed, the whole of the long section is written with a caution and an amount of closeness of observation likely to be the result of long and extensive practice in consultation, in a country where we hear of practitioners requiring a bond of indemnity from a patient, before undertaking the treatment of his case.

In the division of fractures, as in dislocations, he adopts the French distinction into simple and *complicated*, in preference to the two long adopted in England. We confess we prefer the terms so well known in England. To us it seems that the word *complicated* adds nothing definite to the term fracture, beyond saying that there is other mischief beside the "solution of continuity in the osseous tissue."

It is not sufficiently descriptive, and it would seem to be objectionable, because "it is easy to comprise under this denomination every form of accident that can possibly arise, either at the moment of the injury or during the progress of the treatment."

The English reader will be surprised to find that among the various splints, beds, and apparatus of different kinds, proposed for the treatment of fractures of the shaft of the thigh-bone, the one which is more commonly used among us than (we may venture to say) all the others put together, is not mentioned by Dr. Gross. We mean the "long splint," or Liston's, as it is sometimes called, having been chiefly brought into notice by the late Mr. Liston. The omission seems the more remarkable, as the splint was originally, if we remember right, the invention of an American surgeon. The former "long splint," or Desault's, on which it was an improvement, reached only to the trochanter, and counter-extension could not be maintained with it. This defect is remedied in the modern "long splint."

The injurious, and even fatal effects, which are the result of *concussion of the brain*, when the violence is applied indirectly, as when a person falling from a considerable height alights upon his feet, knees, or buttocks, are illustrated by a comparison which we have not ourselves had an opportunity of making.

"Here the force of the injury is transmitted along the bones of the extremities and of the spine to the base of the skull, where exploding, it is communicated to the brain, very much in the same manner as when the head is struck with a hard body, as a bludgeon, poker, or brick. The effect of this form of concussion may be illustrated by what occurs in the boyish amusement of killing woodpeckers, in countries where cherries abound. To prevent the depredations of these marauders, a slender pole is sunk into the earth, its head protruding at the top of the tree. When the bird alights, the pole is struck with an axe, and the vibratory motion thus transmitted through the pole to his body kills him in an instant."

The following remarks upon the use of *opium* in *inflammation of the brain*, though rather opposed to the general opinion, seem to be judicious:

"It is by no means established that opiates, judiciously administered, produce cerebral congestion; and even supposing that they did, the occurrence would be no contra-indication to their exhibition. If they produce congestion at all, the congestion is of a passive, and not of an active character, and therefore comparatively harmless. But I do not look upon the matter in this light —on the contrary, I believe that anodynes, by controlling the heart's action, exert a direct and positive influence in controlling inflammation of the brain, by placing the organ in a state of repose, so essential in every case of disease and injury, no matter how induced or where occurring. The brain in the normal state rises and descends with every movement of the left ventricle of the heart; in injury, this action is greatly increased, becoming often quite tumultuous and overwhelming; the nervous pulp receives a shock at each pulsation; it is never at rest, and has, therefore, no opportunity to repair the mischief that has been inflicted upon it. Now, the object of the anodyne is to insure this result by paralysing the heart, and thus rendering it unable to send to the brain the accustomed quantity of blood. If this mode of reasoning be correct, it follows that the wounded organ, receiving less blood than usual, will be less prone to inflammation. We can secure repose for it in no other way.

We may carry the inflamed hand in a sling, and apply splints to the inflamed leg, but we can insure tranquillity to the brain, heart, lungs, stomach, bowels, and peritoneum only by the use of anodynes. But these remedies do good in another capacity, under these circumstances. They induce sleep, allay pain, and quiet the mind—effects which cannot fail to promote recovery, when, from the frightful nature of the injury, recovery is not impossible."

Trusses form an important article in the palliative treatment of reducible *hernia*, in some cases leading, though slowly, to the permanent cure. Dr. Gross gives the preference to Chase's. We see the same objection to this as to most of the trusses for inguinal or femoral hernia which are formed of a spring encircling the pelvis. While great mechanical ingenuity is applied to the construction of the pad, that it should fit itself exactly to the rings, and to the curve and the neck that it should rest exactly in the groin and bend of the thigh, it seems to us that the necessity for fitting the spring to the pelvis is overlooked. This object would be better attained if the posterior part of the spring were bent, so as to describe a transverse section of a cone rather than a section of a cylinder, which is the form of most of the serpent trusses. The value of a truss as a preventive of the more serious evils of strangulation has been tested at one hospital on a large scale. In a large town in an agricultural county, where the operation for strangulated hernia was not uncommon, the governors some few years ago resolved to supply trusses to every patient applying, if recommended by the surgeon in attendance. We have been told that the operation for strangulated hernia has not been performed in the hospital since this order has been carried into execution.

Gerdy's and Wutzer's operations are briefly discussed, with some modifications by American surgeons. Dr. Gross relates the following successful operation for the radical cure of a femoral hernia:

"The late Dr. Jameson of Baltimore many years ago performed an operation for the radical cure of a femoral hernia, in the case of a young lady, by dissecting up a tongue-like flap of integument, from the neighbourhood of Poupart's ligament, and inserting its base, which was fully three-quarters of an inch in width, into the femoral canal. The edges of the wound were drawn together over the flap, by several sutures. For a few days the patient was restless and annoyed by vomiting; and, although the parts did not all unite by the first intention, yet they soon got well, the transplanted integument contracting into a hard knot over the femoral ring, which was thus completely closed, the recovery being perfect. I am not aware that this operation has ever been repeated."

The mode of *removal of concretions from the bladder* has of late years received increased attention. Lithotrity, a modern improvement, does not appear to stand so high in the author's estimation as a means of cure, as it does in Europe. Of the different modes of lithotomy, he gives the preference to the lateral, as now usually performed; the successful results of the operations in America merit the praise he gives. "Of 895 cases in the practice, chiefly private, of American surgeons, 851 were cured, and 44 died; making a proportion of 1 in $20\frac{1}{3}$." A slight notice only of the median operation is given. Time alone must show whether this is entitled permanently to the confidence of surgeons: it is a more simple road into the bladder, but it is a longer one.

The number of mechanical contrivances, not all of simple construction, and the many operations, not all easy of execution, recommended in modern times for the relief or permanent cure of *stricture of the urethra* testify to the difficulty of the work to be done, as well as to the want of uniform success in the attempts. The author's conviction, from ample experience, is that the very best instrument for dilating a stricture is the common silver catheter with a slightly conical point. Much must depend on the class of patients who come under treatment, and their willingness or ability to submit themselves to the discipline which is needed to give effect to any treatment. The perineal section of Mr. Syme is required in some intractable cases; when the cases are well selected and the operation is properly executed, the author believes the effects are generally all that could be desired.

The text is illustrated by a large number of wood engravings, the greater part original, the work of American artists; some are from the standard English authors: the reader will recognise the familiar sketch in many of them, the evergreens of pictorial surgery. It seems a pity that these woodcuts are so often reproduced, without any author taking the trouble to have the errors in the original blocks corrected. We will mention some. In vol. i. fig. 51, the knot is *upon* the line of incision. In fig. 128 the strap of the tourniquet is passed wrongly through the lower platform. In fig. 267 the surgeon is making extension at a strange waste of mechanical power. In many of the amputations (vol. ii.), the blade of the knife is out of all proportion to the size of the limb under operation. At p. 801 the patient in the text "lies on his side" during paracentesis: the engraving represents a female subject in the erect posture.

The type is good, and the compositor's work is well done: throughout the whole of it we do not remember to have detected a misprint. A few hard words, as we went through the pages, struck us as being scarcely *yet* naturalized:—timeously, sakelessly, lymphatitis, lymphization, fibrinization, localization, horizontalized, irreducibility, incipiency, vulnerating, repullulating, pilous matter, vessels ligated, and bone exsected; spicule, fistule, burse, and sequester may be added.

The index placed at the end of the second volume is copious and well arranged.

The author's style is easy in the reading; he seems to have a clear understanding of what he means, and his language is intelligible. His descriptions of diseases and their symptoms are good and practical; he deals in principles, in descriptions of disease, and approved modes of treatment. There is an absence of the hardness which disfigures much of the scientific writing of the day; and there is a notable absence —a pleasing one—of the frequent appearance of the pronoun of the first person.

Review VII.

Modern Medicine, its Aims and Tendencies. The President's Address at the Twenty-eighth Anniversary Meeting of the British Medical Association at Torquay, August 1st, 2nd, 3rd, 1860.

Medical men for some years past have fallen so much into the way of haranguing each other on all possible occasions, whenever a society can be instituted, an anniversary invented, or a school set a-going, that there is a call in honour on those who report their eloquence to say a word for the silent section of the brotherhood, who, by renouncing their turn of talk, consent to forego a modicum of their significance. Publicists of this class, for the most part, escape notice from the critics, or in any case are but loosely questioned. Yet, as we are prepared to maintain, no set of men who frequent the highways of medical literature should be more carefully looked after. They begin by collecting a crowd. They claim to be heard by appeals to principle, and on matters of more than individual interest. They speak with a quasi-authority, *ex cathedrâ*, from a platform. As a class of compositions, these monologues, voluntary or imposed, are at this present date fairly below the mark. Yet in every year's succeeding crop, there are many that have good stuff in them, with plenty of it. The criticism hitherto spared to them is seldom such as readers are content to suffer, or authors care to receive. It assumes for both a twofold foregone indifference. It offends in either extreme. It is flimsy in its selection, or stereotyped in its generality. By closer attention on the part of those whose duty it is to report on the published opinions and proceedings of our profession, this department of our literature would at once assume its due importance, and on all suitable occasions might be improved into a chronicle of our legitimate gains in practical science with a special facility for their rapid and general diffusion.

Thus thinking, we hold that no class of publications should be more critically examined by those who undertake the censorship of our spoken or written thoughts, than the monologues addressed, under any or whatever denomination, to an assembly, in the flesh, of working medical men.

Want of industry in the collection of materials for a "Discourse from the Chair," or carelessness in their arrangement when collected, are slights unmistakeable on the courtesy and listening faculty of those who have volunteered their attendance in support of the speaker. Mystification and sophistry are a denial of their understanding. Tediousness is a positive cruelty. Irrelevant prettinesses, sentiment, and platitudes are outrages, never to be forgiven, on their literary taste. And these remarks we do not hesitate to extend, in all their strictness, to lectures, single or in the course. Every lecture is in truth a publication, and should be dealt with accordingly. If not good enough to be brought to book, it should not be delivered. Of serial lectures, who would gainsay us, for the cravings of wholesome professional appetite, there has been of late years a more than sufficient supply.

They have, indeed, become the staple of medical journalism; yet how seldom is their presence noted or their quality tested by the critic of the company. Admitted as "advertisements," do they claim a right of undisputed footing? Proclaimed as "lectures," are they received as edicts, and submitted to without reserve? It is precisely this prestige of quasi-authority that should bring them under the closest and most jealous scrutiny. Their authors too often presume on it in their venture of statement and inference. Their not unfrequent plagiarisms from those who sometimes talk but never print, would assuredly by exposure be somewhat held in check. It has gone hard with certain of us to stand by, week after week, while these pilferings have been in progress; in the end, perchance, to see our own damaged goods put up for sale, without acknowledgment, by the enterprising appropriator. In this the normal lawlessness of regular medicine is the safe extension of pretended physic.

Lecturers, frequent and ready as chairs for their sitting, earnest, accomplished; incompetent, careless, antagonistic! The resulting doctrine ("Principles" of our School Prospectus), one of corresponding variety in patient and practitioner, reflected through successive generations of students, in every possible tint and shadow of inharmonious design. A central supervision, searching and persistent, of lecturers and the lecture-room, beginning with an entire revision of the lecture-system, is an urgent present need, and would be accepted by the Profession in full equivalent of the poll-tax levy by their parliamentary council. In the mean time, every lecture that can be caught in print should be strictly interrogated by the sitting authorities; and if not able to give a good account of itself, should be made to suffer for its share of the general scandal. Let the chairman, president, orator, lecturer, who claims an hour's silence from any number of his co-professionals, that he, and he alone, may talk, be made to understand for the future, that if he does less than his best, his very best, to interest and instruct his audience, he is guilty of a rudeness, and has betrayed a trust. Moreover, let him know, that if having conciliated their attention and approval, he be induced, "by request," to print his utterances, he unfailingly challenges the attention of the professional reviewers. Councils, colleges, colleagues, and committees, under such supervision, would, we may hope, become careful in requesting the publication of lectures or addresses to which, having lent an official ear, they desired to affix their official stamp. And previously to such publication they would not hesitate to exercise, within limits, a privilege of critical revision. Many an address, need we say, that has been triumphant in its delivery, has, to the dismay of orator and audience, been hopelessly flattened by the press, irretrievably damped by the printer's ink. Thus nursing our belief, we read with curious attention the little terminal paragraph affixed by our editor to the last page of the January number of our Journal:—"The Editor is particularly desirous of having all Reports of Hospitals, Asylums, Sanitary Boards, Scientific Societies, &c., forwarded to him; as also Inaugural Lectures, Theses, Medical and Scientific Addresses, &c." A proclaimed high festival of paper waifs and strays! A consolidation, under summons, of incongruous floating atoms! On what literary aim and purpose? To what professional end? That our editor is already rich in that which he professes to covet, who of the monad press can doubt? Who would refuse a penny to Cæsar—a small coin from his own particular mint to a great collector? When he shall have raised his paper pyramid, the only convenient design for pamphlet structures, what and where will be his profit from the pile? We are not in the secret, but we have our guesses. In the mean time, having intercepted one of the flying sheets on its way, we propose to decipher, with our readers, whatever of meaning may lie beneath its wing.

'Modern Medicine: its Aims and Tendencies.' "The President's Address to the British Medical Association, assembled at Torquay in the Autumn of 1860." A challenge how safe for notice by our warder, the Publisher! A company how secure of welcome and honourable entertainment by our seneschal, the Editor! "Modern Medicine: its Aims and Tendencies!" The very motto of our brain! The work ever ready to our hand! Room for the swelling theme! Is there enough of verge in our entire trimestral sheet for its long array of statement, its ponderous store of facts, its history, its logic in the main, its wide-spreading conclusions, with their light attendant retinue of inference, suggestion, or illustration? A glance at the task before us, and we are reassured. Modern medicine is presented to us in the form and within the limits of a small octavo pamphlet. The "Aims and Tendencies" have no wider range than a few lines in a few pages can here and there be made to afford. Our readers will submit more cheerfully than ourselves to the reduced scale of critical remark which, in fitness of proportion, attaches to an essay so inordinately small on a subject which is never less than inconveniently large. It will be with exceeding difficulty that we shall in any way conform to the Torquay standard of physic in its widest sense.

After a few modest phrases of conventional humility, our author takes the chair assigned to him as President of the two thousand three hundred wandering Associates, "who represent rather than any other medical corporate body the great bulk of the profession." Having, *suo motu*, proposed, seconded, and carried a resolution, that "of the several medical corporate bodies," the "British Medical Association alone comprises within its ranks representatives on equal terms of each," and having thereupon assumed a "fair expectation" that the said Association should be "more free from bias and prejudice, and to that extent more fitted to form a dispassionate judgment concerning the profession at large, than any other medical body less universal in its constitution," the Speaker of our South Devon parliament at once quits his chair, and resolves his council and senate into a committee of the whole House, to "recapitulate to themselves the existing condition, aims, and tendencies of medicine;" and secondly, "to consider briefly together what is the position of the profession at the present moment in the estimation of the public and in the estimation of its followers."

The Report on the last year's work, and promised undertakings of this great Society, is mentioned only to be dismissed as devolving, in course of business, on their council.

And now, fairly engaged with the President and his Torquay excursionists, we range at rapid pace through general, social, commercial, and political science, physics, metaphysics, law, religion, and theology, until, having left five pages of the pamphlet behind us, we catch sight of our home terminus in modern British medicine.

And here our work begins.

The President's medicine must be construed in the large sense of General and Minute Physiology, Animal Chemistry, Therapeutics, Surgery, Sanitary Science, Morbid and Microscopic Anatomy. This multiform medicine, we are told, "has made more way, gained more facts, obtained more sound opinions towards the alleviation of human suffering and the removal of disease during the present century than in any time before." Certainly, since the century began, we have grown continuously and considerably. Within the last quarter of a century, "rather more than which this Association has flourished," the shoot has been wonderful. The evidence is just now at its best. There never will be more witnesses ready and willing to speak to the fact. Medicine, Scholastic General Medicine, is much more largely developed; but not in full and equal proportion throughout its several parts. As student physicians, we confessedly find much help from the facts accumulated by "Minute Physiology," correct "Animal Chemistry, and Microscopic Anatomy," in the acquisition and arrangement of symptoms which, pretenders as we are, we are pleased to designate as Diagnosis. In the treatment by internal remedies of internal disease (our President, in his notices of practical medicine, is never more or less than a physician), it would be hard to prove that unsound patients had profited more by sound opinions during the present century than in all time before. In any case, this assumed superiority of modern practical medicine is but the latest development of knowledge bequeathed to us by the working physicians of past ages, a further result of the rules of observation and comparison begun by Hippocrates, and transmitted through Aretæus, Celsus, Harvey, Sydenham, and others of like stamp, to ourselves. After all, we are but continuations. And here, in the fear of omission, let us acknowledge, as we are bound to do, how much the physic of late years has gained in power and consideration from the emulous researches, the enchanting discoveries, and refined contrivances of scientific pharmacy. No hint is given of this our chief ancillary support, in the Torquay Address. In one paragraph only, and here with a perversion, is any allusion made to what advanced pharmaceutical chemistry has done for us and our patients. "But one glorious achievement this age has seen which no poet's fancy would have dared to promise, painless operations. And within a year, the great discoverer of chloroform, who by that boon to humanity deprived operations of their present pain, has done much to lessen their after danger."

Now this is not fair. The glory of banishing pain, with its recollected horror, from the surgeon's world, is due to the Boston physicians working in council with their assessor-experimental chemists. They were the great discoverers—æther, not chloroform, was the great discovery.

Reverting to modern medicine, one word, a plain and honest one, for the old doctors, whom many of us have seen and known in the flesh. Baillie has prescribed for us; we have prescribed with him and with the elder Babington. It is truth when we declare that, were these physicians now as in their maturity available, we would trust to them in consultation, for charge of our health, under severe illness, with to the full as much confidence as to any of the many with whom we have been in later years associated, energetic, scientific, chemical, pathological, microscopical, and all popular as some of them have been. Bark, opium, mercury, iron, antimony, leeches, blistering fly, jalap, senna, the lancet, brandy, and the warm bath, were among the means which they employed, and to each and all of them they gave full employment in a fair turn. We would have trusted these men, we have said, with the care of our health, for it was especially health for which they cared; not so much for the disordered organ, with its damaged segments and interrupted functions, as for the remaining sound structure, with its healthy action, protective and of repair. They prescribed widely, on the general inference, from a wide survey of the symptoms on the best valuation they could make from pulse, countenance, tone, manner, tint, posture, from all in combination and result; from a general calculation of material and power, used, wasted, deteriorated, or in reserve; from their experience of the prescribed remedy in past cases, similar, or analogous; not from minute microscopic or chemical observation of certain textures and products. Their decision was on the upshot of what was missing or still in store. They prescribed much as we do now, when we order quinine for an ague, mercury for syphilis, or iodide of potassium for mercurial rheumatism, with no direct reference to the ultimate processes of nutrition, cellular, electric, or endosmotic. How little, or ever, do we concern ourselves with these atomic truths in the practical adaptation of our remedies! We use them sparingly in our diagnosis, and have done with them. In a consultation of physicians, who is he who would venture to suggest any new arrangement, by prescription, of the relations between the cell and its blastema in a given case of organic disease?

Chemical medicine may claim a separate notice. It is in a category of its own. The hero specialists of modern fashionable physic, the chemist physicians, claim for their remedies a large and direct reagency on the secretions of the organs which they hold in exclusive vested charge; but direct interference, by prescription, with the chemical agencies of the living blood in the living structure, will, in our firm belief, do more harm than good in too many instances. Improved animal chemistry did not prevent the orthodox poisonings by the salts of mercury in the decade of 1835—1845, when the "bold, energetic" treatment of "regular practice" had reached its zenith. It did not protect us from the renewed outrage of "cinchonism" some six or eight years ago. And just now it is affording us no facility of appeal from the encroachments of strychnine and of iodine in all its forms, from the rapidly mounting abuse of iron, of the metals generally, and the alkaloids. Having thus glorified "modern medicine" at the expense and to the discredit of the old-fashioned physic; having enshrined it with physiology, animal chemistry, and microscopic pathology, our President throws open the sacellum and invites the general public to an act of worship. There is a new Avatar! Alas! they answer with more than denial;—with revilings. And he, the high priest of the temple, he of all men is employed as their mouthpiece and interpreter. By him, their President, the most universal of all the votaries of modern medicine are made to hear that—"To the public gaze the errors and corrections of our profession are more manifest than its progress, which being in truth a reality, is in semblance for the most part a contradiction." Nay, "the very vitality and greater excellence of medicine have entailed for the moment a diminution in the confidence of the uninitiated," and thus medicine is "held in less respect by the public at large for the very reason that it is more scientific, more exact, and in every way more deserving of respect than ever." Here one would have thought that the British Medical Association would, in self-respect, at once have passed to the order of the day. It is with painful surprise (we honour and admire the Association) that some pages in advance we find them still under public penance, their President consenting without demur to the conventional administrative duty, and after every lash supplying the grateful recipients with a superfluity of reasons for the infliction. That a large number of intelligent, active, well-to-do professional men, paying their own expenses, should assemble for such a purpose on a summer morning, from all parts of England, on the shores of South Devon, and wish for nothing better! *C'est impayable.* The phrase must be forgiven, there is no other. In support of this castigation from the public it is pleaded by their advocate, the President, that "medical men are discordant in their views," that "they assert opposite and contradictory opinions with equal dogmatism," that "they designate the same disease by a plurality of learned names." They differ, "at times, stupidly and dictatorially, from the rest of the world, as by affirming that vaccination has not diminished small-pox." They "espouse medical crotchets" of the most opposite kind, denying mortal danger where it is imminent, asserting it where there is none. A heavy score, as it seems, but capable of comfort, admitting a compromise. A short sentence from the chief of the order, who, having dictated, receives the confession; and the end is absolution. "But such weaknesses of the wise have ever been?" (p. 15.)

The next issue which modern medicine is called on to plead is in her Majesty's law courts; with a jury to decide. The Torquay President has his clients up before the judges, again to take public measure of their attainments, to test their capacity for evidence, and especially to make out what the lawyers think and say about them. Dealing fairly enough, as it seems to us, with the imputation so frequent in certain forensic inquiries, of medical ignorance and incapacity, he rather unduly exaggerates the impression supposed to remain from such imputation on the public mind. The scandal of our inveterate discords in the witness-box is by no means so great as the President seems to imagine: much of this attaches to us only as far as we unduly assume the duties and responsibilities of analytical chemists. Of character, and the power that goes with it, we have collectively earned to the full as much as accrues to the lawyers themselves from the issues that have concerned us both in the courts of criminal jurisprudence. To the uneducated and imaginative the sagacity of the doctors in tracking the murderer by a blood drop, as in obtaining arsenic from the inner tissues of his victim after the concealment of years, must appear as gifts that are supernatural. How much of deadly purpose is, even as we write, being now prevented by this widely-diffused feeling of awe for the power of detective medicine?

Within the last thirty years, the lawyers have been among our best instructors. We owe to them our arms of precision, such as, from the nature of our service, we are competent to use. They will unfailingly superintend us in the necessary practice. Cross-examination in the witness-box has helped us more towards precision of thought in practical medicine, than any extent of dissertation to which we may have listened in the lecture-room. There is much of elective affinity between the bar, the bench, and the bedside. What, as physicians, we specially need in examining and determining a complicated case of disease, is the lawyer's faculty of taking depositions and sifting evidence. Sagacity, rather than cleverness, should be our joint device.

"Every therapeutical conclusion," it was well said at Torquay, "is of course liable to as many sources of fallacy, as it has elements to be considered; and it requires, therefore, pre-eminently a judicial frame of mind."

In large medical consultations how often has one wished for five minutes' sharp remark from a sensible lawyer! What untruthful reports, what confusion of statement, what "windbags" of theory and explanation, would be avoided if the parties, patient and consultant, could only be taken at once before a magistrate.

Lawyers of the better sort have more sympathy with our pursuits than is acknowledged by most other classes of men. When facts are not pressed upon them, they rather lend themselves to our mystifications, as in their ready acceptance on Smethurst's trial of a "certain peculiar expression of countenance" in evidence of poison at its work. The lawyer is occasionally set to look for truth, real or pretended, in the recesses of the human body as in those of the human heart. He associates our intellect and best energies with his own in the prosecution of his task. He appreciates equally with ourselves the exceeding difficulties of the search. He understands our frequent and inevitable failures. And the first among our fellow-workmen are the first to vindicate the many from the folly and presumption of the few.

In the private relations of law with physic we find abundant proof of the respect in which we are held by those with whom we are so often associated. By no class of our fellow-citizens are we better used than by the working gentlemen lawyers. They consult us frequently;

they receive us pleasantly; they defer to our opinion without "arguing the question," and they pay us liberally. "Exclusivism in theory," "exclusivism in treatment," "excessive trust in nature" for the cure of disease which is defined as "nature going astray," "abrogation of mystery," are further pleaded against us by the President in behalf of the public as so many special reasons for the disrespect in which we are held by our employers, and for their general want of faith in the advanced modern medicine which we profess. This bill of wrongs is not more remarkable for what it sets forth than for what it pretermits. The public, we may presume, silently approves of empiricism, finds no fault with vivisection, and does not object to over-medication. Nothing is said of our social errors on one side or the other, in explanation or aggravation of the quarrel between us. No hint escapes of our own sectarianism and false hero-worship, or of their ineffable ignorance, arrogance, and credulity. There is a gravamen in the general charge which we are bound to notice, as President and public are evidently here at one. "But beyond all other causes of weakened confidence in medicine is the avowed scepticism, in medical matters, of some of its followers." This text is expanded into an unshrinking avowal and elaborate defence of our common professional faith. Yet, in the good old days of "system making" and "hair-splitting," when the "pathology of the brain was merely guess-work," when "everything in physiology was referred to the smallest bloodvessels, with or without the special control of the nervous system, according to the fancies of the teacher," in that long period of time which preceded the birth of the Association, would not scepticism have been the true religion? It was never more angrily denounced. During the thirty years' war waged by regular medicine with living structure, when the blood, the fount of all our being, was poured forth *ad deliqium* for slight irregularities of the nutritive function denounced as inflammation,—when attenuated tender bowels were hurried into ulceration by incessant drastic purges, —when the land was rife with mercury, so that few escaped salivation, there were physicians who, on reflection, doubted. Would they be now condemned? The merit then was surely not with blind belief. Denouncing the reckless abuse of purging and bleeding in 1840, were we infidels? Refusing alcohol and cod-liver oil as universal specifics, still honouring in emergency the cupping-glass, leeches, and the lancet, are we thereby heretics?

"In point of fact, the great mass of medical practitioners do undeniably believe in the means they employ." An assumption that is far away from the fact, undeniable only by those who confound belief with credulity. The great mass of practitioners believe in physic, as in much besides. They take it for granted. It is their groove, and they keep to it. What we now hold of rational medicine we owe, not to the belief of the many, but to the wise perennial scepticism of a few. Would that in times past there had been a more lively faith of unbelief.

Such was the great cause that came off at Torquay, on August 1st, 1860, between modern medicine and the public of all nations; the

President of the British Medical Association acting as judge, jury, reporter, and counsel on both sides. As might be expected, being a domestic difference, it resolves itself by arbitration; and who so fit and so ready as the Torquay President to act as arbitrator? Thereupon it is agreed that, "So soon as we have acquired facts to substantiate our opinions, which are accepted by all who are competent to judge, so soon will our own opinions become as unanimous as, on questions which hinge on the balance of probabilities, opinions ever can be, and then, and not until then, will the public estimate our profession at its real value." When we do thus agree, our unanimity will indeed be wonderful. Our "real value in public estimation" remains ever, and as before, an unknown quantity.

We have, as in duty bound, presented to our readers all that we have been able to extract of kernel from the President's address. How of the rest? There is no husk. It is all pulp and rind. "Preventive medicine," "statistical medicine," "middle-class examinations," "Devonian geology," "educational reform," "social and sanitary science," "Dr. Humphrey and the Cambridge hostel," each and all, have a civil word said for them, generally sensible and to the purpose.

"As to the purely professional portion of a medical student's education, all we can say is, let it alone. It is already as good as it need be, and it will keep pace with the times." Are these true words? Do we hear them as they were spoken? "Let it alone!" Poor lads, they are in truth too much let alone. "As good as it need be!" It is the very model of imperfection, and keeps pace with the times only as a kibe with the footsore. "Touching medical politics," the President handles them not. We stand apart—how willingly—and let them go. In the matter of statistics, he is shy of the figures. On the medical peerage question, he is not with the flunkeys.

Of the oration in its completeness, it may still be asked, Was it necessary? Is it convenient? Will anything come of it? It deserves this inquiry, for it is earnest and honest throughout, well-meaning, if not in all respects well-considered. The only unwise passage in the pamphlet is the rhapsody on "common sense." The assumption for the Association, that its orator should be acknowledged as the mouthpiece of the universal medical body, is simply a presumption. That the Torquay address could be so received by the profession, was the day-dream under summer skies of its æsthetical author. "Printed by request," as regards the public, it is still a dead letter. As a voluntary cleansing of conscience, *coram populo,* of the most universal of our corporations, it challenges ridicule rather than sympathy. It is confession without humiliation. The Association sprang, Minerva-like, complete, from the intelligence, the emulation, the irrepressible cravings of the professional mind, in the earlier quarter of the present century. Honour and fealty to its parent Jove!

The Torquay President had notoriously too much on his hands. A great elimination of mischief is undertaken. His process of extraction is one of acrid juices by butterfly sips. These rapid sweeping denunciations leave no end of dust, but seldom remove the nuisance. The individual listener declines even a limited liability in blame which he is invited to share with others. He respects himself, and passes it on. No one of the Torquay excursionists took home his own cap as fitted by the President, however capital the fit. Did any one refuse at his hands the photograph of his neighbour?

To every oration its peroration, its climax, and its echo. It comes as usual, a clear case of afflatus from the genius Loci. Of Torquay, of "fair Torquay," "Queen of the South," how can a chamberlain of her Court presume to speak but with the conventional guide-book rapture of a loyalty that matches with her "large prosperity!" In the one, the only *bonâ fide* medical sentence of the entire composition, the President tells his Council and assembled Commons of the Association that the climate of their present choice is such as will suit advantageously with those among their patients who may be suffering from "pulmonary diseases, renal affections, irritative dyspepsia, inflammatory conditions of any of the mucous membranes, and the disorders of advancing life and old age." Is this the language of "modern medicine?" Does it, indeed, talk thus to its sixth-form boys? Are pulmonary diseases, renal affections, inflammatory conditions, senile disorders, so uniform in their minute physiology and microscopic pathology that they can be thus ticketed in the lump and made available for particular reference? What is irritative dyspepsia? Is it more or less than a "nosological mental abstraction?" Do we owe it to the improved animal chemistry of the mucous membranes, all or any? Does Torquay combine the several special advantages of Pau, Nice, Mentone, Malaga, Malta, Egypt, and Madeira? Is it as good as Avoch in Cromarty, or as the coast-line of Upper Norway, for certain varieties of pulmonary disease? For what especial "disorders of advancing life" is Torquay to be preferred as a place to die in? Was it therefore recommended to the British Medical Association, as being hypochondriacal, already out of wear and caducous in 1860, "the twenty-eighth year of their corporate existence?"

The act is over,—the hour is up,—and the President retires from his audience in front of a South Devon land-and-water scene charmingly painted, brilliantly lighted, and most elaborately set. It really does one good to gaze on it. This glowing final tableau is the more effective, as, according to received dramatic usage, it is made to follow on a gloomy presentment of a "religious revival" in all its plenitude of "emotional riot." This is hardly fair on the Wolverhampton President-elect, who may be next on the rostrum. The "smiling meadows," the "blooming orchards," the "soft red rocks," the "coral strands," the "blue translucent bays," the "cerulean depth" of South Devon skies avail not him when called in turn to dress the stage. His local imagery is of another kind. Still, should he be absolute for an effect at the fall of the curtain, he may find his model in the closing scene of the Torquay address. If he cannot intensify the splendour of its final light, he may aggravate its penultimate gloom. Let him finish by a "Revival" on a bad day in the "black country."

We can assure the President that we lay down his lively pamphlet with regret, long as we have had it before us. We have read it with an attention that would have amused and astonished its author. "Printed by request," and emanating from the associated medical science of all England, it became of necessity a thesis for study. It is, moreover, in itself, a composition of great and peculiar interest. There is a sentiment in it, of physic though it be. It is the outpouring of a medical heart in medical matters, assuredly a true and large one. We thank the President for his true professional fervour. We thank him for his wide suggestions—for his faith in our common calling—for his due appreciation of our social position. The coal which he has left between us and the public will never be blown to conflagration. Collectively, we are safe to hold our own. The public is, and ever will be, out of doors. We are in possession. Spite of ourselves, our talking, publishing selves, within ourselves we are strong—a belief which we cannot more agreeably express than by recalling Dr. Radclyffe Hall's very sensible observation: "There is an amount of sound intelligence and practical good sense afloat in the ranks of the profession over and above what finds expression in print."

REVIEW VIII.

Clinique Médicale sur les Maladies des Femmes. Par M. G. BERNUTZ, Médecin de la Pitié, et M. E. GOUPIL, Médecin du Bureau Central. Tome premier.—*Paris*, 1860. 8vo, pp. 591.

Clinical Treatise on the Diseases of Women. By Drs. BERNUTZ and GOUPIL. Vol. I.

THE tenor of a long life is often the result of an accidental occurrence, and valuable medical works owe their origin to an isolated case, whence, as from a well-spring, has flowed the steady current of long-continued researches. The case may have been eminently synthetic and typical, and therefore worthy of a man's life. It may be his own case, and the author may have worked it out in all its bearings as if he were writing an autobiography; or, on the threshold of medical life, a man may have had to live, face to face, with some particular form of disease, and affection may have magnified his powers of observation and made him strive incessantly to see further and to do better. The work which we are about to consider is, as it were, the development of a case which Dr. Bernutz had under his care in 1844. The patient first suffered from a swelled abdomen and severe expulsive pains at a menstrual period, the catamenial flow being absent. The pains subsided, but returned with increased severity at the four subsequent menstrual periods, which were only once attended by a scanty flow of blood. The patient was carried off by acute peritonitis, and a large coagulum of blood was found in the pelvis. The blood evidently came from the womb through the Fallopian tubes, for liquid blood was found in the uterus and in the much distended oviducts. A densely hypertrophied neck of the womb had caused retention of the menstrual fluid and made it flow into the pelvis.

After cogitating on this case for several years, Dr. Bernutz, in 1848, made it the text of some papers on retention of the menstrual fluid, published in the 'Archives Générales de Médecine.' They attracted considerable attention and a certain amount of adverse criticism, which, instead of replying to, he steadily worked upon, and having done so for twelve years, in conjunction with his former pupil, Dr. Goupil, give us a first volume, as an earnest of future contributions to the study of diseases of women. This volume contains the history of retained menstruation under all its forms and degrees, and of hæmatocele or effusions of blood into the pelvic cavity, and concludes with a chapter on those better known effusions of blood into the peritoneum which are the result of extra-uterine conception; and Dr. Bernutz would have still delayed the publication of this volume had not he considered himself forced to vindicate his just claims, inasmuch as the result of his contributions in 1848 was to draw attention to similar cases, and in 1851 the Professor of Surgery in the Faculty of Paris, Professor Nelaton, claimed the discovery of a new disease for having given the name of hæmatocele to pelvic blood-tumours. Professor Nelaton could well have afforded to have recognised Dr. Bernutz's prior claims to the elucidation of an obscure disease, but this recognition was omitted by him, in his work on surgery, and by his pupils in several theses written on hæmatocele.

We own that we were surprised at finding 345 pages of the work before us given to the investigation of retained menstruation, but the authors have carefully investigated the history of complete menstrual retention, so as to throw light on cases where menstruation is only partially retained, and which occur in every-day practice, as the result of inflammatory affections of the neck of the womb, of its hypertrophy, of stricture of the womb or obstructive dysmenorrhœa. It will be thus understood that the scope of the work is larger than might have been anticipated from its title. Cases of complete and partial retention of the menstrual flow are classed in the following manner:

1. Congenital retention of menstruation is exemplified by well-chosen cases, but we shall only remark, that in some of them the puncturing of the tumour to prevent the rupture of the distended womb was very speedily followed by death, a circumstance that has been noticed by Dr. Ramsbotham. This has occurred when the womb has recovered the power of contracting on the sudden removal of the bulk of the fluid, and thereby caused the rupture of the false membranes, which confined the blood to the vicinity of the distal ends of the oviducts.

2. After the full establishment of menstruation, the complete retention of the menstrual fluid has been due to the uniting by first intention of the lacerated neck of the womb, the result of parturition, as in a case related by Dance. Eczema of the vagina has been known to change the passage into a mere fistulous sinus. Gangrene of the vagina has had similar results. Potassa fusa c. calcê applied to the

os uteri has caused its closure, as in the case of Mr. Williams, cited by Dr. Bernutz. We have also twice seen this result—once in a dispensary patient who did not return to have the wound dressed after the application of the caustic to a deep ulceration, and in another case, where the patient had been attended by a practitioner with unjustifiable energy. A French journal has also lately recorded the obliteration of the neck of the womb having followed the application of the actual cautery.

3. Stricture of the womb or mechanical and obstructive dysmenorrhœa is treated of as a frequent cause of the incomplete retention of menstruation, but our authors own that they have been surprised to find that women frequently menstruate, without dysmenorrhœa, through an os uteri that will only admit the smallest possible bougie, and we have repeatedly made the same remark. In the case of a patient suffering from ulceration of the neck of the womb to which caustic-potash—not potassa fusa c. calcê—had been applied, there remains only the smallest possible opening, yet there is no dysmenorrhœa unless the remaining portion of the neck of the womb becomes inflamed. The authors deprecate the operation of slitting the womb, and observe that French surgeons have done right to reject this operation. We think so too. The operation is generally useless, and its dangers would be better known if coroners' inquests formed part of Scottish law. We know that in one case this operation speedily caused the death of the patient by hæmorrhagy, although Dr. Simpson states that he has never seen this to occur.

4. Arguing from the well-known fact that retention of urine is caused by inflammation of the neck of the bladder, Dr. Bernutz holds that more or less prolonged retention of the menses is caused by inflammation of the neck of the womb. We willingly admit this filiation of events as a frequent occurrence, and concede that if inflammation leads to hypertrophy and the gradual obliteration of the cervical canal, it may cause complete retention of the menstrual fluid, as in Dr. Bernutz's original case. While thus allowing to retention of menstruation a greater importance in uterine pathology than it has hitherto received, we strongly object to undue exaggeration, towards which the authors seem to tend. They endorse Caseaux's assertion, that he has never met with a case of inflammation of the muscular substance of the womb, and they state that in describing acute metritis, Chomel only described cases in which, from temporary retention of menstruation, the neck of the womb became inflamed, and its body distended and sensitive. We are asked to admit that the sudden action of physical shocks and mental emotion inflame the neck of the womb, and produce retention of menstruation; but these causes, which check the menstrual flow, frequently check its secretion, thus causing uterine disease, in which retention can play no part. The authors are too enlightened not to admit this position, but they have never once discussed it, although it is one of the most important questions relating to the theory of menstrual retention.

5. After enumerating polypoïd growths as being susceptible of

causing temporary obstruction to the menstrual flow, our authors mention that the same effect is brought about by the uterine casts passed by those suffering from pseudo-membranous dysmenorrhœa. From a careful discussion of the case published by Dr. Oldham in his interesting communication on this subject, and of other instances of this singular affection, the authors adopt the conclusion already come to by Dr. Tilt, whose researches they quote, that this disease is a form of internal metritis. It is admitted that in these cases the partial retention of menstruation is less to be attributed to the mechanical impediment of the membrane, than to the fact that inflammation renders the neck of the womb less able to dilate.

6. The retention of the menstrual flow by the flexion of the womb on itself, and by other uterine deviations, had been noticed by Scanzoni, but the frequent absence of all dysmenorrhœal symptoms in marked cases of uterine deviations led our authors to believe them innocuous, unless they be complicated by spasm or inflammation. This view has been already ably brought forward by Dr. H. Bennet, and we think that the supporters of the school of uterine mechanics have unwillingly entailed a large amount of misery on their patients.

7. Menstruation may be for a time retained by the spasmodic contraction of the neck of the womb, brought on by anger, fright, cold, &c. The body of the womb then contracts, more or less painfully, according to the amount of retained fluid, and abdominal and back pains occur, as in miscarriage and parturition. In healthy patients this state of things is set right speedily, or else at a subsequent menstrual epoch; but this spasmodic stricture of the neck of the womb may become tedious when associated with anæmia, chlorosis, and hysteria. We must, however, bear in mind that in many of such cases there is no menstrual fluid to retain, because none has been secreted.

8. The menses may be retained from obstruction of the Fallopian tubes at their uterine orifices. This was new to us, but three exceedingly interesting cases are given where pelvic tumours, which had increased every month, were found, on post-mortem examination, to be dilated Fallopian tubes containing blood in large quantities. These cases prove that part of the menstrual flow comes from the oviducts—a position that we have long maintained; and they moreover justify the ascribing of an hæmatic origin to some tubo-ovarian cysts.

Respecting the symptoms and diagnosis of menstrual retention, due regard must be paid to its amount and the length of time it has lasted. The early stages of partial retention are expressed by pelvic and back pains, similar to those of menstruation and parturition; in worse cases, pelvic peritonitis supervenes, and in very rare ones, hæmatocele.

The minor degrees of retention of the menses may be confounded with early pregnancy in primiparous women, but the symptoms of pregnancy soon clear up the difficulty. Confirmed retention of menstruation may be taken for an impending miscarriage, but in that case the os uteri would be soft and elastic, whereas it is hard and unyielding, with lateral enlargements, when the uterine swelling is caused by retention of the menstrual fluid. In miscarriage, red blood flows in profusion, whereas in the other case, only a small quantity of decomposed blood would come away.

We must refer to the work itself for the diagnosis of hæmatocele and the diseases with which it has been confounded, premising that it has been mistaken for a retroverted gravid womb, for ovaritis, for purulent peritoneal collections by Nelaton; for uterine fibrous tumours by men so experienced as Malgagne and Stoltz; for an hydatid cyst in the iliac fossa, for general peritonitis, and even for cholera. It has been repeatedly mistaken for cancer of the womb at the change of life; for if in the advanced stages of hæmatocele, irregular hardnesses are to be felt in the vaginal portion of the tumour, some cancerous tumours soften as they progress, giving an obscure notion of fluctuation.

Extra-uterine conceptions have, however, been most frequently confounded with hæmatocele, and our authors have judged wisely to review their pathology, in order to bring out their differential diagnosis. This has been the special labour of Dr. Goupil. We had marked several passages for quotation, but our very limited space will only permit us to refer the reader to page 567 for the differential diagnosis of these affections. The best rules will not always prevent mistakes, and Professor Nelaton found a fœtus in a tumour which he supposed to be an hæmatocele.

In treating those cases of partial and ephemeral retention of menstruation which so frequently occur as a result of spasm of the neck of the womb, of its inflammation or its hypertrophy, giving rise to stricture of the womb or to obstructive dysmenorrhœa, the first and obvious indication is to soothe by warm and sedative injections, by warm hip-baths and whole baths, by warm poultices to the abdomen, and by leeches applied to the neck of the womb. Such measures will be in general successful, and when the menstrual flow is very scanty, infusions of rue, saffron, and savin are recommended to be taken warm, alone or associated with antispasmodics and opiates. Ergot of rye is said to increase pain, without being otherwise more efficacious than the remedies above mentioned. It will be understood, from what has been previously stated, that mechanical means of relief, by dilatation of the neck of the womb by means of sponge-tents and by Dr. Simpson's stem-pessary, are strongly objected to, as destined to keep up that spasm or that inflammation of the neck of the womb which has caused retention of menstruation. We quite agree that the use of the stem-pessary, seldom justifiable in uterine displacements, is not at all so in the cases under consideration; but when inflammation is removed, we do not understand why dilatation should not be practised, for it can be safely done by bougies and sponge-tents.

Although in a late number we reviewed Dr. Voisin's work on Hæmatocele, we must briefly notice what our authors say on the subject, because their account of the complaint is the latest and most philosophical that has appeared. It was at first assumed that hæmatocele was always due to one and the same cause; but as the disease was better known, it became clear that the effusion of blood might be referred to various sources. We quite agree with our authors that hæmatocele is only to be taken in the same sense as hæmatemesis and hæmoptisis, but these are very convenient terms, and we do not at all side with them in their energetic denunciation of the term.

The idea of hæmatocele comprises both effusion of blood into the pelvis, and pelvi-peritonitis, for the blood must be encysted by false membranes, or it cannot be felt as a tumour in the vagina, however large may be the quantity of blood effused into the peritoneum. Although this variety is the most important, we do not agree with Dr. Bernutz that the term hæmatocele should be confined to cases of intra-peritoneal effusion. Cases of extra-peritoneal effusion have been published, independent of thrombus of the vagina occurring during parturition, or of tubal fœtation; and it is well to understand that hæmatocele is a very rare disease for all those who are known for the precision of their diagnosis.

From a careful analysis of all well-authenticated cases of hæmatocele, Dr. Bernutz divides them into the following groups, according to the pathological conditions in which they originated:

1st. The blood has come from ruptured varicose veins of the pampiniform plexus of the ovary. This condition coincides with a varicose state of one or both labia, and seems to have been caused by pregnancies repeated too often in a short space of time; the rupture occurring after long-continued standing, or jolting in bad conveyances on rough roads.

2ndly. The blood has come from the inflamed peritoneal membrane; hæmorrhagic peritonitis being analogous to hæmorrhagic pleurisy. In the only cases on record it seems to have occurred after inordinate coition.

3rdly. The blood has come from a rent in the tissue of the ovaries or oviducts. Dr. Bernutz gives Dr. Tilt the credit of having clearly indicated this, as a chief cause of hæmatocele, long before Laugier's Mémoire to the Académie des Sciences on this subject; and Mr. Madge has lately communicated an interesting case to the Obstetrical Society of London. Dr. Bernutz contends that this cannot be admitted as a cause of hæmatocele, unless a thoroughly disorganized ovary be found on a post-mortem examination; but we do not see why, without being thoroughly disorganized, the ovary may not, as a result of morbid ovulation, let flow into the pelvic cavity at successive menstrual epochs many ounces of blood instead of several scruples, as when menstruation is normal. Have not large quantities of blood been known to flow from a small uterine polypus? May not a large quantity of blood be effused from a very small portion of mucous membrane, as in epistaxis? As in this variety of hæmatocele, there is a sudden effusion of blood, there is a pallid skin, fainting and prostration, as well as symptoms indicating peritonitis.

4thly. The blood may have a catamenial origin, as we have seen in the preceding portion of this review; the menstrual blood passing through the widely-dilated ostium uterinum of the oviducts. Ruysch, Haller, Sir B. Brodie, and others, have seen cases of this description. Indeed, Mr. Wade, of Birmingham, has recently stated that a cure may be effected by the effused blood passing from the pelvis, through the oviducts; but he had no opportunity for a post-mortem examination, in either of the five cases in which he supposes this to have occurred.

5thly. The blood may be the result of an inordinate flow of blood towards the generative system, which at the same time produces metrorrhagia. For the production of metrorrhagic hæmatocele, the combination of a cachectic condition of the blood and a molimen hæmorrhagicum, which is in general allied to, and dependent on, the menstrual nisus, appears necessary. Thus this form of hæmatocele has been noticed in malignant eruptive fevers, in purpura and malignant jaundice, in both instances after miscarriage. It has also been noticed in the puerperal state; but hæmatocele has been most frequently observed in connexion with anæmia and chlorosis, which variety is well described at pages 496 and 505.

We now come to the *treatment* of congenital retention of the menstrual fluid, of hæmatocele, and of extra-uterine conception. In those distressing cases of congenital retention of the menstrual fluid, when every month there is an additional increase of blood, so as to foretel an untimely issue of the case at an early date, an operation is indispensable. The probability of success is greatest when it is performed before the oviducts have been much distended; and instead of operating during a menstrual period, as recommended by Dance, it is better to wait eight or ten days, until the menstrual excitement be passed. Dr. Bernutz repudiates the puncture of the womb by the rectum, as a mere palliative measure, still attended with danger. He advises us to follow Dr. Debrou's example, if the place of the vagina be occupied by solid fibrous tissue. In his case, this skilful surgeon successfully tunnelled through the fibrous body, and restored his patient to health. If, on the contrary, the rectum and the urethra are in close proximity, without any intermediate fibrous substance, the authors recommend the gradual but forcible separation of the rectum from the urethra, by tearing with the finger, and the subsequent puncturing the tumour, at the end of the *cul-de-sac* thus obtained. This operation was devised by Amussat, and crowned by complete success; and it is impossible to read the case without feelings of admiration for an art which can thus rescue from the grave a fellow-creature in the prime of life, and inevitably doomed to death.

Guided by the fatal results which have often attended the sudden evacuation of the menstrual fluid, Dr. Bernutz shows the importance of making only a small opening, so as to empty the tumour very gradually. Dr. Bernutz joins in the general admission that the blood, in hæmatoceles, should be left alone, to be gradually absorbed by a kind of digestion, according to the happy expression of Dupuytren; to have opened them was contrary to all that was known about the treatment of hæmatic tumours and the dangers of the introduction of air into their cavity. To largely open such tumours, and to tear away the blood-clots, was outrageous practice; for what else but air and decomposed blood could replace the blood-clot? Rare cases will occur in which an opening is justifiable, because the increasing tumour menaces to burst into the peritoneum; but puncture will often suffice, or else a small opening should be made with a tenotomy knife, if the fluid will not run through the tube of the trocar; and when tepid water is to be injected into the cyst, to remove decomposing fluids, the greatest caution should be used, as this injection has been sometimes speedily followed by peritonitis and death. Dr. Bernutz recommends two or three applications of four leeches to the neck of the womb, at a few days' interval, to promote the absorption of the effused blood; and he has seen this plan attended by rapid diminution of the tumour; and considering that he seldom alludes in sanguine terms to the efficacy of medicines, we feel disposed to place reliance on the plan—at all events, we agree with him, that an application of leeches to the neck of the womb should be made when the ensuing menstrual period becomes due, whether the flow appears or not. Except it be at the menstrual periods, it is advisable to forego the leeches in the anæmic and cachectic varieties of hæmatocele. The internal and external exhibition of opium, until narcotism is brought on, seems to be of the most service. Vomiting and nervous excitement are thus often subdued, as well as that morbid impulse which impels the blood to the generative organs.

The treatment of effusions of blood from extra-uterine conception is too hopeless to detain us long. The rational indications, which are judiciously commented on by Dr. Goupil, are—perfect rest, and opium given in as large doses as the patient will bear. Should the patient survive the shock of the internal hæmorrhage, surgical interference is to be avoided, as those patients have done best who were left to nature.

It is unnecessary to state that we entertain a high opinion of the present work. We live in an age of steam and of railway rapidity of action: most of the works we are called upon to review are written too rapidly, they were not laid by for a time and then considered afresh, so as to be brought up to perfect maturity; but seem published more to suit the publisher's convenience, or the more immediate prospects of the author, than his fair fame and the advance of science. The work before us bears evidence of having been repeatedly gone over; cases are not accepted in evidence of pathological views, when they are spoilt by the slightest obscurity, and the cases of old date are extracted from the original sources. We also commend the authors for avoiding that childish mockery of completeness which French writers generally aim at, as if their readers did not know, that for such kind of work, a stout pair of scissors and a good supply of paste are all that is necessary. We shall anticipate with pleasure the second volume, which is to give the history of peritonitis in its relations with diseases of non-parturient women.

Review IX.

Historia Bibliográfica de la Medicina Española. Obra póstuma de Don Antonio Hernandez Morejon, Médico de la Real Cámara, Primer Catedrático de Clinica en los Estudios de Madrid, &c. &c. Tomos VII.—*Madrid*, 1842–1852. 8vo.

Bibliographical History of Spanish Medicine. A Posthumous Work by Don Antonio Hernandez Morejon, &c. &c. Seven Volumes. —*Madrid*, 1842–52. 8vo.

Whence is it, that it is less with a feeling of surprise than of regret, that we look in vain throughout the medical literature of Great Britain for a history of the progress of medicine in our own country? Of medical biographies, and of special narratives of the growth of individual doctrines, or of methods of practice originating among ourselves, our press has neither been lavish, nor has it been altogether neglectful: but of that comprehensive view which seeks to include the whole relations of a science, whether with reference to the intrinsic succession and order of the entire array of its discoveries and improvements, or to the extrinsic bearing of these upon the development of the common culture and upon the conditions of society, limited in so far to our own intellectual existence, yet maintaining a constant regard to the associated progress of mind elsewhere, we have not, unhappily, a single example to bring forward. We do not, of course, speak here of general histories of medicine, though even of these we can boast but a very limited catalogue: while narrow indeed is the list of our writers in this field whom we have been entitled to adduce as original investigators, or of whom it can be justly said that they have achieved, or have deserved, either a wide or an enduring reputation. When we have cited the masterly sketch of Friend, confined, as it is, in range, and scanty in its details, what have we else to offer to compete with the Le Clercs, the Sprengels, the Ackermanns, the Heckers, and the Haesers of other countries? To vie with the separately national histories of a Morejon or a De Renzi we have absolutely nothing.

And yet, neither has the cultivation of medical science in our country ever been in so neglected or degraded a state as not to deserve a special history, nor have the rewards bestowed upon its promoters usually been of so stinted a quality, that there could be no claim upon any one to display his gratitude by registering its career. On the contrary, nowhere has the social position of the practitioner of the healing art, or his scale of remuneration, been higher than within these realms; although his relation to the governing bodies, within his peculiar sphere of duty, may have often been, or even for the most part remains, less close than in other countries. It is, however, more than may at first sight appear, this attitude of freedom of the practitioner towards the State, which contributes essentially to the eminence of his position in the social system; and we trust it may be long, for his own sake, before he may be constrained to lower himself into a more subservient relation, or may have cause to regret, in the diminished independence of our various professional brotherhoods, that diminished influence and power of concentrated action in the united body, and hence of individual prerogative in its isolated members, which has been the deeply felt result of the reduction, or rather the annihilation, of corporate authority and privilege abroad. But the veteran member of a profession, thus, at least occasionally, honoured and rewarded, may, and should be, often enabled to give the eventide of a life of labour to the enjoyment of a comparative leisure; and it might then be legitimately expected from some of the masters of our art, that they should employ a portion of that leisure in celebrating the native annals, or a portion of the native annals, of a science which had not denied to them, at least, the liberal reward of an anxious service, however less enviable may have been the lot of others who may have toiled in so far the more arduously that it was the more obscurely. Surely such a tribute to the science that had fostered them would be a graceful return for the benefits derived through its exercise; and in rendering honour to the humanest of arts, and to those who had co-operated with them in ministering to its advancement, they might fitly gather new honours to themselves.

We have a title to look, with the greater justice, to the more able and prosperous of the seniors of the profession for the discharge of this important office, because, in the first place, it is only by the instrumentality of distinguished talent and comprehensive acquirements that the task can be fittingly attempted; for the historian of British medicine must occupy a judgment-seat where only prolonged experience, joined to extensive learning, sagaciously and impartially wielded, can give authenticity to his narrative or authority to his decisions. Nor, on the other hand, need we hesitate to own our fear, that independence of means and of position must be reckoned, at the same time, as almost essential requisites: for it would be sanguine to hope that the work, in any pecuniary sense, could prove a remunerative one; and we ought not to lay upon the experienced, and, in that very sense, laborious, but less amply requited, physician a charge which would impoverish him still further, and thus impose upon his contemporaries a new debt of gratitude, while removing the labourer more widely than ever from all prospect of a requital. The somewhat mercenary, if not injudicious, spirit of our nation demands too frequently a return which it conceives to be tangible, in compensation of its bounties; and it would not be ready to acknowledge this return in that which did not carry along with it the promise of any immediate practical result. Neither is such a spirit usually willing to acknowledge merit, or to award honour, where it does not notice the marks, assumed or real, of affluence and success: as if his were not really the greater honour whose course has been to persevere in toiling, purely and fervently, though in obscurity; preserving his equanimity, while falling short of his reward, at least in anything like a substantial form. It is not, therefore, to the public, faintly interested, besides, in the records of a science the progress of which it has been little taught to consider as more than a mere concern of the inner social life, constituting no integrant part of the outer national glory, that the writer of a Medical History of England could look for that recompence which might await him for many far easier, however beneficial or otherwise really creditable, achievements. Such achievements we may instance in the composition of a treatise on some individual disease of general prevalence; or in the hazarding some innovation on ordinary practice; or even the discussing some topic of less intrinsic and of merely casual and temporary interest in medicine, yet opportune at the time, because falling shrewdly and deftly into harmony with a popular whim or bias. Nor, were he so inclined, could his toils be requited, or his ambition gratified, in another form, by his appointment to a chair of medical history in some of our universities; for no such chairs have been instituted, or seem likely to be maintained, among us, though their institution be now a recognised obligation in nearly all the more distinguished universities of the Continent.

To the philosophic student of medicine, a science of so wide a scope that every practitioner must remain through life a student, it is unnecessary to point out the value and interest of the spheres of inquiry embraced within the range of medical history. That is, indeed, an exalted study, which pursues the course of the development of our knowledge in one of its most important departments; which shows us the gradual ripening of the faculty of observation; the origin and the fluctuation, with the alternate tyranny and subversion, of opinions; the building up, the casting down, and the reconstruction of hypotheses; and the accomplishment of this, often with the addition of new materials, though not rarely with merely the old materials modelled into new forms, or the old forms disguised under a new phraseology. Cheated thus sometimes with tradition under the proffer of originality, it is yet, with all, and through all, to find the false becoming slowly eliminated, and the true recognised and extended, till, by degrees, a body of irrefragable laws has been evolved, and we are in possession of a clear range of rational theory, resting on its adequate basis of fact. And this is not the less true, that our successes in both of these may be justly accounted narrow, when brought into contrast with the limitless field for investigation from which they have been gathered, and within which so abstruse are the mysteries, that scarcely a single truth can be mastered, without leaving a host of doubts and uncertainties beyond. Thus, without history, the past has no warning, and leaves us no thorough example; and time is wasted, and confidence shaken, and science discredited, so long as each age confines itself to its own narrow horizon. It is only through the careful scrutiny and enumeration, by the judicious historian, of what have been received as determined laws, and through the analysis of the method by which these have been developed, that we are swayed from random courses of investigation and belief, and taught to arrive prudently at what is the very essence of our science. The result attained is precious, because it secures to us the genuine heritage of the past; and the habits formed are precious, because they constitute our safe guide to future conquests, our zeal and success in which is but another form for the promotion of the happiness and welfare of our race.

But without further speculation as to what may be the value and importance of the study of medical history, whether as a record of the past, storing up its wisdom to illustrate the present and the future, or as a registry of honour, to enshrine the worth of old memories; or without examining more minutely into the causes of its neglect in this country, or of the more generous and considerate appreciation it encounters abroad, let us now turn to the work, the title of which we have placed at the commencement of our remarks, and to the consideration, in as far as our limits will permit, of the particular topics which it more immediately suggests. It is only in recent times that the history of medicine in Spain, in the enlarged sense of the term, has received anything like an adequate amount of attention from even the Spaniards, who have been remiss in this like ourselves, devoted admirers as they are of their national attributes otherwise. The present century, however, seems to have made up for the negligence of the past, by producing three several works on the subject, each of them of considerable interest and importance. The first in extent of these, and indeed in value also, is the bibliographical history by Morejon, a posthumous work, the publication of which commenced in 1842, six years after the death of its author; its separate volumes appearing afterwards in successive issues, of which that in 1852 is, we believe, the latest, or is, at least, the last published that we have seen. A second work is that of Anastasio Chinchilla, which has appeared, we are informed, in four volumes, and which has been received with considerable approbation by the author's countrymen, but of which we are ourselves only able to speak on report. The third is the historical compendium of Mariano Gonzales de Sámano,* constituting a single volume, with an appendix, published in 1850; and which we may briefly notice here as entitled to the merit of being deemed generally an able, though it is occasionally an unequal composition, its failures being nowhere more remarkable than at the more retrospective periods of inquiry, where a fancy that appears to have been little restrained by any strict rules of investigation, and equally little directed, there is but too much evidence to show, by any competent classical attainments, or careless, at the best, in the use of them, has sometimes vitiated the details into a singular tissue of inaccuracy and confusion. Thus an inscription derived from some ancient monument, in itself of great promise of interest, or even the title of a work in Latin, is occasionally so inaccurately transcribed, that the former especially becomes almost unintelligible, and otherwise nearly useless to the archæologist. Neither is it agreeable to the reader of a medical historian to have to seek, for example, in Valdemont for Van Helmont, or in Brochave for Boerhaave. Adopting the treatise of Morejon as our principal guide, while referring occasionally to that of Sámano, where entitled to confide in it, and especially with regard to existing or recent conditions, we shall yet adhere exclusively to neither in the

* Compendio Historico de la Medicina Española. 8vo. Barcelona, 1850.

brief sketch which we now design to offer of the progress of medicine in the Western Peninsula; but shall turn to other sources of information as these may chance to present themselves, or as the opportunity, which will not tender itself rarely, for the immediate consultation of any author under notice may qualify us to attain, or to attempt, an independent judgment.* The subject is one which has been little regarded in this country, nor have its more essentially national portions been generally treated either copiously or accurately elsewhere. It remains to be shown, whether, in the narrow limit we can assign to it, necessarily constraining us to a merely cursory examination, it can be invested with any such degree of interest as to demonstrate that it has been unduly neglected, or that the record may have still its value, though, in as far as Spain is concerned, it relate too predominantly to the past, and can point with difficulty to any living essence as the undegenerate representative of what may be proved to have been its previous honours.

Morejon divides his history of medicine in Spain into five epochs. Of these, the first embraces what is known, or can be surmised, regarding the condition of medicine among the primitive inhabitants of the Peninsula, with the relations to the Egyptian, Phœnician, Greek, and Roman colonists: the second relates to the period of the Gothic domination: the third, to the era when the Jewish element was that which predominated in the medical science of the country: the fourth gives the interesting period during the rule of the Arabian or Moorish dynasties: and the fifth, entering upon what may be more pertinently termed the strictly Spanish field, narrates the progress of medicine from the union of the kingdoms of Castile and Arragon, with the establishment of a true national individuality, under Ferdinand and Isabella, down to our own times. The mere indication of these epochs shows how important, and how peculiar, must be the details included under so prolonged and so varied a course of investigation; but it evinces, at the same time, how extensive is the task of surveying it, and the necessity of chiefly confining ourselves to only a portion of the multiform inquiry. Even the earliest epoch marked has its many points of intense interest; and, among the rest, has its numerous appropriate illustrations in the monumental inscriptions, religious or sepulchral, which remain in the country and have been treasured in its collections, upon the consideration of which we would willingly have entered. The Gothic period has left its curious records also, in the rules of medical police which constituted part of its legal codes. The position of the Jewish physicians,† and the condition of their art, struggling against bigotry in religion and barbarism in

* To distinguish more clearly what we have arrived at as our own opinions, from what we have derived, more or less entirely, elsewhere, we may point out, once for all, that the whole of the references at the foot of the pages are to works which we have ourselves directly consulted with a view to a judgment.

† We are unwilling to withhold here the picture of the life of a fashionable and prosperous Jewish physician of the twelfth century, presented to us in a letter of the Rabbi Moseh-Ben-Maïemon, a native of Cordova, but practising afterwards at Cairo: "I reside," he writes to a friend, "at the Egyptian capital, and am in terms of the greatest privacy

manners, present many attractive points for observation: while of the arts and learning of the Moors, so remarkable in origin and in development, and so peculiar in their characteristics, we may be held to know enough, through general history, to dispense in so far with the consideration of them in their more exclusive relations to the history of medicine in Spain, from which, however, it is manifestly not the less impossible to view them as separated. Pleasing as would have been the effort to have followed Morejon through his discussion of these several periods, and to have watched, aided by the illustrations he supplies, the influence of each upon that which was immediately to follow; surveying that part elimination, and part addition and consolidation, which, applied to the whole, led to the point of culmination in the medical science of the country; it is, nevertheless, to this culminating point, or to its nearer approaches, that we must at once pass, content that our limits only permit us to view that as a truly Spanish medical literature, which emanated from those whose sole native tongue was the language of Castile.

While the Moors encouraged literature and maintained schools throughout their dominions in Spain, the Christian States of Castile and Arragon, it will be remembered, were solely occupied with war, until the time of Alonzo X., surnamed the Wise, or till during the latter portion of the thirteenth century. It is true that Alonzo VIII., in the last year of the preceding century, had already founded the University of Palencia; while his successor, Alonzo IX., had founded that of Salamanca in 1243: but it was only under the tenth monarch of this name, distinguished alike as a man of learning and a lawgiver, that these institutions rose into vigorous and efficient action. One of the proofs of the just claim of this ruler to the epithet by which he is designated, may be discerned in his general directions for the location of the greater scholastic establishments. These are to be placed, he ordains,* in a good air, and in an agreeable neighbourhood, so that the masters who instruct, and the scholars who learn, may enjoy health, and may find means of pleasant recreation at their hours of leisure; while care should be taken that the necessaries of life are in abundance, and that a place of entertainment should be provided, where the students might assemble to have their pastimes, and to

with the Grand Sultan, whom, in the discharge of my duty, I visit daily, morning and evening; and when he, or any of his sons, or of the ladies of the harem, are unwell, I remain in the palace the whole day. It is, besides, my duty to attend the principal state-officers in their illnesses. When I go to the court in the morning, and meet nothing new to detain me, I return at mid-day to my own house, which I find full of Jew and Gentile, nobility and commonalty, judges and merchants, friends, and even some who are no friends, all awaiting me. As soon as I arrive, I salute them civilly, and beg them to allow me to take some refreshment; and then, leaving the table, I busy myself with inquiring into their ailments, and direct the necessary remedies. Many there are who are obliged to wait till night, because the attendance is so numerous that I am occupied with them the whole afternoon; and sometimes I am so worn out, and overcome with drowsiness, that I drop over asleep even while conversing, unable to utter another syllable." Without pausing to inquire as to what might be the value of the prescriptions of the half-asleep sage, we must confess that Maiemon was not undeserving of his reputation in his time, nor is his name wholly unrecognised now.

* Las siete Partidas: Part ii. Tit. 31, Ley. 2, 5.

enjoy inexpensive refreshments. The first medical professors of the newly founded institutions were brought from the schools of Cordova and Toledo, where they had become versed in the science of the Moors; of many of whose principal writings, as of those of Avicenna and Averroes, they made translations. Thus early initiated in the medical doctrines of the East, the schools of Spain could not, like those of Italy, gather advantages from the Crusades; and not till the sixteenth century was the sway of the doctrines of Avicenna, thus naturally assumed rather than imported among them, finally superseded by the study of the better founded methods and observations of the school of Hippocrates. Passing over, with reluctance, Arnald of Villanova, the most distinguished physician of the fourteenth century, whom Morejon, along with most Spanish authorities, claims as a native of Villanova, near Gerona, in Catalonia; and leaving aside also, with slighter hesitation, the names of a few authors of mere compilations, through whom the science of healing made little real progress, we advert to the foundation of a number of the other universities of Spain which took place during the fourteenth century, and which afforded the means of giving an added stimulus and a wider diffusion to the study of medicine.

But we thus arrive at the name of a Spanish writer of this period, upon whom we are willing to bestow a more special notice, chiefly on the score of his actual merits, yet not uninfluenced by the circumstance that a copy of his very rare, and in a few respects remarkable treatise, is now lying before us. Juan de Aviñon, to whom we refer, was nevertheless not a native of Spain, but, as his name implies, of Avignon, in France. He had, however, he tells us, practised in Seville, where he arrived in 1353, for a period of thirty-one years, at the time when he completed the work by which he is remembered, and which is one of the very earliest written in the language of Spain; so that he may well be admitted, through both circumstances, to have acquired a double title to a place among Spanish physicians. Notwithstanding the date which we have mentioned, it was not till 1545, or more than a century and a half afterwards, that the work was published by Monardes,* who then characterized it as old, and worthy to be read. In editing the manuscript, Monardes describes it as frequently almost illegible from decay: a circumstance which we must accept as accounting for a confusion introduced among certain of the dates. Thus the era of our Saviour has been inserted on one occasion, instead of the era of the Cæsars, which takes its commencement thirty-eight years earlier, and which was that still in ordinary use throughout a great part of Spain in the time of Juan de Aviñon, by whom it was evidently adopted. An inadvertency to this circumstance has led Morejon to fancy that he was able to correct errors in the dates of Monardes, while suffering himself to fall into greater errors; an embarrassment from which he might have escaped, by observing the inconsistencies into which it conducted him. A manifest proof that Juan uses the

* Sevillana Medicina. Que trata el modo conservativo e curativo de los que abitan en la muy insigne Ciudad de Sevilla. 4to. Sevilla, 1545.

era of the Cæsars occurs in his account of the contemporary epidemic diseases of Spain, in which he mentions that tertians of a mild type prevailed in the year 1404, in which year, he adds, the King Don Pedro left his kingdom. As this must refer to the flight of Pedro the Cruel, which historians assign to the year 1366 of the Christian era, the precise difference here of thirty-eight years shows the mode of computation employed by the author. We point to this, because it is important to show that it is to the fourteenth, and not, as Morejon mistakingly infers, to the fifteenth century, that a work really belongs, which we believe has the merit of having had scarcely a forerunner on the field of topographical medicine. Juan claims the greater credit for his treatise, that he had previously seen many countries, and was therefore the better able to point out the specialities of the city of his later residence. We find in it many interesting particulars of climatology and epidemiology, as well as many curious notices of the customs and manners of the Spanish citizens of five centuries back, their description of clothing, their habits of life, and their supplies and methods of preparation of diet. The filth and corruption of the quarter allotted to the Jews are more than once animadverted upon. Under the head of the regulation of the hours for meals, we have the notice that precision as to this need be no longer a matter of difficulty, inasmuch as the Archbishop had caused a clock to be erected, which was to strike twenty-four strokes: one for the first hour, two for the second, and so on through the succession of the twenty-four hours which made up the day and night.* We are thus introduced here to the first turret clock in Seville, the erection of which seems to have been attended with better success than can be boasted of in our modern Westminster. Among the other medical worthies of Spain, of the fifteenth century, we make passing allusion to the just reputation, in his day, of Fernan Gomez de Cibdad Real, and of Julian Gutierrez of Toledo, for the latter of whom is claimed the merit of having first suggested the fabrication of artificial mineral waters, while it is certain that he anticipated, by nearly three centuries, that alleged discovery of the remedial powers of calcined egg-shells in stone, for which our not always scientifically liberal or sagacious Parliament bestowed upon Mrs. Stephens so munificent a reward. But the reader of medical history encounters no lack of proofs of how belief revolves in cycles, and how what appears even to be high intelligence alternately admires or slights what it treats as its bauble, as the humour catches it.

Early in this, the fifteenth century, a lunatic asylum was established in Valencia, through the exertions of Jofre Gilaberto, an eloquent preacher of the Order of Mercy; and probably Spain may thus claim the high distinction of having been the first to look with humanity on this unfortunate class of sufferers, and to provide for their safe and gentle treatment.† Before the close of the same century, a general hospital

* Op. citat., fol. 64.

† Morejon points with satisfaction to the circumstance, that his countryman, Diego Mérino, so early as 1575, recommended a gentle and cheerful treatment of lunatics, and that "madmen, and those affected with melancholy, should not be shut up in gloomy

was joined to this institution, in connexion with which it still exists. Now also the art of printing was introduced into Spain; and it is interesting to record that the first medical production of the Spanish press, speedily followed by a number of others, was a translation of the 'Treatise on the Plague,' by Valesco de Taranta, which appeared in 1475 at Barcelona, and was the work of Juan Villa, as he is called by Morejon, though more correctly Villar, if we are to follow Antonio,* who on such a point may be accounted a still higher authority. It was with a more than questionable benefit that, nearly at the same period, the system of quarantine regulations originated in Spain, induced by the terror caused through the frequent pestilences by which the country was afflicted. On the other hand, an ordinance, issued in 1488 by Ferdinand, bestowing on the members of the Brotherhood of St. Côme and St. Damian, and on the physicians and surgeons of the hospital of Sancta Maria de Gracia, in the city of Saragossa, the privilege in perpetuity of opening and anatomizing the body of any person, male or female, dying within the hospital, not only without penalty or molestation, but with a penalty for interference imposed upon all others, was a measure, considering the prejudices of the times and the character of the people, of conspicuous liberality and wisdom.† It was to the same enlightened monarch, we may add, that our armies are indebted for the first institution of the humane provision of a regular field hospital during their periods of actual service: a point of interest in medical history which we took occasion to discuss in a previous number of this Journal,‡ and upon which, therefore, we need not now dwell longer.

The great and often since contested question which relates to the origin of syphilis, takes its initiative at the close of the fifteenth century. Upon this question the limits which we must assign to ourselves forbid us to enter with any minuteness of detail. It may

places, but should be lodged in light apartments, and attended by lively persons who would sing to them and amuse them" (Hist., tom. iii. p. 229.) That the lesson thus given has not always been better remembered in Spain than it has been known and practised in other countries, is shown by Sámano's account of a recent visit made by him to this very asylum of Valencia, in which he speaks of certain of the cells as "cages in dimensions, sepulchres in aspect, and sewers in cleanliness." (Comp. Hist., p. xi. note 17.) The same author paid also a visit, in 1849, to the asylum of Saragossa, and stigmatizes the filthy and unwholesome "hog-styes and cages," within which the lunatics were there imprisoned. (Apend., p. 210, note 39.) And yet, as if in contrast to this, we have, in a recent number of a Spanish medical journal (Semanario Medico Español, 1854, p. 67), a description of an establishment in the vicinity of Barcelona, the details of which are so glowing as to represent almost one of those paradises in which even the sane might be glad to find a retreat from the turmoil of the outer world.

* Bibliotheca Hispana Vetus, tom. ii. p. 306.

† If this ordinance of Ferdinand point to an early instance of anatomical investigation, having the sanction of law, we have, in a document from the communal archives of Bologna, cited by De Renzi, perhaps the first instance of a prosecution for body-lifting for anatomical purposes contrary to law. The process was instituted in 1319, against certain medical students of Bologna, who were charged with having committed sacrilege by entering a cemetery during night, and removing from its place of burial the body of a man who had been hanged, for the purpose of conveying it to be dissected by their teacher, Magister Albertus. The delinquency was accomplished and proved, but unfortunately the extract furnished does not report the sentence. Salvatore de Renzi, Storia della Medicina in Italia, tom. ii. p. 249.

‡ British and Foreign Medico-Chirurgical Review, April, 1857.

be sufficient, therefore, to state, that Morejon, coinciding with nearly all our best modern authorities, maintains that the disease had existed in the Old World, Europe included, for long prior to the discovery of Columbus, and thus treats the notion of its first importation from America as a groundless fable. As to the fact of a prior existence of the disease, with whatever modifications of form, in Europe, that we may be entitled to infer from numerous authorities, gathered from all civilization and throughout all periods of history: while as to the opposite and more special allegation of its introduction from America, we should naturally look for our proofs to the Spanish authorities themselves, in order that we may judge in how far the quality of their testimony at the time is entitled to outweigh the probabilities, or the conclusions, founded on an evidence derived from a different and earlier source. Nor can any one be said to have fairly entered here the proper course of investigation, who has neglected this, so manifestly the most obvious and indispensable of its directions. Whoever will test, in this way, the credibility of the narrative of Ruiz Diaz de Isla, and will weigh the allegations and the general character of the reasoning of this otherwise obscure writer, who has been received as a prime authority for the American origin of the disorder, and will then contrast these with the conflicting evidences, negative or positive, derived from other Spanish sources, will, we are assured, arrive at the result of at once denying to the former their validity, and will see cause to free Columbus and his companions from the stigma of having tarnished the splendour of their gift to Europe by so loathsome an accompaniment. We think it deserves to have been noticed with more particularity in these discussions, that Columbus finally sailed from the Gulf of Samanà, in Hispaniola, on his return from his first voyage, on the 16th of January, 1493; that he reached Lisbon on the 4th of March following, where he remained for nine days; and did not arrive at Palos till the 15th of that month, or after a period of fifty-eight days, nor at Barcelona till the middle of April,* or after about three months in all. Yet we advert to this without desiring to insist on these circumstances, because we are not entitled to assign any decided importance to the assumption that the disease, after so long an interval from the alleged period of infection, must, especially if we look to its usual phases as recorded in contemporary writers, have passed into its secondary form, and thus have lost much of its virulency in as far as regarded its capacity for communication. Nor, in throwing out what is thus merely a hint, are we passing wholly out of sight what may have been the *rôle* here of the Indian females whom Columbus conveyed with him as part of the evidence of his discovery.

But even had the disease, after this period of isolation, retained all its virulency, and setting aside the fact of its remaining unheard of at Lisbon, where there was the earliest and an ample opportunity for

* Fernando Colon: Relacion de la vida y hechos de el Almirante D. Christoval Colon, su padre, cap. xxxvi. Barcia: Historiadores primitivos de las Indias Occidentales, tom. i. pp. 33, 37, 38.

its propagation, that of its sudden diffusion afterwards, to so extraordinary an extent and in so intense a form as that described, becomes just as difficult of explanation under the hypothesis of the introduction of a new contagion, as under that of a rapid development of a virus already known to exist, owing to a concurrence of circumstances favouring its aggravation. We may conceive these circumstances, with no violation of probability, to have lain in the extreme dissoluteness of the times, as well as in the prevailing inattention to ordinary hygienic rules, and especially to those of personal and domestic cleanliness: while in the inclemency of the seasons, and in the extensive inundations from which various countries then suffered, in various years, with the consequent dearths, we find sufficient reason to infer a tendency to the prevalence of scurvy and of malignant types of fever, which really then manifested itself, and the union of which with the syphilitic infection may serve to explain, at least in part, as has been suggested by Hecker,* the singular constitutional effects of that disorder as it then presented itself, differing as these did from what had been known before, as well as from what has occurred since. Yet it might be no more entitled, through this difference, to be classed as a new disease then than to be held as an extinct disease now. We seem to discern a kind of confirmation of these views in a somewhat unexpected quarter, and in one utterly opposed, in most of its conditions, to those countries of Europe in which syphilis made its principal ravages towards the commencement of the sixteenth century. Yet he who is acquainted with the habits of the Fins, in all that relates to personal and domestic cleanliness, will at once agree to the existence of an adequate as well as an analogous cause, at least on this score, for the nurture and propagation of any immediate contagion. We learn from a paper by Dr. F. I. Rabbe, contained in the fourth volume of the 'Transactions of the Finnish Medical Society,' that although syphilis was unknown in Finland before the beginning of the seventeenth century, or the period of the Thirty Years' War, it has increased since then to so monstrous an extent, that within only two years, and out of its limited population, nearly 6000 cases have been treated in the Lock Hospital; so that it has been looked upon by the authorities as a more perilous visitation than war, or than all epidemics.† But we must leave this interesting discussion—Spanish, and therefore germane to our matter, as we have been forced to account its topic—contenting ourselves with thus merely indicating an opinion which we need the less regret, not having space to defend, inasmuch as the subject has been ably and satisfactorily treated, in accordance with similar views, by various recent writers, and by none with more learning and judgment than by Haeser‡ in the last published portion of his valuable 'History of Medicine.'

The fortunes of Spain were now approaching to their highest pitch

* Geschichte der neueren Heilkunde, p. 612; Aph. 27, 28.
† Norsk Magazin for Lægevidenskaben, B. v. p. 349.
‡ Geschichte der Medicin, B. ii. Th. i. pp. 184, 246.

of prosperity. The gradual concentration of its different and formerly often conflicting States beneath a single authority; the discovery and colonization of a new world, commenced under Ferdinand and Isabella; and, above all, the vast increase of influence conferred upon the country through its occupation of a first place among the rich and extensive territories inherited by Charles V., and ruled by him with so conspicuous talent and energy; all contributed to assign to it a position of the most signal prominence in the politics of the time, as well as in the annals of human culture and progress. Thus, everywhere throughout civilized Europe its language became known, its enterprise was witnessed, and its strength felt; so that it was long afterwards a well-grounded boast of Leicester, after the gallant fight at Zutphen, that if he had gained little immediate advantage by his victory, it had at least proved that the Spaniards were not invincible. The mother tongue of the soldiers of a Gonzalvo de Cordova began to be used with authority also in the fields of science and learning; and the national literature rose into vigour and reputation with the national spirit from which it emanated. It is of this literary activity during the sixteenth century, as it relates to medicine, that we shall now consider briefly a few of the evidences and the results.

Unquestionably, that which most contributed to the progress of medicine in Spain, at the period in question, was that attention to the methods and doctrines embraced in the Hippocratic writings, which then began to follow the revival of the general study of the literature of ancient Greece; and which aspired to banish from science the arbitrary conceits of the Galenico-Arabian writers, to replace them by the surer harvest to be gathered from actual observation, as directed towards the development of general truths or laws. It was no longer now the rule to form in the mind abstract notions of the ontology of disease, and to bend to these the facts which might present themselves; reasoning thus within the narrow confines of a self-imposed limit, which neither permitted the exhibition of the truth in its due range nor in its genuine essence. Men became henceforward more solicitous about the outer aspect of nature, than curious about the explanation of her inner movements; or, at least, they only sought, and sought more diffidently, to approach the latter through the former. Nor could the value and importance of classical studies, possibly sometimes under all considerations overrated now, be then, and with regard to medicine, estimated too highly; for their result was to lead men reverently back to those purer fountains which a vitiated taste had long neglected, or the source of which had only been approached near enough to pollute or pervert the stream. It is with somewhat less fitness that, in our present era, in speaking of a learned physician, the vulgar notion too frequently still pictures merely one who is versed in the dead languages, and especially in the language of ancient Greece, as peculiarly and solely entitled to the designation. Such a one, we fear, must be contented now to be accounted rather as a learned philologer than a learned physician, and even the former in not the widest sense of the term: for to be the latter in any just modern application of the phrase he has ceased to have any paramount or even adequate pretensions. Let him speculate with what ingenuity he will on the dawnings of the art among the Asclepiades; be versed as a Foesius in the writings of Hippocrates; distinguish to a shade the doctrines of the different schools; have all Aretæus by rote; and plunge and replunge with the heartiest zeal into the diversified speculations of Galen; his range will yet remain now but a narrow one, and he will have compassed little of what has become the wider and better condition of the science of medicine. Such a one, indeed, wilfully limits himself to be learned as a physician, in relation merely to the era he has made the chosen subject of his study. In so far, we are as unwilling to question his merit as to depreciate his labour. Not the less, to have paused at this is to have done injustice to all later times, and, above all, to his own times. To acquire a just conception of the science of these, or, in other words, to be learned in relation to his own period, in medicine assuredly an advanced one, he must acquire the languages spoken within the sphere of our modern instruction and civilization; while that of Greece, in real practical application to medicine, falls back into a secondary rank. But it was far different at the epoch of the revival of letters, when nearly all the light of medical knowledge was reflected from its dawn; and inquiry looked back for a while for the guide to that progress, for which its object was to prepare a comparatively independent future. Thus, with the physician of this period classical learning was not merely erudition, but was the living source of vigour and reality to his pursuits.

The Spanish Universities, profiting by this better spirit of inquiry, received that increase in their number, and development of their individual efficiency and capacity for instruction, which was necessary to correspond with the newly-awakened energies. In 1502 the University of Seville was founded by Ferdinand and Isabella, with a special right, among its other functions, to teach medicine. In Granada, the University, founded in 1531 by Charles V., received for its model, according to the then prevalent usage, that of the kindred institutions of Paris and Bologna; while in that of Saragossa, founded also by Charles eleven years later, the provision for instruction in medicine is more expressly included. In 1550 an ordinance was published, directing that in all the schools where medicine was taught, the science of anatomy should form part of the course of tuition, and should be illustrated by the necessary dissections. Instructions were given in this department by Rodriguez de Guevara, and Bernardino Montaña de Monserrate; but these were soon thrown into the shade by the far more distinguished lessons of Andreas Vesalius, who had been brought from Flanders to give the benefit and the lustre of his genius to the Spanish schools. The work of Montaña, which was published at Valladolid in 1551, when the author, we learn, had been forty-five years engaged in practice, was, there seems little reason to doubt, the first on its subject that appeared in the Castilian tongue. It is this, its first edition, constituting a volume in folio of nearly three hundred pages, which we have ourselves examined.* The treatise of Montaña is not without merit; although we must allow much for national predilections, when we find Morejon assigning to it deserts commensurate with the state of the science of the times, while yet aware that its publication followed eight years after that of the greatly superior labours of Vesalius. Montaña speaks of the study of anatomy as still unpopular in Spain; though customarily enjoined by the best physicians, and at that time taught practically and ably at Valladolid by Rodriguez, under the authority of the State. He describes the human body according to its regions; but his details are generally very meagre and defective, and his illustrative plates are of little value. In his physiological portions he gives upon the whole a judicious view of the prevalent notions of the day, but nothing more than this, and he rises above few of the prevailing errors. As then usual, the doctrines of final and efficient causes are made to play a prominent, and often a singular part. He tells us, among his other illustrations, that he has himself seen men who had abundance of milk in their breasts, and nursed their children; he assigns, probably by an inadvertency, a lower position to the left kidney than to the right; and he believes, as indeed was common in his time, that the duration of a normal pregnancy may extend to fourteen months, though between nine and ten months be the ordinary limit. But we may form a tolerable conception of what were the restraints placed on the ideas of a Spanish anatomist and practitioner of no mean pretensions in these days, from the account which he communicates of what was evidently a case of extra-uterine pregnancy, encountered by him, he informs us, in the Alhambra of Granada. It occurred to the daughter of a squire of the Marquis of Mondejar, who, after a certain amount of suffering, evacuated by the mouth such a quantity of the flesh and bones of a human being, that an infant might have been formed of them. These, he suggests, could only have come from the womb; the mouths of the veins of which they must have entered, and thence found a route into the stomach by means of the vena cava, proceeding from the liver: all which, he says, was effected through the agency of the vital spirit, and must appear impossible to any one who had not witnessed it.† After perusal of this, we may turn to his averment, in his dedicatory epistle, that it is owing to ignorance of anatomy that there were few physicians in his day who knew medicine, and many who wrote of it. May we not, in all humility, look hence to what may be our own position with reference to the future, and take such lessons to ourselves?

A second anatomical work, published in Rome in 1556, but written in Spanish, and by a native of Spain, is that of Juan Valverde. This treatise occupies decidedly a position in advance of that of Montaña; and enjoyed so high a reputation as to be four times republished, having been translated into Latin and Italian. Valverde knew well and highly appreciated the original character of the inquiries of Vesalius; and adopting for the most part the views of the illustrious Fleming, in

* Libro de la Anothomia del hombre. Folio. Valladolid, 1551.

† Op. citat., pp. 44, 57, 70, 83.

some points he developed these further. Morejon, we may notice, habitually jealous as he shows himself of whatsoever he can interpret into a slight of his countrymen, charges Desgenettes, and after him Jourdan, with having done injustice to Valverde, by representing him as having exhibited the corpses, in several of his plates, armed with sword, shield, and cuirass; with little attention to congruity or the dignity of science. With what right Morejon reprehends here what he calls the scandalous boldness of those who criticize works that they have never consulted, we cannot determine, for we have not ourselves seen the original edition in Spanish of Valverde; but most assuredly, in the Italian edition of 1559, which we have seen*—which was published, like the other, at Rome, and only three years later; and which does not profess to have any engravings added,—we find, in the plate representing the superficial muscles, a figure carrying in the one hand his skin and scalp, and in the other the short sword with which we are to suppose he has just completed the process of flaying himself; while in another plate, we have unquestionably the mutilated trunks of two corpses, each duly invested with the Roman *lorica.* In the Latin edition, published in 1589,† we can testify to the occurrence of the same figures, in the same garniture; but to this alone we should not have felt justified in adverting, as the title-page warns us that additional plates had been inserted. Nor would the matter have been worth notice, inasmuch as somewhat analogous extravagances are not rare in many of our older treatises, were it not that occasion had been sought in it for inflicting a sharp censure. We refer to another work, composed probably about the middle of the century, though not published till 1598, and after the death of the author, as presenting a broad survey of the anatomical and biological knowledge of the times, while it accompanies these with considerable amplitude of literary and historical illustration. It is that of Juan Sanchez Valdes de la Plata, and bears the title of a 'Chronicle and General History of Man.'‡ Those who may be pleased to follow us in its cursory perusal, will find in it much credulity and many fables, but they are the credulity and fables that are characteristic of the age; while they will be rewarded with occasional amusement, though we fear rarely with anything tantamount to modern notions of instruction. During this century also, Juan Tabar, one of the physicians of Philip II., acquired celebrity by the perfection of his anatomical models, a kind of ingenuity which we have seen revived in a variety of forms since the era of his invention.

It is perhaps not generally known, though it does not admit of dispute, that the interesting device of a method of imparting a large measure of instruction to the deaf-mute, is of Spanish origin, and belongs to about the middle portion of the sixteenth century. Fray Pedro Ponce de Leon, its contriver, was a Benedictine monk, of Sagahun, in Old Castile. His contemporaries allege that he was enabled by his system to teach his pupils to speak, to write, and to draw, as

* Anatomia del Corpo Humano. Folio. Roma, 1559.

† Anatome Corporis Humani. Folio. Venetiis, 1589.

‡ Coronica y Historia General del Hombre. Folio. Madrid, 1598.

well as to attain other educational acquirements; and among those who attest having actually witnessed the progress effected, we have Francisco Vallez, then eminent as a physician and medical writer. The art seems to have maintained a lingering existence in Spain after the death of its inventor, and to have thence gradually made its way into other countries, where its value has since been more enduringly appreciated. But thus it is too often with the Spaniards; who, able and generous individually, as they still are, collectively have grown inert, and prone to make little effort to avoid losing in the conduct of an enterprize the merit which was justly due for its conception. Morejon, who spares no opportunity of claiming a discovery or an invention for his country, does not always carry with him the same justice as in the instance of Ponce de Leon, and sometimes, also, arrogates for it a trophy on far less meritorious grounds. He does not, for example, judge it superfluous to enter on a considerable disquisition to prove to us* that it is to a Spaniard the world is indebted for the process of obtaining fresh from sea-water through the process of distillation; and he points, for his earliest instance, to the date of 1566, and to a work of Andrés Laguna. We shall not pretend to have traced this invention to its origin, for we have not made the attempt; but we recollect perfectly to have seen the method announced in the 'Rosa Anglica' of John of Gaddesden, an English writer long anterior to the time of Laguna, in whose pages we have had the curiosity to verify the passage.† The services to the progress of botany by the Spanish physicians and naturalists were at this time of great value; and their intercourse with their new conquests beyond the Atlantic enabled them to enrich the materia medica with many precious resources, which have never since ceased to justify their first reputation. An event of importance in the history of disease, and one which has recently become entitled to increased consideration among the physicians of this country, is the first observation in Europe, about the year 1530, of the affection which we now recognise under the name of diphtheria; but which was then known in its epidemic form, especially in Italy and Spain, as the *cynanche maligna*, or, more peculiarly, as the *garrotillo* of the latter country, where it received this designation owing to the resulting symptoms of suffocation having suggested, in their intensity and fatality, the idea of the death by strangulation produced by the penal instrument of the *garrote*. The whole doctrine of fevers received also from the Spanish Medical School of this time an extensive and enlightened consideration, and the treatment of this class of diseases was improved and rationalized.

And why should we hesitate to follow Morejon, in conceding to Spain the credit of having first delineated that form of mental disorder, now so generally admitted under the designation of monomania; or refuse a place in medical history to Cervantes, although no physician, for the admirable manner in which he has marked the features of a

* Morejon, vol. ii. p. 64.

† "Destilletur aqua sulsa per alembicum suaviter, et erit aqua dulcis."—Rosa Anglica (1492), p. 173.

morbid state which his sagacity enabled him to recognise, and in behalf of which, through the ever-living example he has depicted, he has claimed so genially the sympathy of the humane in succeeding ages? It is indeed with a gentle spirit that Cervantes touches the folly of Don Quixote; and it is with a wonderful tact that we are led to smile at the tale of his extravagances, without a tincture of contempt mingling for an instant with the feeling of the ludicrous to which they minister. Nor need we wonder that the nobleness of the motives apparent in the "cuerdo loco," together with the felicity of their exposition by the writer, should carry us beyond the absurdities of the actions, scarcely, perhaps, so far as to warm us into affection, yet to lure us into something nearer akin to respect than to compassion. It was not in accordance with the then almost everywhere prevailing spirit, by which lunacy had been customarily treated rather as guilt than as infirmity, that the author never obtrudes a single taunt or rigorous expression, to leave us to infer his sense of a degradation incurred by him whose reason was partially destroyed. Those who attempt Quixote's cure attempt it without harshness, and proceed by humouring delusions which their own reason teaches them it would be in vain to oppose by dint of reason. In the adaptation of the change in the character of the insanity, also, in the second part, we have evinced to us the closeness of observation which Cervantes must have employed to gather the materials for his picture. But the observing, as well as the graphic powers of this unique writer were everywhere great; and it has often occurred to us to admire the singular force and vividness of his picturesque descriptions, whether of mountain or of champaign scenery, the merits of which have only escaped universal attention, because they have been surpassed by still superior characteristics. It would be an interesting inquiry, were it a possible one, which should have for its purpose to ascertain in how far the sympathy, or even the esteem, with which Cervantes was able to surround the sufferer from insanity, may have gradually and insensibly diffused a lesson, the fruits of which were to appear happily afterwards in the philanthropic exertions of a Pinel and his followers.

But let us turn now to a notice of a few others of the chief names that illustrated this crowning period of the medical history of the Peninsula. Among the more prominent of these is that Andrés Laguna to whom we have already made a passing allusion. Laguna was born in Segovia, in 1499, studied chiefly at Salamanca and Paris, and early distinguished himself for the depth and extent of his classical acquirements. It was not till 1539 that he graduated as Doctor of Medicine at Toledo, for doctors in these days were not plants of a rapid growth: after which, being attached to the service of Charles V., he accompanied that monarch to Flanders, residing afterwards in various European cities, and gaining everywhere celebrity for his eloquence and learning. In 1557, we find him once more settled in Spain, and resident at Segovia, where he died in 1560. His writings related to many departments of the science of medicine. One on anatomy, of which we may note that it presents a description of the ileo-cæcal valve, was entitled, 'Anatomica Methodus, sive de Sectione Humani Corporis Contemplatio,' and was published at Paris in 1535. He then constructed an 'Epitome of the Works of Galen,' which passed into several editions. To this was appended, among other tractates, a disquisition on "Medical Weights and Measures;" in which he takes occasion to point out the irregularities practised with respect to these by the Spanish apothecaries, who appear, in their confusion and misappropriation of a diversity of weights, to have shared the reproach then, which our own compounders of medicine inherit now. A treatise of some interest, published by him in 1551, was his 'Methodus Cognoscendi, Extirpandique Nascentes in Vesicæ Collo Caruncnlas.' He translated much from Aristotle and other Greek writers into Latin; and, what is more to our present purpose, while adverting to Spanish medical literature in its stricter sense, he transferred to his native language the work of Dioscorides. The first edition of this performance does not appear to have been known to Morejon (vol. ii. p. 264), who mentions none antecedent to that published in Salamanca in 1566, while that of 1586 is the first mentioned by Antonio.* We have now before us, however, an example of an edition published in 1555, and therefore five years antecedent to the death of the author,† so that the work does not appear in the character of a posthumous publication, as is left to be inferred by the Spanish authorities. No one can bestow attention on this translation by Laguna, and on the ample comments with which he illustrates his original, without deriving from it that satisfaction which justly attaches as well to the merits of the performance as to the character of the writer. The author does not rise in it wholly beyond his times, for in that no man ever succeeded. Yet it is only necessary to compare him with by far the larger proportion of his cotemporaries, who have entered on the subject of the history and actions of medicines, to discern his manifest superiority. Nor was this acquired at any light sacrifice. Laguna indicates to us, in the dedication of his work, the many laborious journeys he had made, the mountains he had scaled, the declivities he had descended, risking his life among ravines and precipices, with the cost attendant upon this, and upon the acquisition of specimens from remote countries, to the expenditure for all which he had devoted the greater part of his substance. He intersperses his work with many illustrative facts or anecdotes, chiefly derived from the stores of his own experience. After a long period of sleeplessness, induced by anxiety and over-exertion, he derived great benefit, he informs us, in his own person, from the use of a pillow stuffed with the leaves of henbane. Opium, he considers, should only be resorted to as a last resource, when other remedies had failed. While treating of the deadly nightshade and the character of the delirium it produces, he

* Bibliotheca Hispana Nova, tom. i. p. 78.

† Pedacio Dioscorides Anazarbeo: Acerca de la Materia Medicinal, y de los Venenos mortiferos, traduzido de lengua Griega en la vulgar Castellana, y illustrado con claras y substantiales annotationes, y con las figuras de innumeras plantas exquisitas y raras. Folio. Anvers, 1555.

adverts to the delusions connected with the subject of witchcraft, and leans evidently, though with some cautious reserve, inspired doubtless by the terrors of the Holy Inquisition, to rational views as to the mental condition of the unfortunates charged with, and pleading guilty to, this imaginary crime. Yet that he does not spare the clergy, where they exposed themselves deservedly to his censure in anything apart from the belief of Mother Church, is so clearly evinced by more than one passage, that we may almost class Laguna with those writers who, like Erasmus, prepared the way for the diffusion of the Reformed doctrines, by lowering the faith in their clerical opponents.* The cuts with which the volume is illustrated are of superior execution for the time, and many of them characteristic.

We may pause here for an instant to renew our acquaintance with Rodrigo Ruiz Dias de Isla, in order that we may do him the justice to mention, that he seems to have been thoroughly on his guard with respect to the risks attendant upon a careless or excessive use of mercury in syphilis; while he naïvely discloses to us some notion of the extent of the practice and profits of a syphilidologue of the sixteenth century, when he concludes his account of this powerful remedy by telling us, that "he does not now need to say any more of mercury, unless that he has gained by it more than twelve thousand ducats." Continuing our notices of the writers on the materia medica, we remark here of the instructive work by Nicolas Monardes, on the drugs imported from the Spanish possessions in America, published in 1565, that a Latin translation of it by Clusius appeared in 1574,† and an English translation in 1580,‡ to both of which we have referred with interest. An analogous treatise, relating to the materia medica of the East, is that of Garcia de Orta, a Portuguese, first published at Goa in 1563, but afterwards translated into Latin, also by Clusius, as well as into Italian, English, and French. It is only with the Latin translation, or rather re-compilation,§ of which five

* As an example of this, and of the habits of the times, as well as of Laguna's generally forcible and lively style of writing, we translate the following passage, in which our modern advocates of temperance may possibly be glad to recognise a worse condition even than that which occasionally so sternly claims their attention in our own day:—"And, what is worse than all, this infernal passion (I mean drunkenness), which formerly prevailed solely in Germany, and in the northern countries, is now extending itself through all Italy and Spain, exercising its beastly tyranny; and that which formerly oppressed and enslaved only plebeians and the vulgar, holds now a joint empire over nobles and princes, over men of letters, and, what cannot be expressed without tears, over the clergy, who, at least, of all the world, should have given us an example of sobriety and temperance. Thus it is that, for our sins, drunkenness has become so prevalent, so exalted, and so honoured throughout all Europe, that we have only to live awhile to see it canonized as a saint; it being manifest that there is no occasion of life, however important, whether it be the birth of a son, a betrothal, marriage, or funeral, or whether it be a bargain or a contract, into which it is not the first to thrust itself. Would you have more, unless that it is now considered by every one that he does not treat his guest handsomely or liberally, if, having received him a man, he does not send him home a beast? And in what respect a beast? I should say, rather, a cask, a stone, or a block." Op. cit., p. 504.

† Simplicium Medicamentorum ex novo Orbe delatorum quorum in Medicina usus est Historia. 8vo. Antverpiæ, 1579.

‡ Medicinall Historie of the things brought from the West Indies. 4to. 1580.

§ Aromatum et simplicium aliquot medicamentorum apud Indos nascentium Historia. 8vo. Antverpiæ, 1574.

editions have appeared, and of these the earliest in 1567, that we have had the opportunity of forming an acquaintance. The work of Orta served as a foundation for that of Christoval Acosta, published at Burgos in 1578,* of which an Italian translation appeared at Venice in 1585,† illustrated, like the original, with figures. From a perusal of this work, which we have seen in both forms, we gather that Acosta had travelled extensively in India, China, and Persia, with the express view, as he tells us, of examining the plants used in medicine; and he was unlucky enough during his enterprise, we learn from Juan Costa, to have suffered captivity "in Africa, Asia, and China." Much of the information he had thus painfully accumulated is curious; and, varied as it is by personal anecdotes, may still be considered neither unattractive nor valueless. His account of the opium-eaters of the East, with the Chinese, as now, bearing his part among them, will at least exculpate our traders from any charge of having been the originators of this vicious traffic with the Celestial Empire. As to the quantity of solid opium taken, he mentions an individual in Malabar whom he knew to swallow five drachms daily; though from twenty grains to a drachm was the usual allowance with others. Acosta introduces an account of the method of preparation and the effects of bang,‡ as obtained from the Indian hemp. A work by Diego de Sanctiago,§ on 'Distillation as a Branch of Pharmacy,' which we have examined, may be mentioned here as curious from its rarity, as well as from the nature of its contents, and the pretensions and hardihood of its promises.

With Luis Lobera de Avila, though a physician and writer of great mark in his day, it will not be necessary that we should long detain ourselves. Lobera was one of the physicians attached to the person of Charles V., whom he seems to have accompanied in most of his numerous voyages; in the course of which, according to one of his editors, Dr. Francisco Vargas, he proved himself "hombre de todas sillas," having distinguished himself as highly in his suit of mail, when it was necessary to fight, as by his learning and skill when called upon to treat a patient. The collection of his works which we have seen, in which, however, the whole of them are not included, is that published at Alcala de Henares, in 1542. No date prior to this is assigned to any of them separately, by either Morejon or Antonio; though it is evident that at least the 'Vergel de Sanidad,'‖ or 'Garden of Health,' of which we have already a German translation issued at Augsburg in 1531, must have appeared in Spanish at an earlier, though with difficulty determinable, period. Those who are curious to ascertain what was the manner of diet of the knights and nobles of Spain

* Tractado de las Drogas y Medicinas de las Indias Orientales. 4to. Burgos, 1578.

† Trattato della Historia, natura, et virtu delle Droghe Medicinali, et altri Simplici rarissimi, che vengono portati dalle Indie Orientali in Europa. 4to. Venetia, 1585.

‡ Tractado de las Drogas, pp. 360, 412, 415.

§ Arte Separatoria, y modo de apartar todos los Licores, que se sacan por via de Destilacion. 12mo. Sevilla, 1598.

‖ Vergel de Sanidad: que por otro nombre se llamava Banquete de Cavalleros y orden de vivir. Folio. Alcala, 1542.

in these their palmy days, will find much to interest them in this treatise: and they may be pleased to discover that the wines of the Peninsula were as distinctly characterized, then as now, by their superior strength to those of France; while of the qualities of the "double stout" (*dupla biera*) of the times, and its composition of water, grain, and hops, they will find not less precise information. His account of the latter description of beverage, however, is by no means so minute as has, somewhat later, been supplied by Placotomus, in his treatise 'De Natura Cerevisiarum,' to which, with all due solemnity, we refer the reader. It may be only worth while to add, that Andres Laguna, writing in 1555, speaks of the ales of England as at that time surpassing all others in quality. The most important, by far, of the works of Lobera which we have examined, is his 'Silva de Experiencias;'* a compendium of practical medicine, in which possibly we find few traces of originality, but which may be held to represent, fairly and judiciously, the medical knowledge of his period. We may gather, here and there, in Lobera's pages, as in those of Laguna, some graphic traits of the times.

A far more remarkable writer than Lobera, in as far as general topics are concerned, was Francisco Villalobos, a reminiscence from whom suggests itself to us here, but whom it would be unjust, on every other ground, to pass without a brief notice. We extract the following from his 'Glosa a la Cancion de la Muerte,' and recommend it to our readers for the lesson it conveys from his medical experience, with reference to the always essential consideration for the physician of the state of society in the midst of which his art is exercised, as exemplified in the striking picture it elicits of a sick room of a grandee of Spain. "Since," he says, "in the end all power changes into vanity, for the more manifest proof of this I shall relate here what I saw in Saragossa, while the king was residing there before his marriage. The Grand Chancellor died then of a sudden attack of apoplexy. He was a man who, next to the king, held the chief authority throughout the kingdoms, and was obeyed by all the nobles and magistracy. While rendering up his soul to Him who gave it, his bed was surrounded by his domestics, among whom were a barber-valet, and some servants of the pantry, who, in no long space of time, had been indebted to their master's favour for many thousands of ducats. One of these had chanced to fall asleep while reclining on the pillows of the Chancellor, gaping and snoring loudly; and his fellows, having taken the crucifix from their master's breast, and placed it with great parade on that of the sleeper, had begun, amid bursts of laughter, to chant a funeral service." Contemplating with consternation this shocking spectacle of the unfortunate man and his unfeeling attendants, Villalobos proceeds with many excellent reflections, which we must allow ourselves to pass over, though they indicate finely the thoughtful and independent spirit which characterizes the man. And, indeed, we suspect that Villalobos showed customarily too much

* Remedio de Cuerpos Humanos, y Silva de Experiencias en Medicina. Folio. Alcala de Henares, 1542.

honesty of thought and terseness of expression to find the result always profitable for those worldly interests which he may have sought, through the lesson of such scenes as that he has here painted, to qualify himself to disregard. At least, we know from Capmani that he was not rewarded according to his merits; while we seem to have the right to feel assured that never in his poverty did he even stand under that suspicion which induced our Hobbes to ask for himself, as in defence: "Do I flatter the king? Why am I not rich?"* It was for the supple mediocrity of a Ruiz Diaz to gain his twelve thousand ducats by a single medicine in a single disease: for the sterling worth of a Villalobos remained the severer lesson embodied in the apophthegm of Beaumarchais, that "pour gagner du bien, le savoir-faire vaut mieux que le savoir." Antonio characterizes Villalobos as "disertus et eloquens sermone patrio;"† and no one was more conspicuous in his time for the elegance and purity of his Spanish style, so that he has been accounted one of the great improvers of the language. A remarkable piece by him is his poem, 'Sobre las Bubas,' or On Syphilis; a curious production, on a theme in the management of which he preceded Fracastorio, the whole of which is given by Morejon in an appendix to his first volume. The date of the birth of Villalobos is uncertain, but he survived till the middle of the sixteenth century.

It is not universally admitted that the celebrated passage in the 'Christianismi restitutio' of Michael Servetus, published in 1553, clearly indicates a knowledge on his part of the proper mechanism of the lesser, or pulmonary circulation, and places him therefore in the position of the first oppugner of the old Galenical notions of the heart's action, and the first definite pioneer in that succession of faint perceptions of a coming truth which at last terminated in the full light of the discovery of Harvey. Yet, from a consideration of the passage, we are inclined to agree with Morejon that this degree of credit really belongs to him: and it becomes therefore an honour to be justly awarded to Spain; for Servetus, though a graduate of Paris, was a native of Villanueva, in Arragon. Not less a liberal philanthropist than a zealous Catholic, Morejon, in the one capacity, deals with the heresy of Servetus, as with that of his persecutor Calvin, by regarding them both as monomaniacs; while, in the other, he denounces the barbarity of those who used the faggot to purify or sustain the faith. How terrible is the cry of agony of the victim from his pile at Geneva! "Throw more wood on the fire, that I may die quickly! What! Are not the hundred pieces, and the collar of gold you took from me when I was seized, enough to buy wood to burn me quickly? Oh! wretched me! More wood!" But turning to a more genial subject, this is afforded to us by a consideration of the character and the career of Luis Collado, one of the most distinguished physicians and honourable men of his time. Collado was a native of Valencia, in the university of which city he at first pursued his studies, and afterwards occupied a professorial chair. Himself a skilful anatomist, he was an enthusiastic admirer of the merits of Vesalius, to whom he owns himself indebted

* Hobbes' Answer to Bishop Bramhall. † Bib. Hisp. Nov., tom. i. p. 498.

for valuable instruction, and whom he defends warmly against the grossly abusive animadversions heaped upon him, with the literary courtesy of the day, by Jacobus Sylvius. The writings of Collado are chiefly directed towards an exposition of the doctrines of Hippocrates and Galen, and to subjects connected with the materia medica. The well-known anecdote regarding him, when called upon to visit the Marchioness of Mondejar, wife of the viceroy of Valencia, is so illustrative of the country and the times, as well as of the character of the physician, that we may be excused for repeating it. On his first introduction to the sick room, he felt the pulse of the noble dame while standing by the side of the bed. On retiring, he was followed by an attendant, who signified to him that the physicians of Castile were accustomed to feel the pulse of a lady of her distinguished rank while on their knees. "But I am Collado, and I kneel only to God," was the indignant answer, as he left the palace, resolved not to renew his visit; a determination which he only abandoned on the entreaty of the Marquis, and the assurance that thenceforward he should be presented with a chair. Another, and more trivial anecdote connects Collado with Francisco Valles, perhaps, if we except Mercado, the most distinguished in the catalogue of his cotemporaries and colleagues. Valles occupied the position of first body physician to Philip II., when the reputation of Collado drew towards the latter also the attention of this monarch, who desired to attach him to the court in a similar capacity, and more especially to confide to him the post of physician to Queen Isabel. But Collado neither loved the glitter of courts, nor was so dazzled by their pageantry as to be blind to the turmoil that heaved uneasily beneath it; and he was better pleased to remain in his calm retreat at the University, engaged in the faithful exercise of his duty among his scholars. There was therefore an earnest motive hid under his playful reply, as he declined the proffered honour, saying that it would be the most monstrous thing in the world were he to accept the second position where Valles had the first, for it would be to place a hill (*collado*) below a valley (*valle*).

As to Francisco Valles himself, he was a native of Covarrubias, in the diocese of Burgos, and studied at Alcalá, where he graduated in 1553. Little is known of that early career which led him, at a later period, to so marked distinction, that he received from his cotemporaries the epithet of "the divine;" a title said to have been first bestowed upon him by Philip II., in a burst of gratitude for the relief that this monarch, patient of toil but of nothing else, had experienced through the prescriptions of the physician under a violent paroxysm of gout. His influence with Philip gave him great authority, which he well employed in the advancement of the means for the promotion of that learning among others in which he was so conspicuous a proficient himself. The fame of Valles soon extended to other countries, and his writings were especially esteemed in France. These writings are very numerous; and nearly all of them, however neglected now, passed through a succession of editions during the century which immediately followed their publication. Among them are commentaries on different portions of the works of Aristotle, in discussing whose topics Valles displayed great metaphysical acumen. His treatise, published in 1588, 'De iis quæ scripta sunt physice in libris sacris,' if we may judge from the analysis by Morejon, is apparently a work of remarkable interest. With his 'Methodus medendi,' first published in 1589;'* his 'Commentaries on Galen;'† those on the 'Prognostics,' and on the 'Diet in Acute Diseases,' of Hippocrates;‡ and with his various dissertations on the urine, on the pulse, and on fevers, we are ourselves acquainted, and we have not consulted them without admiration of the sterling qualities of the writer. The judicious practical maxims and sagacious appreciation of facts evinced by Valles, and illustrated rather by a choice than by a profusion of learning, bespeak the master in his art and the accomplished scholar; if he could not escape altogether from the defects of his times, the taint of which nowhere appears more strongly than occasionally in the quality of his therapeutic resources. We have given, from Laguna, an account of the prevailing proneness to intemperance in the sixteenth century. Let us see, in a short extract from Valles, what remedies a court physician, writing prior to 1589, judged advisable, and actually employed, to check the propensity: "Iis, qui usque ad ebrietatem vini suavitate capiuntur, solemus propinare incautis vinum in quo stercus aliquod, humanum maximè, dissolutum sit, aut spuma sudoris equorum, aut in quo sit anguilla suffocata, vel putrefactæ uvæ. Ea enim abominabile vinum reddunt, et ejus abominationis vestigium in imaginatione imprimunt."§ The writings of Valles have been greatly lauded by Boerhaave. He died at Burgos in 1592.

It is enough to record the fact, that the first treatise on dentistry in Spain, the work of Franco Martinez, a resident in Valladolid, was published in 1557, in that city; while Juan Tomas Porcell, who, in 1565, published at Saragossa a treatise on the Plague, has an undoubted claim to remembrance, as reputed to have been the first who opened the bodies of those dead of that disorder. Porcell experienced practically that bloodletting was prejudicial in the pestilence; and he accounted for this hypothetically, on the grounds that, as the disease proceeded from bile separated from blood, to remove the blood was to remove what restrained the bile, and so produce frenzy and debility: giving us here, at least, a valuable practical fact, while showing us at the same time, what it is well for us now and always to remember, how independent this may be of a theoretical explanation. But with this slight notice, and passing over others altogether, whose labours it would have been agreeable to us to have indicated had the scope of our remarks permitted, we must reserve a more considerable, though still a scanty space, for one so deservedly prominent as Luis Mercado. Born at Valladolid in 1520, Mercado held for some time a chair in the university of that city; afterwards occupied the position of body

* Methodi Medendi Libri quatuor. 8vo. Lut. Par., 1651.
† Commentaria in Galeni Libros sex, et Tractatus Medicinales. Folio. Francof. 1645.
‡ Commentaria in Prognosticum Hippocratis. Folio. Aurel., 1655.
§ Methodus Medendi, lib. ii. cap. 9.

physician to Philip II., and his successor, Philip III.; maintained throughout life a distinguished reputation, as well as a high character for unpretending worth and probity; and died, in 1606, of calculus of the bladder, having reached the age of eighty-six years. It was rather for the value and extent of his practical knowledge than for his philosophical acuteness, that Mercado was esteemed by his cotemporaries, who reckoned him superior to Valles in the former respect, if inferior in the latter. His principal works were collected in five folio volumes, usually bound into three, many editions of which followed that first issued in 1605, at Valladolid. That which we have consulted was published at Frankfort, between 1614 and 1620.* The earliest of his treatises, in their separate form, according to Antonio,† was the 'Methodus Medendi,' published in 1572. He appears to be the first Spanish physician who described accurately the epidemic diphtheria, or angina maligna, already alluded to as popularly designated in Spain under the name of *garrotillo*. Mercado's description occurs among his 'Consultationes Medicinales,' of which it is the fifteenth; and relates to the successfully treated cases of the son of a Spanish nobleman, and of the nobleman himself, the affection having been apparently communicated through immediate contagion from the former to the latter. The details, which occupy upwards of seven closely printed folio pages, are exceedingly interesting, and their examination may alone thoroughly satisfy any one, as they have satisfied us, of the learning and judgment, and other eminent qualities of the writer. His remarks on the treatment of petechial fever, or *tabardillo*, so long an object of familiar inquiry among the Spaniards, evince equally his sound practical tact and talent for observation. Morejon speaks of Mercado as one of those who believed in the American origin of syphilis. Such, however, is by no means the impression which we have been able to gather from a perusal of his treatise, 'De Morbo Gallico,'‡ in which he seems to leave this question almost untouched, and wholly undecided. It is his singular idea of the disease, laid down by him as a certain dogma,§ that it has its pathological seat and origin in the liver, although it have its initiative in an external contagion, prior to its reaching the liver. In 1593, Philip II. employed Mercado to draw up two compendious treatises, which were to serve as text-books for an examination of a practical nature, to be undergone by all graduated bachelors of medicine and all surgeons, after they had been two years engaged in practical studies; failing in which, they were to be debarred from the further exercise of their functions. Philip III. also, in 1603, confided to Mercado the charge of preparing a treatise, designed to diffuse correct notions with regard to the nature and treatment of the plague. It may be interesting, though perhaps scarcely consolatory, to a modern medical writer to learn, that for the execution of this work, extending to fifty folio pages, the king presented the author with a remuneration of two thousand ducats.

* Ludovici Mercati, Medici Hispani, Opera Omnia, Medica et Chirurgica, in quinque tomos divisa. Francofurti, 1620.

† Bibl. Hisp. Nova, t. ii. p. 50. ‡ Opera, tom. ii. p. 629. § Ibid., p. 630.

Of the principal surgeons of this period, whose names it would be an injustice to omit, our attention is first directed to Francisco Arceo, whose little treatise on the Cure of Wounds long enjoyed an extensive reputation. Arceo was born at Fregenal, about the year 1493, and survived to a great age, having been distinguished as an operator, according to the preface to his works by Montano, even after he had passed his eightieth year. Part of his educational studies were pursued in the monastery of Guadalupe, then, and long afterwards, celebrated for a medical school rivalling the universities, and possessing the advantage of an excellent hospital, with other valuable means of instruction. His works, which are of very modest dimensions, and include only two treatises, were first printed at Antwerp, in 1574, and appeared in various editions and translations subsequently. That which we have examined was published at Amsterdam in 1658.* In his comments on the treatment of wounds, Arceo did not aim at their cure by the first intention; nor did he, as Morejon asserts, dispense with the use of tents, in at least the treatment of the larger wounds.† Still, this treatise contains many excellent observations, and details many remarkable cases and cures. In his second treatise, which relates to the treatment of fevers, we have little more than a loosely strung collection of prescriptions, conceived in the stoutest spirit of polypharmacy, and with scarcely the intervention of a vestige of symptomatology or pathology. Reasoning from the analogy of his good fortune in the treatment of penetrating wounds of the chest, he aimed at the cure of the ulcerated lung in pulmonary consumption; and with such success, he tells us, that his patients under his method became "pingues, et boni habitus, et bene colorati, ut nihil supersit expetendum."‡ Our modern healers of phthisis may find it an advantage to add to their fuller knowledge of the pathology of the disease, the information, if they use it rightly, that this happy result was produced by pills of agaric, taken with infusion of scabious, and syrup of roses and maiden-hair. But the surgeons for whom we must assert a title to attention far beyond that of Arceo, whether in Spain or elsewhere, are Daza Chacon, Juan Fragoso, and Hidalgo de Aguero. What vivid reminiscences, connecting themselves with scenes not willingly forgotten while energy and gallantry continue to be prized, nor yet cheerfully remembered while associated with the thoughts of the carnage and suffering that necessarily attended them, crowd around us as we contemplate the history of the fortunes and the labours of especially the first of this eminent trio! The campaigns of the fifth Charles; those of the stern, yet conscientious Alva; the gallantry of John of Austria, with the story of his almost unmatched victory of Lepanto, and the unhappy accident of his death; the sad career of Don Carlos; the devoted service and heroic end of Quixada; with much more of the stirring events and striking or curious recollections of the times, all exhibit their

* Franciscus Arcæus: De recta curandorum vulnerum ratione, et Aliis ejus artis præceptis, Libri ii. 12mo. Amstelodami, 1658.

† Op. cit., pp. 111, 112.

‡ Ibid., p. 310.

distinct traces before us in the important work on surgery* which has been left to us by Daza. We gather from it also nearly all that we know of the biography of the writer himself.

Dionisio Daza Chacon is said by Morejon to have been born at Valladolid in 1503, but we glean evidence from his treatise that this date should be placed seven years later. He entered upon the practice of his profession, he himself tells us, at twenty years of age, and devoted to it a period of fifty years; retiring at the time when Philip II., of whom he had been appointed one of the surgeons, proceeded to take possession of Portugal. As this event took place in 1580, and as in Daza's announcement we have precisely seventy years of his life accounted for, we become entitled, through so plain an induction, to correct the statement of the Spanish historian, and to assign 1510 as the proper year of Daza's birth. That of his death it is necessary to leave undetermined: but he must, at all events, have reached a very advanced age; for we find him, in 1596, when he had attained his eighty-seventh year, appending his name to a favourable censorship of the work of his fellow-surgeon, Hidalgo de Aguero. There is evidence to show that Daza's treatise must have been completed about the time of his retreat from active service; though the first edition mentioned by Antonio is that of 1605, subsequent to which three others, at least, have been published. Whoever will take the trouble, like ourselves, to examine the somewhat bulky volume of this surgeon, and compare with it the works of any of his other more remarkable cotemporaries, will be at no loss to account for the distinguished reputation he acquired in his own day, of the extent of which we have many evidences, derived from among those of his countrymen who were best competent to form a judgment. Willingly paying our tribute to the acuteness and to the happy ingenuity of Ambroise Paré, still, with regard to all the more solid acquirements of the mind, Daza was unquestionably his superior; while in depth and variety of erudition, and in the capacity of judging of the observations of others, wherever for that purpose was required the application of a high intelligence, with power of abstraction or logical deduction, the inferiority of the French surgeon was still more conspicuous. Hence arose the marked credulity of Paré, for which scarcely any prodigy of middle-age superstition was too extravagant or repulsive. Hence the comparative scrupulousness of Daza, who, preferring to throw the responsibility of his fame upon the thinkers of his age, was indebted to no happy accident, or single prominent discovery, for the basis of a reputation, the light of which may have been less brilliant than that of Paré, but was more equably and expansively diffused.

Nor could the endowments of Juan Fragoso be brought into equal competition with those of his more able countryman, although the work of this surgeon seems to have been generally the more popular, and to have passed rapidly through numerous editions. The impression

* Practica y Teorica de Cirugia, en Romance y en Latin. Compuesta por el Licentiado Dionisio Daza Chacon, Medico, y Cirugiano de su Magestad del Rey Don Felipe Segundo. Folio. Valencia, 1650.

of Fragoso's treatise, which we have examined, bears to be the sixth,* and is dated in 1601, while the first had been apparently issued only ten years earlier. But there was much even in the relative inferiority of Fragoso to conduce to this greater popularity; for the comprehensive erudition of Daza, embracing all epochs of his science, from its dawn to its relative maturity, extended sometimes to what, for the everyday world, was beyond the common wants and the common intelligence. Still, the treatise of Fragoso must rank as a work of high merit, though not of the highest merit; nor, above all, of marked originality. His consideration of medico-legal cases is curious for the time, and his illustrations from actual observation, among which we may point to his description of a leper, are occasionally very instructive. In discussing the means of determining whether a person had been hanged while in life, or suspended after death, his description† of the effect of the cord under the former condition, as causing a discoloured, livid, or black mark, with the skin contracted or wrinkled, is tolerably graphic. He alludes to the death of Don John of Austria, as reported by Daza, and repeats as an unquestioned fact,‡ that this cherished hero of the soldiery perished from loss of blood, consequent upon opening a hæmorrhoid with the lancet; a disaster which, along with the affecting details regarding it, as these are briefly narrated by the former eminent surgeon, seems to have escaped the attention of our best historians, by whom the fatal issue has been usually attributed to fever. He quotes a story§ from the renowned Fra Luys de Granada, which, with its severe discipline, we recommend to the attention of our writers on Gout, for the behoof of their generally not singularly abstemious patients. An Italian, a thorough and veteran martyr to the disease, fell into the hands of his feudal enemy, who for four years kept him imprisoned in a tower, giving him as his sole sustenance a scanty supply of bread and a jug of water daily. At the end of this period, when a lucky chance freed him from his prison, he was freed from his disorder. Fragoso published an earlier work in 1570, the subject of which also relates to surgery. None of the Spanish authorities whom we have consulted record either the date of his birth or of his death.

Bartolome Hidalgo de Aguero, a native of Seville, died in 1597, at the age of sixty-six, and was born, we thus learn, in 1531. He held for some time a chair of Surgery in the university of his native city. In the year preceding his death, we find a censorship of his principal book by the veteran Daza, who speaks of it as a very learned and laborious work. It was not published, however, till 1604, or till seven years after the death of the author, when the first edition, which is that now before us,|| was issued under the care of his son-in-law. One or two minor works appeared during his lifetime, and other editions of

* Cirugia Universal aora nuevamente emendada y anadida en esta sexta impression. Folio. Alcala de Henares, 1601.

† Op. cit., p. 551. ‡ Ibid., p. 448. § Ibid., p. 443.

|| Thesoro de la verdadera Cirugia y via particular contra la comun. Folio. Sevilla, 1604.

his principal treatise were published subsequently. Hidalgo, or his son-in-law in his name, assigns what we may here refer to as the usual reason proffered by the writers of the day for composing in the vulgar tongue, their motive being the desire that their works might in this way become generally useful. The signal merit of Hidalgo, for which the art of surgery must remain for ever indebted to him as one of its most sagacious promoters, lay in his advocacy of what he designated as his *via particular:* according to which almost every description of wound was to be attempted to be healed by what was in so far a revival of the method by the first intention, the rules for accomplishing which he extended and modified; while all the vile apparatus of dilatation, tents, and digestive ointments were to be happily excluded from the prevailing routine. With the same wise opposition to all officious and often perilous interference, the use of the trepan and the levator was banished, or nearly banished, from the treatment of injuries of the head. This great innovation, so happy in its effects, and so precious to the art, was not received without much resistance on the part of other surgeons at the time; but the experience of its benefits, at a period when no weapon rested long in its sheath, soon became too notorious not to meet many sufferers who found reason to confide in its efficacy, and who were glad to profit by the gentleness of method and promptness of cure by which it was attended. Even the bullies of Seville, we are told,* gathered added courage during his life, and set to in their encounters with little fear of the consequences, committing themselves, as they crossed swords, to God and to Hidalgo, to whom, with pious ruffianism, they commended themselves as to a kind of saint. But Hidalgo, if he have been a dexterous and judicious surgeon, is not equally skilful with his pen, and we find little in him to remind us of the wide attainments and erudition of Daza. Through a maze of much confusion, we arrive only with difficulty at a full comprehension of his methods; nor when we enter into their details do we discover, however great his improvements on what had preceded him, that he is by any means uniformly exempt from false and extravagant notions of the nature and uses of remedies, or that his rules are always so sound or so simple as those which form the basis of modern practice. He speaks of his plans† as having been developed with great labour in the hospital of the Cardinal at Seville, where he had acted as surgeon for more than twenty years. The wound of a cutting instrument, he remarks,‡ is healed by the mere approximation of its edges, because it contains nothing to impede the union; and now experience has shown, he adds, that by removing whatever intervenes, such as extravasated blood, or foreign substances, or the existence of abnormity of temperature, to prevent a union, we may attain a like success with contused or lacerated wounds, for which, therefore, a cure by the first intention is to be similarly attempted. He applies these maxims to the treatment of a great variety of wounds, subjecting his processes to modification according to the precise seat and nature of the injury. An able,

* Op. cit., Prolog., p. 3. † Op. cit., Trat. i. cap. 26. ‡ Ibid., Trat. iv. cap. 6.

though eccentric English writer, Sir Kenelm Digby, born six years after Hidalgo's death, gives the secret of his success in the cure of wounds by the powder of sympathy, in his advice to "cast away all your playsters, onely keep the wound clean and in a moderate temper, 'twixt heat and cold;"* but we have seen how fully all that was worth in these directions, profitable as they were in their time in spite of their association by Digby with a ridiculous superstition, have been anticipated by the more reasonably developed precepts of Hidalgo. Those who will turn to the work of Cesare Magati, for which, as proposing a reputed new method, he attained great credit, also after the death of Hidalgo, will find† that neither in the treatment of wounds generally, nor of injuries of the head in particular, are his practical rules more judicious, if in the latter they are not sometimes less judicious, than those of the Spanish surgeon, though it must be confessed they are detailed with a more precise and scholarly diction. In leaving the surgeons of this period, we may select from among those of lesser note the name of Francisco Diaz, a graduate of Alcala, and a pupil of Collado at Valencia, who published in 1575 a Compendium of Surgery, which we have not seen; and in 1588 a treatise on the Diseases of the Urinary Organs,‡ which we have consulted, and which was in high esteem in his day. His account of renal calculi, with their effects and symptoms, is judicious and accurate for the time, and he recommends among the rest a recourse to mineral waters in their treatment. In strictures of the urethra he introduces what he calls a novel method of cure, which consists in a modification of that by caustic bougies, with, in otherwise intractable cases, the employment of the canula, armed with a stylet,§ the danger of which he confesses, but justifies on the plea that it was only resorted to by him to avoid a greater danger.

Among not a few Spanish ladies who distinguished themselves by their general studies in the sixteenth century ("rarum in sexu decus," says Antonio,|| "quamvis inter Hispanas minus rarum"), we find one who rendered herself conspicuous by the views she promulgated regarding the science of medicine. Doña Oliva del Sabuco de Nantes Barrera, a native of Alcaraz, in la Mancha, was the author of a treatise on the Philosophy of the Nature of Man, published at Madrid in 1587; of which at least three editions appeared afterwards, that of 1728, which is now before us,¶ being the latest. While compelled to detract somewhat from the laudations bestowed upon this learned lady by the Spanish critics and bibliographers, an examination of her work, nevertheless, reveals to us much that justifies us in assigning to her no ordinary degree of merit. She propounds, as her fundamental doctrine, that all life, with all diseases or depravations of life, proceeds from the

* Sir Kenelme Digby: A late Discourse touching the Cure of Wounds by the Powder of Sympathy, p. 9. (1658.)

† Magatus, De rara medicatione vulnerum (1616), lib. ii. cap. 1, 6, 25.

‡ Tratado de todas las enfermedades de los Riñones, Vexiga, y Carnosidades de la verga, y Urina, dividido in tres libros. 4to. Madrid, 1588.

§ Op. cit., p. 352.

|| Bibl. Hisp. Nova, tom. ii. p. 156.

¶ Nueva Filosofia de la Naturaleza del Hombre, no conocida, ni alcanzada de los grandes Filosofos antiguos, la qual mejora la vida, y salud humana. 4to. Madrid, 1728.

brain; the various actions and manifestations being effected through the means of a white juice or fluid, sometimes named by her chyle, and sometimes white blood, which emanates there, and is thence diffused throughout the body. In this idea of a white juice, Martinez, and after him Morejon, affect to discover an anticipation of the later received doctrines regarding a nervous fluid; and indeed, whatever be the value of these, had she limited herself to the statement that by the supposed fluid, which, she remarks, was not detected by the ancients because of its like colour with the nerve, the sensitive and motive power is communicated, and not by irradiation, the claim might have received at least a qualified assent. With how little real justice it is advanced, however, any one who will trace the development of Doña Oliva's doctrines in her treatise* will easily determine. In truth, her philosophy lays at no time any very strict restraint on either her imagination or her credulity; and mere dreams pass with her, not less promptly than with the most fanciful of her cotemporaries, as thoroughly investigated and determined problems. It is one of her therapeutic maxims, connected with her notion of a nervous fluid, that scratching the top of the head with the nails constitutes an admirable remedy, from its power of raising the general pia mater. Yet in many points which she touches, and especially in those which relate to practical ethics, and to moral and political philosophy, she is often clear, solid, and ingenious; and it is impossible to follow her speculations without respect and without interest. With the boldness which belongs to her mission, she does not hesitate sometimes to discuss subjects not usually considered congenial to the feminine character; but the fashion of the day was not fastidious in matters of delicacy, and little more harm was seen then in naming a thing directly than in reaching it by a circumlocution. Upon the whole, Doña Oliva Sabuco does not deserve to be forgotten.

(To be continued.)

REVIEW X.

Urethro-Vaginal, Vesico-Vaginal, and Recto-Vaginal Fistulæ, &c. By N. BOZEMAN, M.D., of New Orleans.—*New Orleans,* 1860.

IN the October number of the year 1858, we gave a notice of Dr. Bozeman's important and interesting observations on the nature and varieties of vesico-vaginal and urethro-vaginal fistulæ, with the results of his experience in the treatment, by surgical operations, of these lamentable injuries.

"Something more than two years have now elapsed," writes Dr. Bozeman, "since my last paper upon the subject of urethro-vaginal and vesico-vaginal fistules appeared. Since that time I have been steadily engaged in prosecuting my labours in this department of surgery;" and the result of his labours is, that 13 cases have been recorded in his pamphlet, all of which have been under treatment

* Op. cit., pp. 228, 267, 345.

in the hands of Dr. Bozeman, in addition to 19 cases previously recorded: making a total of 32 cases.

The results of the treatment we shall presently discuss; in the meantime we have to call the attention of our readers to the introductory remarks in the paper on "*metallic sutures*," and the best mode of using them in the management of these injuries. Dr. Bozeman has no novelty to urge respecting the preference of metallic sutures, in the operative treatment of these injuries, over the use of silk sutures. The profession, or those members of it who have experimented in the field of plastic surgery, have entirely given the verdict in favour of metallic sutures, for almost all operations embraced by the term "*plastic surgery*." Indeed, it is perhaps singular, that the lesson taught us by the wise men of the East of past ages, respecting the non-irritating nature of metallic substances introduced through the flesh, should have been so long overlooked and neglected. Wire, manufactured of gold or of silver, introduced through the lobe of the ear; or of other metals, through the nasal cartilage, was, as respects the former, made an ornament necessary to "the dress" of English damsels; and as regards the latter, was adopted by the careful herdsman, to tether by the nose in security every dangerous or obstreperous bull. In every instance the metallic substance remained in the flesh, not only without producing irritation, but even allowed cicatrization of the parts with which it was in contact, and thus formed a perfect hole through the flesh, covered with skin or membrane.

We have ourselves tested the non-irritating property of silver wire sutures, by leaving portions in the position in which they were first placed, for many weeks after the wound, for which the sutures were introduced, was entirely healed; and we have then found the parts in contact with the suture presenting no indications of irritation, nor the presence of the suture producing the slightest suppuration; in fact, acting no other part, in this respect, than the earrings in the ears of the fair sex.

Dr. Bozeman is, however, desirous to draw attention to the superiority of *silver wire* over *iron wire sutures*. His idea is that iron wire, being subject to more change than silver while in contact with living tissues, is consequently more irritating to those tissues; and that, therefore, wherever the slightest traction is exerted on the approximated edges of a wound, there the iron wire suture has a greater tendency to "cut out" than the silver wire. The remarks on these points are worthy the perusal of all interested in questions relating to operations for the closure of fistulæ. We pass on to the consideration of the other points in the pamphlet.

Dr. Bozeman presents us with the records of the treatment and result of 13 cases. In 10 of the cases reported, the injury was confined to that variety of fistula known as simple vesico-vaginal. Dr. Bozeman operated but *once* on each of 9 cases out of the above 10: 1 of these 9 having been operated on previously three times by another surgeon.

Dr. Bozeman's report satisfies us that he succeeded in closing *per-*

manently the aperture in the vesico-vaginal wall in 6 of the above 9 cases by one operation in each case: incontinence to a troublesome extent persisting in 2 out of these 6 cases.

In one (27) of the 9 cases, the ultimate result was unsuccessful, though union is stated to have for a short time attended the operation, but subsequently gave way from ulceration. In another (28) of the 9 cases, death occurred on the sixth day, the result of pyæmia; but the aperture, as far as could be decided in this short interval, had apparently been closed. In the ninth case (31), incontinence continued after the operation, and there was some doubt how far this depended on the dribbling through the urethra, or upon any small opening remaining in the seat of the original fistula; therefore we may consider the result of this case as uncertain.

The tenth case (20) required to be operated on *twice;* the second operation was followed by permanent closure of the fistula. In 2 cases out of the 13 related by Dr. Bozeman, the injuries consisted of vesico-vaginal fistulæ complicated with urethro-vaginal fistulæ. In one of these cases (21) the injury was the result of the operation of lithotomy through the vesico-vaginal septum; and the apertures were permanently closed by two operations. In the second case (26), Dr. Bozeman's best efforts and perseverance were taxed to the utmost; ten times did he attempt to close the openings without success. Ulceration of the parts followed each attempt, and ultimately the unfortunate case was discharged, hopeless beyond relief.

The last of the 13 cases (25) to be specially noticed, was one in which vesico-vaginal fistula was complicated with a recto-vaginal aperture. Four operations were requisite to entirely and permanently close these openings.

Out of the 13 cases taken altogether, the various forms of fistulæ were permanently closed in 9 cases, by one or more operations. Out of the 13, 1 (27) was unsuccessful from relapse, or ulceration; 1 (28) died; 1 (31) is doubtful as to results; and 1 (26) entirely failed to be relieved after ten operations. This failure, dependent on consecutive attacks of ulceration, we believe to have been quite beyond the control of treatment.

This summary of the 13 cases is highly satisfactory. Can surgery attain greater success? When we recal the fruitless, hopeless efforts witnessed twenty years ago in the operating theatre, to remedy these calamitous injuries, we feel that one of the greatest improvements in surgery has been achieved by the success of Dr. Bozeman's operations. There is one discrepancy in Dr. Bozeman's paper; or perhaps we read him inaccurately; and we are bound to draw his attention to what appears to us an error or an omission—which, no doubt, he will at some future period satisfactorily explain. At page 6, Dr. Bozeman says:

"Several of the cases I am *now* about to report [the italics are ours] are of unusual interest; *one*, perhaps, the most remarkable on record. This was a case of both vesico-vaginal and recto-vaginal fistules, and from its peculiarities occasioned the inauguration of a new and successful plan of treatment never before adopted, that I am aware of, and one to be regarded of the greatest practical utility. The vesical opening was of enormous dimensions, involving nearly the whole of the vesico-vaginal septum, together with half an inch of the root of the urethra, and a large part of the cervix uteri. The recto-vaginal opening, situated about two and a half inches from the anus, was associated with a broad, hard, and unyielding band below it, which prevented any movement of the septum. . . . . This unyielding nature of the posterior septum could not be overcome by any plan of treatment," &c.

Dr. Bozeman then proceeds to state, that the only means of relief was to close the vagina at the vulva; but it was necessary first to cut off the communication with the bowel by closing the recto-vaginal fistula. This was closed by one operation, before any attempt was made to obtrude the vagina. He then adds—"It remained now for me to see what could be realized by the procedure above proposed (obturation of the vagina at the vulva). The mode of performing this, however, *we must defer until the case is introduced in its appropriate place.*" Now we have looked in vain for the report of this interesting case, and the particulars of the operation on the vagina. Case 25 is the only one reported, of vesico-vaginal fistula complicated with a recto-vaginal opening; and of this case Dr. Bozeman writes—"I proceeded to operate upon the vesico-vaginal fistule in the *usual* manner;" and further on he adds, "only the rectal opening *now* remained, and at *another* operation this was closed." This case, therefore, does not agree with the description so partially given of that in which obturation of the vagina was practised; and we must therefore conclude that Dr. Bozeman has accidentally omitted to insert the case referred to at page 6.

In conclusion, Dr. Bozeman generally makes use of the word "disease" when alluding to the various conditions of vesico-vaginal and other fistulæ of the genito-urinary organs. We have so far objected to the application of this word, that in our remarks we have invariably applied to these fistulæ the term "injury." These apertures are, as a rule, the result of injury, dependent on malformation of pelvis, rigidity of external parts; delay, malpractice, ignorance, carelessness, violence even, at the period of childbirth. As an exception, these apertures are the result of disease. Following the rule, we believe they are most favourable to operations for permanent relief. Attending the exception, they are more rarely benefited by surgical interference.

PART SECOND.

Bibliographical Record.

ART. I.—*A History of Infusoria: including the Desmidiaceæ and Diatomaceæ, British and Foreign.* By ANDREW PRITCHARD. Fourth Edition, enlarged and revised by J. T. ARLIDGE, M.B., B.A. Lond.; W. ARCHER, Esq.; J. RALFS, M.R.C.S.E.; and W. C. WILLIAMSON, Esq., F.R.S. Illustrated by Forty Plates.—*London*, 1861. pp. 968.

THIS large work, just published, might well be called a cyclopædia of microscopic organisms, animal and vegetable, so numerous and so varied are the groups of created objects comprised in the ill-defined aggregation of living beings here called "Infusoria." Scientifically, in fact, the term "Infusoria" is so loose and varied in its meaning, that it ought to be rejected. For instance, in the work before us, it is made to comprehend truly vegetable forms, such as the Desmidiaceæ, a congeries of doubtful organisms, here styled Phytozoa; all the lowest phases of animal life, represented by the Protozoa; and, along with these heterogeneous beings, the class of the Rotatoria, which in organization rank alongside the Crustacea. On the contrary, as Dr. Arlidge remarks (p. 199) in his elaborate 'General History of Infusoria,' several authors have used the term in so restricted a sense as to comprehend only the ciliated division of Protozoa.

But before we can reject any term from science, we must be able to replace it by another, or others, more appropriate, and many terms are to be tolerated as provisional. Among those of this class, that of "Infusoria" may be admitted, for the present; or, at least, it may be tolerated in the instance of the work before us, as affording a short and simple title to a collection of essays on the general organization, physiology, and systematic distribution of most of the varieties of organized beings which come within the compass of microscopical investigation.

The publication of so extensive a treatise is a proof how widely microscopic studies are now pursued; and at the same time it will afford an impetus to those studies, since it places in the hands of microscopists a compendium of the present state of knowledge on so large a number of their favourite objects, and one, we must say, without its like in any other language. It professes to be a "fourth edition," but, on comparing it with the previous editions, brought out under Mr. Pritchard's management, we find it so completely revised, re-arranged, and re-written, that it is *bonâ fide* a new work. It is sub-divided into several parts, which have been written by various authors, and each part may be looked on as well nigh an independent treatise. The 'General History of the Infusoria,' by Dr. Arlidge, occupies no less than 484 pages, and presents a most complete *résumé* of the existing state of knowledge respecting each of the interesting groups comprehended under that term. The same writer has also undertaken the systematic history of the Phytozoa and Protozoa—a task which will be valued by observers as furnishing the means for identification of species and for the more accurate study of their affinities and classification. The group "Rotatoria" is treated systematically by Professor Williamson, of Manchester, a naturalist whose name will be a guarantee for the work he has done; whilst the Desmidiaceæ are arranged and described by Mr. Archer, of Dublin; and the Diatomaceæ by Mr. John Ralfs, of Penzance. The last parts are sure of being highly appreciated by microscopists, for the systematic history of the Diatomaceæ presents all the originality of views and accuracy of detail which have rendered Mr. Ralfs's writings on these beings and the Desmidieæ so highly valued by naturalists throughout Europe and America.

ART. II.—*Die Brandstiftungen in Affecten und Geistesstörungen. Ein Beitrag zur Gerichtlichen Medicin.* Von Dr. WILLERS JESSEN. —*Kiel*, 1860. pp. 335.

On the Incendiarism of the Insane, &c. By Dr. W. JESSEN.

THIS volume is of too special a character to call for a lengthened notice in this journal, although it will be of particular interest to students of medical jurisprudence, and to medical men engaged in the treatment of the insane.

The subject of "pyromania"—the insane propensity to destroy by fire—has never, in Dr. Jessen's opinion, received the attention it deserves. He accordingly undertakes its complete examination, and views it in relation to the mental conditions found accompanying it, and to its association with positive mental aberration. To illustrate his subject, he has collected from various quarters a considerable number of instances of this destructive propensity, and has further added to the value of his work by presenting an historical *résumé* of the writings and opinions of other authors who have written upon it. However, as is too frequently the case, German medical writers are little acquainted with what has been produced in other countries beside their own. This defect attaches to Dr. Jessen, who, in his preliminary notice of the literature of his subject, quotes none but German authors; but this is a small matter, and detracts little from the merit of the treatise, which, we may add, is the most complete contribution to the etiology and psychology of pyromania in any language. So far as this country is concerned, we cannot, indeed, point to one satisfactory *brochure* on the subject; all that we have published upon it being but mere *disjecta membra* in Transactions of societies and in journals.

ART. III.—*On the Theory of the Ophthalmoscope.* By GEORGE RAINY, M.D., Assistant-Surgeon to the Glasgow Eye Infirmary. Pamphlet. pp. 66. Eight plates.

IF circumlocution be a weed that all reviewers should unite in eradicating wherever it may be seen to sprout, it is nevertheless somewhat tantalizing to find an author so incommunicative as to leave entirely to the conjecture of his readers the objects he proposed to himself in the publication he offers them. Dr. Rainy is so sparing of words that he does not tell us whether he lays claim to the discovery of new principles in the application of the ophthalmoscope, or new modes of exposition of them, or whether he merely compiles or abridges from German and French writers on the Theory of the Ophthalmoscope. Nor clearly whether he aims at supplying the demands of those mathematically conversant with optics, or to instruct the understandings of novices in this science. It is not that he fails to make such handsome acknowledgments of his obligations to the writers just alluded to, as to proclaim that he has studied their essays, but the greater portion of the book is veiled as to its origin.

In the midst of these enigmas it is so real a gratification to encounter a book in our language on the ophthalmoscope from the pen of a writer with a sufficient acquaintance with optics to be competent for the task, that we frankly welcome it. We will endeavour to judge impartially of its character by comparing it chiefly with the treatise under the same title ('Theorie der Augenspiegel'*), by Helmholtz, the inventor of the instrument, who will be admitted to be a proper person to give an account of it.

We perceive, then, that he shapes his course essentially upon the track observed by his predecessor, though he be rarely found steering to the same point of the compass. As an exposition to satisfy the expectation of geometers, Helmholtz's appears to us not only more complete, but to possess the advantage, dear to such readers, of greater simplicity. And even for a less informed class of readers his, we believe, to be the more intelligible, as a portion of his article is written with especial reference to them. Dr. Rainy interpolates a chain of optical propositions, such as are discussed in every work on the elements of optics, and manifests some skill in compressing them into a small space. Such a summary cannot be of use to those who are familiar with such elements, and we hardly imagine that those who are not so will be able to catch their import to any amount from statements in such embryonic folds. The eyes in his diagrams, he tells us, "are represented as homogeneous bodies, possessed of a single condensing refracting surface, which is regarded as the optical equivalent of the various sur-

* Allgemeine Encyklopædie der Physik, s. 164.

faces in a real eye," after the example, he subjoins, of Stellwag von Carion, in his 'Theorie der Augenspiegel.' It may not be a sufficient reason for not accepting suggestions from this treatise, that Helmholtz affirms of it that the "improvements which Stellwag von Carion has sought to introduce (into the said theory), I cannot acknowledge to be such;" but we may rather doubt the propriety of substituting this fundamental eye—which is plainly founded on the lenticular simplification (schema) for the compound organ proposed by Listing, and elsewhere employed by Helmholtz himself, in formulizing certain optical properties of the human eye—because the idea at the root of this substitution finds here too little development for the higher order of students not previously initiated into it, and does not seem to facilitate the conveyance of instruction to others. Not to object that our author begins his essay by references to diagrams framed for this equivalent eye, without letting us know, until twenty pages after, of this mode of proceeding, and consequently gives some descriptions that dwell for some time on the mind of the reader as inaccurate. Again, any deviations from a standard type, if thought unavoidable, should be carefully detailed. As an instance of the absence of this precaution, we may cite the calculation of the size of the retinal image at page 22, which involves the assumption that the optical centre of the eye lies an inch from the *punctum aureum*, a measurement that exceeds the whole diameter of the human eye; yet the author confines himself to the remark, in a foot note, that "the proportion between the size of the eye and the distances, &c., represented in these diagrams, is of course different from what it would really be in almost any actual case." This does not state that the section of the eye is drawn to exceed the original, much less state how far the optical centre is placed therein from the *punctum*. It is only by actually measuring this distance in the diagram, that we ascertain that it, if not the real eye, justifies his estimate. There are peculiarities of definition, too, that we have no predilection for, as we deem it irksome, and therefore undesirable, to load the brain with novel conceptions when familiar ones might suffice.

Still the essay is decidedly able, and comprehensive enough to embrace every optical principle of interest to the practical ophthalmoscopist, and will repay a careful study; we are not aware that there exists in our language any book on the subject which can at all compete with it in substantial merit.

ART. IV.—*Clinical Lectures.* By ROBERT BENTLEY TODD, M.D., F.R.S., &c. &c. Second Edition. Edited by LIONEL S. BEALE, M.B., F.R.S., &c.—*London,* 1861.

THIS edition forms a portly volume of well nigh a thousand pages, and is a republication of Dr. Todd's various lectures, which were given to the public in 1854, 1857, and 1859; along with an introduction, consisting of the author's valuable remarks on "Clinical Instruction and Hospital Study," and his observations on the "Treatment of Acute

Diseases," which originally appeared as prefaces to his volumes 'On Urinary Diseases,' and 'On Certain Acute Diseases.' But in addition to the above matter, the editor, not willing that any single crumb of his preceptor's teaching should be lost, has wisely appended two chapters, hitherto unpublished, "On the Mode of Taking Cases," and "On the Diagnosis of Disease;" subjoining the dedications which belonged to the original edition.

Of the value of Dr. Todd's series of clinical lectures, here associated under one cover, our opinion was expressed at the time of their issue.* It remains for us only to allude to the two lectures on case-taking and on the diagnosis of disease, which are now submitted to us for the first time. The first of these, containing homely advice, which cannot fail to be acceptable and wholesome to the junior student, bears strongly on the necessity of making clinical investigation the grand "*aim*" and result of all his work. The latter lecture is of much greater general scope and importance, and, like all the author's systematic works, is eminently suggestive and practical: equally with them it exhibits the writer as the energetic, enthusiastic teacher, and the skilful diagnoser, ever resting pathological conclusions on a physiological basis. This lecture, besides other matter, contains a brief notice of several instructive cases, adduced for the purpose of illustrating the importance of diagnosis, and is one of which the careful perusal would prove of peculiar help to those studying disease among hospital out-patients.

Dr. Beale has done the profession good service in superintending this second edition, rendering it the more useful by the list of cases which is found at the beginning, and by the completeness of the index which closes it. We are, however, at a loss to understand why he has disturbed the order in which the lectures were delivered, giving precedence to those which the author wrote last. Surely the peculiar circumstances connected with the modifications in Dr. Todd's mode of practice, so evident in comparing different parts of his lectures, would, as a matter of history, and as a more faithful exponent of the modelling of his opinions, the rather have left the stones just as they were originally laid.

ART. V.—*The Principles of Physiology applied to the Preservation of Health, and to the Improvement of Physical and Mental Education.* By ANDREW COMBE, M.D., &c. Fifteenth Edition. Edited by JAMES COXE, M.D., F.R.C.P.E. 1860.

THIS edition of a work so unassuming and yet so remarkably adapted to the requirements of the public as to have reached the enormous sale of 38,000 copies printed in Edinburgh, whilst it is conjectured that no less than 100,000 copies printed in America have found purchasers, appears to be quite on a level with the present state of our physiological and chemical knowledge so far as it can

* See Review No. 27, p. 93; and No. 39, p. 89.

be applied to hygiene and to popular and social purposes. The additions and necessary corrections apparent in the present volume, supplied by the editor (who, it may be mentioned, also edited the fourteenth edition of 1852), are amply and felicitously introduced, and render the work easy and pleasant reading even to the scientific practitioner, as well as instructive to educators, trainers, and managers of children, and to those whose duties include the general personal superintendence of masses of men. Those also who are entrusted with the disposition and arrangement of human dwellings, factories, schools, hospitals, prisons, barracks, ships, &c., ought to be thoroughly conversant with the principles advocated in it.

ART. VI.—*A Manual of Elementary Chemistry, Theoretical and Practical.* By G. FOWNES, F.R.S. Eighth Edition, revised and corrected by H. BENCE JONES, M.D., F.R.S., and A. W. HOFFMANN, F.R.S.—*London*, 1861. pp. 771.

THIS comprehensive and highly condensed epitome of chemical science is so well known to all who are likely to be readers of this review, and is so deservedly in request by all students of medicine, that any demonstration of its character and composition would here be superfluous. The new edition following so closely on its predecessor is chiefly remarkable for the appendix, in which the editors have given "the substance of Gerhardt's views on chemical notation which are daily gaining ground; and likewise a synoptical classification of the more important groups of chemical substances founded on the new system." The editors regret that the time has not yet arrived for adopting the system in the text of the book. There are one or two observations which we shall make on this volume in an article which we hope to be able ere long to present to our readers on recent chemical text-books, and on existing methods of teaching chemistry.

ART. VII.—*Transactions of the Epidemiological Society.* Vol. I. Part I.

THE literary labours of the Epidemiological Society since its creation, in 1850, have hitherto been known to the profession only through the periodicals of the day, and of late through the pages of the 'Sanitary Review.' Now, for the first time, has the Society ventured a craft of its own, well manned and worthy of every prosperous breeze.

This first part of Vol. I. commences with a short but pertinent preface, bearing the well-known initials of the President, Dr. B. G. Babington; and this is followed by an address in his name and that of the secretary, Dr. McWilliam, whose experience in all matters connected with epidemic disease or sanitary measures is so well known.

In the address which was delivered at the commencement of the session 1859–60, we find mention of all the papers read before the

Society in 1858, and, in addition to other matter, a pithy digest of the reports of the various accounts of yellow fever, diphtheria, cholera, small-pox, &c., which have latterly prevailed in different parts of the globe. Any recapitulation, or even summary of these we are precluded by want of space from giving; but there are one or two facts of interest which strike one on reading them, for which we will find room. For example, we learn that the Australian colonies have hitherto enjoyed immunity from endemic and epidemic diseases, those cases of scarlet fever and measles which have occurred having been imported. In 1859, however, diphtheria broke out in Van Diemen's Land and in New South Wales; and, as the sulubrity of the climate renders the country quite unfavourable to the production of endemic disease or the propagation of infection, it is a matter (as is stated in the address) of the highest moment and interest to study the origin and progress of diphtheria or other epidemic diseases which may arise in those parts. Again, with regard to small-pox, we learn that the comparative immunity of our sailors and soldiers from this disease whilst mixing so freely, as they necessarily do, with the Chinese, from whom, owing to their system of inoculation, it is never absent, is due to the caution of the proper authorities, which, in addition to other instances of great forethought, required re-vaccination to be performed on each of our men who did not show satisfactory proof of infantile vaccination.

The address concludes with allusion to the subject of Quarantine, and to the two papers read before the Society by Dr. Milroy—viz. (1), a preliminary Report from a Committee appointed by the National Association last year at Liverpool; and (2) an Account of the International Quarantine Conference held at Paris in 1851. We are glad to be able to hold out a promise to our readers of an early article in this Review on the subject of Quarantine.

Following the presidential address is a selection, made by an appointed committee, from the papers read at the monthly meetings of the session 1859–60. First according to date, is a speculative communication by Dr. B. W. Richardson "On the Theory of Zymosis"—a theory which (according to the author), if true, extends the verbal list of epidemic disorders, whilst it reduces such disorders to a few unities. Dr. Richardson graphically states the various views that from time to time have been held regarding zymosis or ferment, a process which has been considered as the origin of epidemic diseases, and for the most part looked upon as a vital one. Those who have been in opposition have opposed the zymosis idea altogether, no evidence satisfactory to them having been adduced to indicate the presence of a cell or other mark of organic growth in the poisons of zymotic diseases. Both sides argue from analogy alone, and the difficulties on both sides are, in Dr. Richardson's opinion, explained by the supposition,

"That in the propagation of all communicable diseases, there is a step in the process which consists in the development of an albuminous matter having the power of exciting chemical changes, or zymosis, in the organism; and that there is a second process, consisting of a modification in the animal chemistry—i.e., in the formation of new and inorganic poisons, upon the presence of which the symptoms depend."

Thus, it is assumed, the Cobra secretes an organic poison, and when the victim dies in an hour or less from its bite, one cannot suppose that it is from the multiplication or reproduction of the poison in his body; it rather is because the poison sets up new chemical changes and a series of simple poisonous chemical inorganic compounds. After death the blood of those who have died in this way is found unusually alkaline, and the alkali has been met with in a volatile form. Now, the volatile alkali, if so given as to destroy life, produces the symptoms and post-mortem appearances of poisoning by the Cobra. Again, in the case of yellow fever, the blood after death is said to emit ammonia, and in this disease the symptoms, even the yellowness of the skin, are just such as are produced in animals subject to small doses of the alkalies. In both these cases the poisons introduced from without do not kill by a continuance of the organic force, but by exciting new chemical changes incompatible with the natural chemistry of the body.

The author looks upon the above-mentioned view of zymosis as explaining why directly communicable diseases resemble other diseases not so communicable, and also why so small a portion of virus produces symptoms. It, moreover, indicates that diseases caused by absorption and external poisons are allied to those produced by changes occurring in the body apart from external poisonous agency; thus acute rheumatism is due to the presence of a poison generated in the blood, and really is a genuine zymotic disease, although it is not so considered because it cannot be excited by inoculation in a healthy body. Tetanus, again, belongs to this class of zymotic diseases, according to the author, as being produced by an alkaloïd poison analogous to strychnia, developed in the wound as a result of decomposition.

Dr. Richardson concludes by suggesting that this chemical view of zymosis is of the highest value, as "leading to the direct means by which the zymotic may be studied and experimentally defined," and to the causes, symptoms, and methods of meeting such diseases.

The second article in the 'Transactions' consists of a statement of the committee appointed to collect information, and report upon the recent epidemic of diphtheria. For this purpose the committee issued above two hundred circulars to all the members of the Society, and also to the entire profession by means of the periodicals, offering certain suggestions as to matter and form of the required answers, so as to insure uniformity. Although, however, the profession was appealed to so emphatically, from some cause or other only twenty-two replies, including but thirteen specific reports, were sent in. This information was, of course, too small to serve as a basis of any complete history of the disease. The committee give an interesting and valuable analysis of the reports and correspondence, which we have no space for quoting.

The third article in the 'Transactions' cannot fail, as coming from the pen of such an authority as Dr. Gavin Milroy, to be of the utmost practical value. It is entitled, "Suggestions for Utilizing the Statistics of Disease among the Poor," and when we reflect to what an extent facts in medical science abound, whether public or private practice be considered, and that, except in few instances, comparatively rare attempts at storing them up for inference and deduction are made; we cannot but accord unqualified thanks, on public grounds, to any one who, like Dr. Milroy, comes before us with such a project as the one contained in his paper. In this communication we have a plan drawn up by which a vast range of facts may be brought to light, classified, and utilized in the broadest way for the public benefit.

Dr. Milroy premises that in England and Wales we have no less than a million of poor people in the receipt of pauper relief, and perhaps, in addition, another million who, though receiving pauper relief, are under the care of (about 3000) parochial medical officers. Each case of illness must at present be of necessity duly registered, and Dr. Milroy clearly shows what a quantity of material, unattainable by any other machinery, exists, which might (by giving information as to the varieties and modifications of disease produced by varying circumstances) be rendered available in determining the causation, progress and modes of cure of those diseases which especially abound among the poor, and which are known to be preventable to a great degree by hygienic means. Complete registration of all the numerous cases of sickness and deaths before alluded to, existing, but only meeting the eye of the guardians, Dr. Milroy proceeds to suggest that these should be systematically utilized in the same way as the statistics in the army and navy have of late years been made useful; and that a medical officer should be appointed and attached to the Poor-law Board whose duty it should be to examine the numerous documents furnished by the parochial officers, and embody their facts and conclusions. This medical officer would also be of infinite service to the cause of public hygiene by making known the results arrived at, by helping to guide local boards in their various sanitary plans, and in their regulation of workhouses; by assisting the poor-law inspectors, and by backing up the parochial medical officers in their various recommendations to guardians. In speaking of Dr. Milroy's suggestions, those upon the same subject made by Miss Louisa Twining to the Social Science Association,* September, 1860, and by Miss Nightingale at the Statistical Congress, at once suggest themselves.

The 'Transactions' conclude by a valuable paper on the 'Topography and Diseases of the Gold Coast,' by R. Clarke, Esq., late surgeon to the natives, in which many valuable and curious details are to be found regarding the natural history of the locality, and the manners, modes of life, and diseases of the people.

* See Journal of the Workhouse Visiting Society, No. 10, Nov. 1860, p. 293.

ART. VIII.—*How to Work with the Microscope. A Course of Lectures on the Practical Use of the Instrument and Microscopical Manipulation.* By LIONEL L. BEALE, M.B., F.R.S., &c. Illustrated Edition.—1861. pp. 124.

THIS volume, of the same size as the previous edition which we favourably noticed in a former number,* is so far superior to it that it possesses "numerous explanatory illustrations," which were wanting, and which we before recommended as necessary to give the work that complete practical value of which it was capable. For efficient working in the clinical wards, and out-patient room, and for the proper study of histology, physiological and pathological, this little volume appears all-sufficient as an instructor. The illustrations consist of thirty-two plates and one hundred and fifty figures.

ART. IX.—*On Gout: its History, its Causes, and its Cure.* By W. GAIRDNER, M.D. Fourth Edition.—*London.* pp. 430.

THE former editions of this work are well known to the profession as offering the reliable results of experience, judiciously arranged and presented, by one who has seen many years of practice, in a pleasant, intelligible, and scholarly manner. The new edition places before us fresh matter, and is otherwise recommended as containing such notes of Dr. Braun (the well-known physician at Wiesbaden), who translated Dr. Gairdner's work into German, as set forth his views of the nature of gout. We naturally were anxious to see how far the author fell in with the opinions of Dr. Garrod, as to the part played by uric acid in the causation of gout. The influence of this acid in the blood, as the sole and sufficient cause of gout, advocated by Dr. Garrod, is denied *in toto* by Dr. Gairdner, and as a consequence, the latter repudiates the expression "uric-acid diathesis." With regard to the use of lithia in gout, a remedy highly recommended of late, Dr. Gairdner asserts that as yet he has "not discovered its superiority." It is manifest, however, that, hitherto, too short a time has elapsed since its introduction, and it has been too insufficiently tried by medical men to warrant any positive or final conclusions of one kind or another.

Respective of the comparative value of alkalies and neutral salts in removing the uric acid from the urine, Dr. Gairdner is very strenuous as regards the claims of the latter. He states that he has never seen the uric acid disappear, or even "in any marked degree diminish," under the former; but he has "seen it vanish under the influence of the neutral salts (the phosphates, tartrates, and citrates of potash and soda), when it had resisted all change from the employment of carbonates." Not in any way pretending to give here an analysis of this book, with the former editions of which our readers are so familiar, we cannot close this brief notice without recalling their attention to the valuable sections containing remarks on the too frequent abuse of colchicum (as to the quantities administered); and also to the observations on so-called metastasis and metastatic gout.

* See Review, No. 40, p. 448.

Art. X.—*New Colonial Medical Journals.*

We receive and greet the evidence of increased activity among our professional brethren in far-off parts with no little pleasure, and rejoice at every opportunity of expressing our sympathy with the earnest labours of those who at so great a distance are working hard and honestly in the same cause as ourselves. The first numbers of newly-established medical journals from Madras and from Jamaica have reached us. Of these, the largest, a goodly volume of two hundred and forty-four octavo pages, is the 'Madras Quarterly Journal of Medical Science in all its Branches,' embracing original essays, reviews, selected cases, and medical intelligence. It seems that some years ago Madras possessed its 'Quarterly Medical Journal,' but this for some reason or other only lasted for a period of seven years; since the breaking up of that journal Madras has been left without an oracle, whilst the other two Presidencies have been duly represented in medical journalism by the 'Transactions of the Medical Physical Society of Bombay,' the 'Indian Annals of Medical Science,' and the 'Indian Lancet.' With regard to the future Madras journal, we are informed that arrangements have been made for the publicity of the elaborate Reports which are submitted by commissioned medical officers for the information of the heads of their departments in the Presidency. In the present number, we notice an interesting Historico-medical Narrative of the 1st Madras Fusileers during the year 1857-8, by Dr. Arthur; also one by Mr. Waring, on some of the medical plants of India, in which are some useful instructions to those who may have to study the uses of native herbs. Of the communication by Mr. F. Day, on the Cochin, a notice was given in our last number, page 257. In the miscellaneous department the following cases are related:—1. Amputation of an Osteo-sarcomatous Tumour of the Jaw. 2. Case of Ovariotomy, by a native surgeon. 3. Case of Chylous Urine. 4. Case of Traumatic Tetanus. 5. Removal of Elephantiasis Scroti. Altogether this initiatory number promises well, saving in the matter of the plates, which are ugly inartistic representations of an ugly subject, and we have no doubt that considering to what extent recent events must have multiplied both medical readers and writers throughout India, there is ample room for, and indeed need of, such a journal.

The other new journal alluded to, coming from the remote west, is published under the title of the 'Jamaica Quarterly Journal of Medicine, Science, and Arts.' It is hoped that this production may be less ephemeral than the 'Jamaica Physical Journal,' which closed its career almost twenty-five years ago. It contains the following articles on medicine:—1. On the Tubercular and Anæsthetic Leprosy of Jamaica, by Dr. Fiddes. 2. On the Fevers of Spanish Town, by Dr. Phillippo. 3. On a case of Ascites in which a cure followed spontaneous rupture of the abdominal parietes. 4. On the Phases which Fever sometimes assumes, by Dr. Altman, Coroner. We also find two original articles, on the Detection of Poison in the Human Body, and a short one on the Natural History of Jamaica. Moreover, there is prefixed a Preface by the editor, in which, referring to the difficulty which exists in securing a regular influx of medical men into the island, he alludes to a project which has been started in some quarters for the complete education in the Island of medical practitioners, at the public expense. This is held to be most untenable, but in lieu of this plan the editor suggests that young men in the island might be induced to embrace the profession, if public exhibitions for a period of four years were established, during the first year of which time the exhibitioners might act as dressers, after which they might study the elements of their profession, and then, after competitive examination, be sent to England to finish their education. On their return, they might be expected to reside in the public hospital as assistant-house-surgeons for a year on a moderate salary. How far such a scheme is feasible must no doubt depend upon the political condition of the island, and on the attitude of the local government towards scientific considerations. We shall be glad in the Fatherland to welcome any future Jamaica medical exhibitioners that may find their way hither.

ART XI.—*The Medical Vocabulary; comprising a concise Explanation of the Terms used in Medicine and its accessory Sciences.* By R. FOWLER, M.D., District Medical Officer of the East of London. —*London.* pp. 366.

THAT a handy work, much cheaper than the elaborate 'Expository Lexicon' by Dr. Mayne, and true to the above title, would be serviceable and popular there can be no doubt. We are, however, sorry to say that a review of parts of Dr. Fowler's work has proved disappointing. It contains at once too little and too much, and some portions of what it does contain appear questionable. Searching hap-hazard in different parts for words which (considering that the work professes, "as its chief aim," to supply a nomenclature of natural History and natural Philosophy) might have been expected, the following, among others, were found to be absent: "actinia," "artesian," "chara," "choke-damp," "daguerreotype," "equivalent," "ecchinococci," "oïdium," "pennicilium," "spiculum," "vorticella," "vallisneria." Again, why, in a scientific vocabulary, should such words as the following be inserted: "accent," "accompaniment," "accessory," "application," "abstemious," "actual," "aromatic," "sign," "scaly," "wound," &c., the ordinary untechnical meaning of which is given? They appear to us perfectly out of place. We should like also to see some guide as to the *quantities* of certain syllables which are often very ill-used, such as abdomen, coronary, sarcina, plethora, &c.; and the volume would be more useful if, in reference to certain objects, an explanation was furnished to the novice of the origin of their names, as, for example, "Lieberkuhn," "Woolfian bodies," "Wormian bodies." We think, also, that in certain instances the derivation of names given is questionable, as when "chemosis" is said to be derived from χῦμος, instead of from χήμη.

In any future edition we trust the Greek derivations may be rendered in the original characters, as less embarrassing and much pleasanter to the eye.

PART THIRD.

Original Communications.

ART. I.

Clinical Researches into Morbid Pigmentary Changes in the Complexion. By THOMAS LAYCOCK, M.D., &c., Professor of the Practice of Medicine and of Clinical Medicine, and Lecturer on Medical Psychology and Mental Diseases in the University of Edinburgh.

(Concluded from our last.)

Melasma as a Neurosis.—The symmetrical deposit of pigment, and the occurrence of leucopathia without any known local disease of the skin, both seem to point to an influence exercised by the nervous system on the pigmentation of the skin, independently of the afflux or reflux of blood in the cutaneous capillaries, as in emotional pallor or blushing. We have now to inquire whether such pathological influence can be demonstrated.

It is well-known that certain reptiles (as the chameleon and frog) change colour under the influence of emotions. In an interesting paper communicated to the Royal Society, June, 1857, by Mr. Lister,* it is shown, by experimental proof, that the changes of colour observed in the skin of the frog under varying circumstances, such as would excite activity of the nervous system, are due, in fact, to the influence of the nerves distributed to the skin. The pigment is seen in branched cells, as minute dark granules suspended in a colourless fluid, in which they appear to move freely. When the skin of the animal is very pale, the pigment is accumulated in the centre of the cells; otherwise it is diffused through the cells and their branches or offsets. This accumulation is a morbid process, and occurs at the death of the animal. As the movements of the pigment-granules are evidently under the control of the nervous system, Von Wittich, of Königsberg,† made experimental researches to determine the mode of connexion, and came to the conclusion that in this respect the cutaneous pigmentary system of the frog was circumstanced like the heart or intestines. Mr. Lister, taking up the subject at this point, deduced from his researches "that the cerebro-spinal axis is chiefly, if not exclusively, concerned in regulating the functions of the pigment cells." But the circulation of the blood through the vessels independently of

* On the Cutaneous Pigmentary System of the Frog, &c. &c.
† Müller's Archiv. 1854.

the nervous system also influences the pigmentary changes. Then, again, the calorific rays act directly upon the cells also; thus Professor Goodsir held the side of a living chameleon, during broad daylight, before a dull red fire for a short time, and it became much darker on that side, while elsewhere the skin retained its former pale green colour. So that there are, at least, three sources of pigmentary change: the nervous, vascular, and physical; the vascular includes the direct influence of the blood in the capillaries, the physical those of light and heat, and perhaps other dynamical excitants.

The nervous forms of pigmentary change are of two kinds; those due to centric cerebro-spinal influence (the emotional), and those due to the peripheral influence of the sympathetic without consciousness, as the *melasma* which accompanies pregnancy. I will give illustrations of both these forms drawn from the recorded experience of others.

CASE VII.—*General melasma from terror; anæmia; discoloration permanent.*—A woman was condemned to death by a Parisian mob during the first French revolution, for using some benevolent expressions respecting the king. The "lantern" (the mob instrument of execution) was let down before her at the moment she was menstruating: menstruation immediately ceased. Her execution was deferred, and soon after (in a few days) her skin became as black as that of a moderately black negro. The tint was deeper on the neck and shoulders than on the face; on the face and chest the tint was the same; it was less deep on the abdomen and legs. The limbs were marbled with white spots, which probably indicated the situation of some previous eruption. The joints of the fingers were blacker than other parts; the soles, palms, and folds of skin in the inguinal region paler. She became "languishing" (anæmic), subject to beating in the head, with sense of oppression and general uneasiness. She died in 1819, aged seventy-five, more than thirty years after the shock, the skin remaining dark until death. The post-mortem examination revealed old cardiac lesions, apparently rheumatic.*

Leucopathia will, however, result from depressing emotions. I do not refer so much to the changes which are said to take place in the skin of the negro from mental causes (which have not been carefully observed, I believe) as those more well authenticated instances in which the dark hair of Europeans and others has become suddenly grey. An interesting and authentic example of this kind of leucopathia has been recorded lately.

CASE VIII.—*Sudden whitening of the hair from terror.*—A correspondent of the 'Medical Times and Gazette' having asked for authentic instances of hair becoming grey within the space of one night, Mr. D. P. Parry, Staff-Surgeon at Aldershott, writes the following very remarkable account of a case of which he says he made memoranda shortly after the occurrence: "On February 19th, 1858, the column under General Franks, in the south of Oude, was engaged

* M. Rostan: Nouveau Journal de Médecine. May, 1819.

with a rebel force at the village of Chamda, and several prisoners were taken; one of them, a Sepoy of the Bengal Army, was brought before the authorities for examination, and I being present had an opportunity of watching from the commencement the fact I am about to record. Divested of his uniform, and stripped completely naked, he was surrounded by the soldiers, and then first apparently became alive to the dangers of his position; he trembled violently, intense horror and despair were depicted in his countenance, and although he answered the questions addressed to him, he seemed almost stupified with fear; while actually under observation, within the space of half-an-hour, his hair became grey on every portion of his head, it having been when first seen by us the glossy jet black of the Bengalee, aged about twenty-four. The attention of the bystanders was first attracted by the sergeant, whose prisoner he was, exclaiming 'He is turning grey,' and I with several other persons watched its progress. Gradually but decidedly the change went on, and a uniform greyish colour was completed within the period above named."

Many attempts have been made to explain this curious phenomenon; the most common hypothesis is that of Vauquelin, that a bleaching acid is poured out, which bleaches the hair. But this is not tenable; not only has no acid been found, but no known acid is competent to produce the effect. It is far more probable that the *living* hair, like the pigment-cell of the frog's skin, is so constituted that the pigment-granules can move and be concentrated under certain vital conditions. These of course will cease with the removal of the hair from the body, and the microscopic and other phenomena will be thereby modified. That the pigment-cells of the human skin are influenced by changes in the capillaries and nervous system, such as influence the pigmentary system of the skin of the frog, is probable from various facts, some of which will be shortly adduced. I will only mention here the observation made by M. Billard in the case of cutaneous *cyanopathia* recorded by him. A young girl had her forehead, face, neck, chest, and abdomen discolored of various shades of *blue:* but the intensity of tint increased or diminished according as the integumentary circulation was excited or retarded; for example, when questions were put to her calculated to excite emotional feelings, she blushed blue instead of red. The changes in tint were as sudden as those of the chameleon.*

The action of the cutaneous capillaries, independently of the nervous system, influences the colour of the frog's skin, according to Mr. Lister's researches; but the extent to which they may be influenced by the nervous system, and those changes in the latter be reflected in the colour of the skin, is not fully understood. That this influence will extend beyond the face and neck in emotional states could be shown by numerous facts. The following is an interesting illustration. I had engaged an artist in Edinburgh (Mr. N. Stewart) to delineate accurately from the life the colour, form, and distribution of certain vascu-

* "Lorsque, par exemple, on addressait à la malade des questions propres à l'émouvoir, sa figure bleuissait au lieu de rougir. C'était absolument l'apparition subite des nuances du caméléon."—Arch. Gén. de Méd., tom. xxvi. p. 455.

lar nævi and small varicose vessels scattered in groups over the thorax and epigastric region of a patient. The process was somewhat tedious, and the patient dosed a little from time to time. This somnolency interfered, however, with the accuracy of the drawing as to colour, for Mr. Stewart found that, if he roused the patient from his doze, the colour of the nævi and varicose vessels became brighter; while, on the other hand, as he became dozy again, the tints became duller. This observation is in accordance with many analogous facts that might be stated; it sufficiently demonstrates the close connexion between the cerebro-spinal axis and the vascular system of the skin, and the mode in which it may modify cutaneous excretion and coloration, and the nutrition and colour of the cutaneous appendages through the capillaries.

There are more chronic forms of change in colours of this kind in which the connexion between the nervous system and pigmentary change is not so obvious, but it is not less certain than in the preceding. Of this class are those dependent upon the excessive use or decline in functional activity of the nervous system.

The coloured hairs on the body of Europeans become white with age. In old negroes the hair not only becomes grizzled, but the black tint of the skin changes to a pale or dirty yellow. The negro is most perfect in colour when at the prime of life—i.e., about the age of thirty. In Europeans greyness of the hair of the head is one of the marks of approaching age, but the hoary head is not necessarily coincident with a decline in the powers of the nervous system, and of the body in general. It is sometimes the sequel of anxiety or of a fever, or other exhausting illness. "Too much care will turn a young man grey." In these instances the change is due probably to the same class of changes in the nervous system as the sudden whitening under intense emotion, but taking place very slowly. And there is a diathetic form of *canities* (as in Case II.), in which the hair remaining thick upon the head becomes very white even at a very early age. This is by no means uncommon in persons constitutionally predisposed to gouty affections, and when it occurs in connexion with other characteristics, constitutes one of the best diagnostic marks of the gouty diathesis. In persons of this kind with dark complexions, it is not uncommon to find one lock prematurely grey; I know one or two instances in which this, together with a predisposition to gouty affections or rheumatic gout, is hereditary.

Although age and grey hairs usually go together, there are exceptions to the rule, so that old persons are occasionally met with who have not any. Further, sometimes the grey hair of the aged becomes dark again. A weaver (William Strachan) died lately in Aberdeen at the advanced age of ninety-two. About three years before his death, his hair, which was quite grey, changed its colour, and assumed a darkish hue. But at the same time his vision improved so much, that he renounced the use of spectacles, being able to read the smallest type by the naked eye with the greatest ease. He had always been a very healthy man. Similar cases are on record as to the development of teeth, and the recurrence of true menstruation in old age.

Utero-ovarian Melasma.—In these neurose forms of morbid pigmentary changes, the extent of the change will be determined according as one or other portion of the nervous system is predisposed to be modified in function, or as the blood or bloodvessels are predisposed to take on morbid action. Now, this kind of modification takes place in pregnancy, or in cases of uterine or ovarian irritation, complicated with cachectic states. The complex action of these causes is illustrated by the following case.

Case IX.—*Neurose and cachectic melasma of face, axillæ, and abdomen, complicated with pregnancy; chloasma of the thorax.*—Fleming, a married woman, aged twenty-seven, admitted into the clinical ward in May, 1858. She is of olive complexion, with dark grey eyes; was delivered of a child seven weeks ago: had her leg amputated six years ago, and has now a chronic abscess in the axilla in which her crutch rests; has a blowing systolic murmur, loudest at the base; palpitation on any exertion; pulse 76, of fair strength. The appetite is impaired; vomits occasionally; has pain on pressure in epigastric region; conjunctivæ slightly icteric; complains of pain in the right hypochondrium. On the infra-mammary regions of the thorax scattered liver-coloured patches of pityriasis are observed, sporules of a microsporon being shown under the microscope. On the surface of the abdomen, and especially towards the groins, there is a general dark tint; the right axilla very dark, said to be so from childhood; areolæ of the nipples unusually dark. On the face a dark yellow patch is seen at the root of the nose, extending laterally on the cheeks, and more particularly to the left side of the nose. The upper lip is somewhat discoloured; small warts on the left cheek and lip are darkly tinted. The face and abdomen first became discoloured during her last pregnancy, but have become deeper in tint since the abscesses formed in the axilla. Her health improved much under the use of the tincture of sesquichloride of iron.

If we consider this case with reference to the etiology, we may distinguish three forms of melasma. First, there is the cachectic form, due to the same cause as the abscesses, and which must be considered as a *causa vera*, without which the local or cutaneous irritants would not have induced morbid pigmentation. 2. That pigment-deposit, which is the characteristic of pityriasis versicolor, and is known as chloasma or "liver-spots," from their colour. Here the locality of deposit is determined by the local irritation of the parasitic fungus, the *microsporon furfur*. Its usual habitat is the thorax, back and front, and I may add it is most commonly seen on the skin of rheumatic subjects. 3. The melasma of the face and abdomen which occurs so frequently during pregnancy. Here the pigment deposit is determined probably according to the same law which leads to pigmentation of the mammary areolæ in pregnancy, and to the excitation of the functions of the surrounding glands and of the mammary gland itself. Although so common in pregnancy, it occurs also in ovarian disease. In a case of multilocular dropsy of the ovary, of some years' standing, in a woman aged thirty-eight, admitted into the clinical ward, there was well-marked melasma of the face and abdomen. It will sometimes occur in cases apparently of functional disorder of the reproductive organs. Hence I would term this variety of neurose pigmentation *utero-ovarian melasma.* This form may be characterized by a very intense blackness of the face and abdomen; nor is the colour limited to these surfaces. Le Cat refers to a case in which the left leg became black during each pregnancy.* In other cases the mammæ became discoloured, and assumed a yellow or black colour. A young girl, referred to by Le Cat, felt an itching of the mammæ; she took a laxative, but her mammæ became of a yellow colour. The mammæ of the Samoyed women are black; and Dr. Latham, who notices the fact, thinks it may be due to a peculiar mode of sexual excitation. "Nuptâ virgine, pro primitiis mammæ a marito sugebantur. Multi de nigritudine mammarum apud Samoyedas scripsere historici. Olim credidi aut gravidas aut fusciores visas fuisse. Quid si hæc mammarum stupratio causæ nigritudinis fuerit?"†

These utero-ovarian forms must certainly be classed amongst the neurose, so far as the *exciting* cause is concerned, but they may be considered to differ as the action of the viscera is pathological or physiological. Thus the melasma of the abdomen, which occurs in diseases of the uterus or ovaria, may be classed with that seen to occur in diseases of the peritoneum and abdominal viscera, already noticed, and in which there is probably an incident-excitor action of the diseased structures on the spinal cord, and thence on the skin. On the other hand, the influence exercised by the uterus and ovaria through the cerebro-spinal centres upon the cutaneous structures of the face, neck, axillæ, and thorax (the mammæ) is indirect and physiological.‡

Masculine Neurose Melasma.—The cutaneous structures of the groins, pubes, labia, and scrotum are in the same category as the preceding, but they are equally under the influence of the sexual organs in each sex respectively. This is shown by the development of hair on these portions of the surface at puberty, and by the deposit of pigment to a greater or less extent. In the coloured races this latter is very remarkable as to the mammary areola, penis, and scrotum; so that their surfaces are not unusually very dark in persons who have a taint of negro blood, hardly otherwise appreciable. It is, therefore, a normal tendency which is exaggerated in cases of melasma. This sexual causation explains why children so rarely present the practitioner with examples of melasma. In 36 examples of Addison's disease recorded by others, or observed by me, none was under twelve years of age: 1 boy was twelve, 1 boy thirteen, 1 girl fourteen years of age; of the remaining 32 cases, 21 were men and 11 women, and of these 18 were under forty, and none above sixty. Further, these facts in the clinical history of the disease point out a connexion between the nervous system and the supra-renal capsules, either direct or indirect, through the sexual organs.

* Op. cit., p. 142. † Descriptive Ethnology, vol. i. p. 268.

‡ I have endeavoured to explain the mechanism of this in my lately published work, Mind and Brain, vol. ii. p. 412.

But there is another sexual difference in the development of the hair which leads to morbid manifestations of pigment-deposit. The trunk of the human female as well as the face is free from hair; this is true of all races, with the one alleged exception of the females of a very hairy race of mankind—the Ainös—inhabitants of the Kurile Islands. On the other hand, European men, especially of the Celtic race, are often very hairy; in such, tufts of hair may be seen on the shoulders, over the scapulæ, around the mammæ, and umbilicus, &c. Now, these are some of the localities of the symmetrical deposit of pigment, as in Case II. How far the back and shoulders may be the seat of melasma in women can only be determined by more careful and specific observations; but I do not remember a case in which I have noticed it; and upon asking Professor Simpson as to his experience upon this point, I find it coincides with mine. In a case of cutaneous cyanopathia, already referred to,* it is stated that the anterior surface of the trunk was alone coloured—a statement which at first led me to suspect it was an example of hysterical simulation, until, upon further inquiry, it seemed probable that it is the anterior surface which is also specially affected in utero-ovarian melasma. We may therefore conclude that there is a masculine pathological tendency to melasma of particular portions of the skin as well as a feminine. I believe, too, that it is as rare to find the *symmetrical* forms on the trunk of women, as it is to find melasma of the eyelids in man. And so as to the glandular forms; while *acne* (a disease of the sebaceous glands) is the most common of diseases in young men, I have found no recorded case nor seen one of stearrhœa nigricans except in women, and generally young women. Again, the melasma in which the epithelium covers the pigmented cells, as in the coloured races, is less commonly seen as a *uniform* change of facial colour in women than in men. From all these considerations it follows that, in observing clinically the deposit of pigment, we must note it in relation with the general physiological laws which guide both the development of hair and the deposit of pigment. These laws have reference to race, to sex, and to age, in so far as age bears on the development and functional activity of the reproductive organs. In relation to sanguification, and the healthy or morbid action of other organs, the influence of age on pigment-deposit is quite another thing.

Neurose Blepharal Melasma.—Amongst the sexual forms of neurose pigmentary change, the most common is discoloration of the eyelids. It is so much more easily observed than that of the axillæ, pudenda, or other portions of the clothed surface, that it merits especial notice. It has been variously termed blepharal nigrities, blepharo-melæna, stearrhœa nigricans (Dr. Neligan). We have to distinguish two forms; one in which there is simply a pigment-deposit in the epidermic scales, or a simple form of discoloration, like the ordinary swarthiness, and another in which there is a deposit of free pigment on the skin, so that it can be wiped off. This latter is the true *stearrhœa nigricans*. The two kinds differ so much clinically, that I shall distinguish the former as blepharal melasma.

* Archives Générales de Médecine, tom. xxvi. p. 455.

Discoloration of the eyelids (especially of the lower lid) is well known to be of very common occurrence during menstruation; it is not, however, necessarily due to pigment deposit, for upon careful examination, in many cases (and even in young male patients), it will be found to be a sort of venous lividity. This lividity differs, however, so much in different persons, that I am inclined to think in some there is pigment in the blood, although not deposited in the epidermic cells, for after menstruation has ceased, the colour seems to pass away. In others, however, it only became less deep or yellower; and careful examination shows that there is a faint melasma in these cases. In well-marked forms, the hue is so deep as to leave no doubt of its true character.

Permanent or chronic blepharal melasma is seen in women with chlorosis or melancholia; in women of a certain age who have never had children, but without any important change in the health; in those of a rheumatic diathesis; and in women who have had children, and in whom it is the permanent sequel of pregnancy. It is also permanent as a sequel of some pre-existing disease from which the patient has long recovered, as in Case VII. In ordinary instances of this kind it is rarely limited to the eyelids, but extends over the forehead as a brown stain, and in patches upon the cheeks. It is, however, sometimes a normal condition. An instance came under my notice of a woman, aged forty-four, with well-marked blepharal melasma of both lids, uterine disorder and leukæmia, who informed me (and the statement was corroborated by her sister) that they were six sisters in family, and that they all had dark eyelids as a family characteristic. Five had very dark hair, one was fair, with eyelids not so dark as the other. In all these the tint varied, but was especially darker during the menstrual period.

The most interesting forms of blepharal melasma are those in which it is associated or coincides with emotional or functional disorders of the nervous system. It is not, however, strictly speaking, a neurose form in these, because uterine and hæmic disorder are equally prominent. The following case illustrates its connexion with a deeply-depressed condition of the nervous system, and probably with melanæmia.

CASE X.—*Profound melancholia; anæmia; leucocytosis; amenorrhœa; yellow bronzing; symmetrical melasma of eyelids and face; melanæmia.*—Bartleman, a domestic female servant, aged twenty-four, admitted into Ward 10 (Edinburgh Infirmary) during the summer clinic of 1858. She was profoundly melancholic; rarely spoke; her countenance expressive of the greatest mental distress. The body was meagre, the lips and mucous surface very pale, the complexion of that peculiar sallow hue termed waxy (yellow bronzing). There was symmetrical melasma on the face, but the eyelids, both upper and lower, were in particular deeply tinted with pigment. Menstruation had been suspended for several months; no hepatic or splenic enlargement detected. The blood was carefully examined in this case on three separate occasions, when brown masses like particles of hæmatine were observed. The coloured corpuscles were normal in colour, but crenated easily; the white were greatly in excess. She was removed to an asylum.

The tint in cases of melancholia in young female patients is sometimes *chlorotic*, the yellow hue verging upon green. The popular theory is that in young women with chlorosis, the tint and the corporeal condition with which it is associated are due to emotional influences of an amatory yet depressing character. Thus Shakspeare observes of one of his female characters who "never told her love"—

> "She pined in thought,
> And with a green and yellow melancholy
> She sat, like patience on a monument
> Smiling at grief."—*Twelfth Night*, act ii. scene 4.

In this he doubtless expressed the pathology of the time, for chlorosis was formerly known as the *fièvre de Vénus*. And when we come to discuss the etiology of this class of pigment changes, we shall find that there is really a connexion between them and the uterine functions. But the complexion in melancholia varies much, and doubtless according to its causes. In some, in which it is peculiarly of centric origin, there is little change of colour; in others the tint is icteric; in the most typical forms the face is as if washed over by sooty water, or very delicate Indian ink: chlorosis is, however, often really a form of scorbutus, dependent upon the bad hygiene of girls' schools. And when melancholia depends upon a depraved condition of the blood from imperfect nutrition, as is too often the case with the poor, there is a chlorotic tint of the complexion. But this morbid state may be undoubtedly induced by long-continued strain on the nervous system coinciding with want of sleep; or by a fever or epidemic disease of a depressing kind. And it seems probable that it may even be caused by pigment-deposit in the capillaries or nerve-cells of the brain. I shall not now discuss, however, its connexion with melanæmia, and the theory of causation which refers it to morbid states of the spleen. I would only remark here that blepharal melasma coinciding with melancholia in women, should direct our attention to the condition of the uterus and ovaries.

Transitory, acute or sub-acute, blepharal melasma, is often associated in women with other neuroses, especially those of the hysterical class, and is not uncommonly complicated with stearrhœa nigricans. It is also induced by emotional causes acting on the nervous system of women. I take the following illustrations from Le Cat.

CASE XI.—*Neurose blepharal melasma from fright, extending to the face and arms; at first yellow, ending in desquamation.*—In October, 1749, the daughter of M. Yeury, aged sixteen, met a man in the dark, who insulted and greatly terrified her. In the morning the lower eyelids were observed to be yellow (yellow bronzing); the colour gradually extended over the face for eight consecutive days until it was covered.

Then the yellow tint gradually deepened into black (swarthy bronzing.) Eight days after the date when her face became yellow, the bends of the arms became yellow also, and thence the colour extended over the forearm, and this also became black. She took a quantity of drugs, and many prayers were said for her in the "Quartier," as the change was believed to be due to supernatural causes, but all without effect.

In about four months after, the young lady, when touching her face, found scales coming off it, and to her great joy, she observed that the skin below was of the natural colour. She worked diligently ("de grand cœur") at peeling off her Ethiopian skin, and succeeded in a few days in regaining her natural hue. The arms were the first to be restored.*

The combined influence of the state of the blood and nervous system in pregnancy, conjoined with a highly nervous temperament and the depressing emotion of grief in inducing a strongly-marked deposit if not excretion of black pigment, is shown in the following case:—

Case XII.—*Hysteria, grief; melasma of the forehead, eyelids, and face; hyperæsthesia of the affected surfaces; recurrent during successive pregnancies.*—In 1761 a Parisian lady of high rank (a duchess) had had four children (daughters). She had much anxiety and grief, two of her children dying while her husband was at the wars, when she lost a third. From excessive weeping her eyelids became livid, and then discoloured, as if painted black. The colour also extended over the cheeks in patches. She had hardly recovered from this state when she became pregnant. She had, however, neither violent convulsions nor vomiting, as in her previous pregnancies, and went on well until the seventh month, when her remaining child fell ill. Her forehead then began to appear of a reddish brown, and became finally black, the colour extending to the eyelids, and thence over the face in patches, until the whole face was black. The tint varied, being sometimes deeper, sometimes paler. The skin was very tender to the touch. Her hair was naturally dark, but the roots of the hair on the forehead were darker than elsewhere. She rouged to hide the discoloration. At her confinement (of a son), she had a profuse perspiration, and black colouring matter oozed from the skin, blackening the handkerchiefs and leaving the skin white.

At her next pregnancy, at the seventh month, the melasma took place again, without any obvious cause, appearing as black points. She could not bear the surface to be touched, and again rouged to hide the discoloration. In the eighth month she was attacked by a double tertian, and the pigment gradually disappeared, but convulsive attacks came on, and continued daily until she was delivered of a daughter.

During a third pregnancy the same symptoms appeared. Dreadful headaches at the end of the fifth month, and at the seventh, facial melasma, which was ushered in by feverish paroxysms and pulsations in the head. The melasma commenced on the eyelids, was not so deep in tint as on previous occasions, and ended in about three weeks in desquamation.

* Le Cat, op. cit., p. 173.

Blepharal Melasma in Men.—This form is so commonly met with in women, as to be thought peculiar to the sex; this, however, is an error. It may be observed occasionally in men. It usually commences on the inner margin of the lower eyelid, and extends like a daub of Indian ink from thence to the middle of the lid. I have observed it most commonly in men with dusky complexions, of enfeebled health, and nervously disposed. It is not easy to determine how far it is in relation with the genito-urinary system; but I think it was so in the three or four cases I have noticed. That the lower eyelid in men is influenced by that system, as well as in women, I have no doubt; in young men who indulge greatly in sexual excesses, it may be sometimes seen as dark and swollen as in a menstruating woman. And considerable observation has led me to connect that bladdery lower eyelid seen in men past middle age, with disorder of the prostate or some other portion of the genito-urinary system.

Stearrhœal, or Free Facial Pigments.—The free excretion of pigment from the eyelids (the other form of blepharal melasma) has been observed in women exclusively. The unctuous character in some cases of the black stuff excreted on the face, and its apparent origin from the sebaceous glands, have led various observers to consider it as essentially a flux of sebaceous matter, and hence it has been designated *stearrhœa.* There are two clinical forms usually described, the yellow and the swarthy; or, S. flavescens and S. nigricans. A third form—the *cærulean,* or blue—ought, however, to be added. The *stearrhœa flavescens* is most commonly seen upon the cheeks and nose. There are various diseases of the face and scalp in which an exudation of a yellow matter, drying into yellow crusts, is the chief characteristic. Its resemblance to honey has originated some of its names, as *meliceris, effloratio melita, psydracia mellifera, melitagra,* &c. Alibert has two forms—"melitagra acuta, vel flavescens," and "melitagra chronica, vel nigricans." The latter is evidently nothing more than a species of chronic impetigo occurring in aged and cachectic persons, not peculiar to women, and not limited to the face or neck. It has, therefore, no nosological affinity with the true stearrhœa nigricans, but is simply one of those cachectic affections of the skin in which the deposit of black pigment is a common symptom. The other is identical with the stearrhœa flavescens, and is peculiar to women. The exciting causes are various local irritants, of which the sun's rays seem the most common. Alibert mentions an instance of this kind in which both eyelids and the lobes of the ears were affected as well as the face, in a woman aged thirty, with dark hair and brown but very fine skin. In another woman of the same age, the complaint extended from the face to the arm. The exudation was accompanied with the most intense itching of the part affected. This patient, at the commencement, had an irresistible tendency to sleep, and she drank large quantities of coffee to keep her awake, but without effect. The following shows its connexion with emotional states.

CASE XIII.—*Fright during pregnancy; hysterical susceptibility;*

neurose stearrhœa flavescens.—Justine, a laundress, being pregnant, was very much alarmed by a peal of thunder. After her confinement of a healthy child she did not become regular, and was so irritable that the least contradiction excited her. The upper lip and the surface of the nose became covered with yellow crusts like honey. A little steam easily brought them off, but they soon reappeared, the skin presenting an erysipelatous swelling of a deep red colour.*

Relation of the Yellow and Black Forms of Stearrhœa.—It is probable that the pigment to which the exudation or excretion owes its colour is identical with that to which the yellow "bronzing" and the tint in the xanthous races of mankind are due; and that, consequently, it is simply a modification of the brown or black pigment. In commenting on a case of stearrhœa nigricans, Dr. Neligan expresses a different opinion, to the effect that in the latter the black tint occurs because the sebaceous fluid is "stained with the colouring matter of the blood; just as in the same cases, and as occurred in those above narrated, matters rejected from the stomach are often of the colour of grumous blood, while in other examples we have dark grumous sputa, dark bloody urine, or hæmorrhagic extravasation into the areolar tissue beneath the true skin."† The objection to this doctrine is that the excretion from the sebaceous glands has its own proper colouring matter. I have already shown how true animal pigment differs from that spurious kind which is the result of a transformation of the hæmatin of the blood-corpuscles. Now there is nothing in these recorded cases of S. nigricans to indicate that the pigment is of this class, although they have a pathological relationship through the nervous system as to cause, with the hysterical and other cutaneous hæmorrhages (*hæmophilia*) which simulate purpura. On the contrary, microscopic researches prove that the colour is caused by pigment-granules identical with those contained in the epidermic cells.

Dr. Neligan's own case illustrates very well the close relations of the yellow and black varieties of stearrhœa, and the leading points in their pathology. His patient presented both forms; as is usual, was a young woman of nineteen; had hysteria, amenorrhœa, vicarious hæmoptysis and hæmatemesis (hysterical hæmorrhage); and at each monthly period a condition of a portion of the skin of the trunk designated "erysipelas," but which appears to have been only a cutaneous neurose congestion, continuing for three or four days. The pigment deposit appeared subsequently to this, first as a *bluish*-black stain at the inner canthus of the left eye. The following morning a large black patch was observed by Dr. Quinan under each eye. The conjunctivæ were unaltered in appearance, not being either congested or swollen. The black patches were permanent, but extended somewhat and became deeper in colour at each monthly period. The skin was excessively tender to the touch, so much so that she would not allow any local application to be tried. In December, 1854, Dr. Neligan saw the case with Dr. Quinan. At this time vomiting recurred every

* Alibert: Monograph des Dermatoses, tom ii. p. 117.

† Dublin Quarterly Journal of Medical Science, vol. xix. p. 301. May, 1855.

morning; her cough exceedingly troublesome; the girl much emaciated, *except in the face*, highly hysterical, nervous, and excitable. The dark patches extended now over nearly the whole of the right upper eyelid and partly over the left, and on the right side reached the cheek. The colour was precisely that of Indian ink. On examining the patches with a powerful lens, it was seen that the tint was not uniform in depth, but was dotted over the surface of the skin, the dark dots corresponding to the orifices of the sebaceous glands. No attempt was made to remove the pigment, as the surface of the skin was so exquisitely tender that she could not bear the most gentle pressure of the finger.

In April, 1855, the cough was better, and the vomiting of dark matter had ceased to a great degree, but the general health of the patient was much more impaired. The discoloration was deeper in tint, and had extended very much over the face, now engaging both cheeks to below the malar bones, and also the alæ of the nose.

"On the forehead and around the black stain on the cheeks, there was observed an exudation of *yellow* matter, precisely the same in appearance as is witnessed in *stearrhœa flavescens*. Wherever this was rubbed off or the parts below irritated, the integument was seen to be in an inflamed state, the sebaceous follicles hypertrophied, and their orifices enlarged; in short, in precisely an analogous condition to what occurs in that disease, except that many of the enlarged orifices were filled up with the black matter which was the cause of the black discoloration on other portions of the face."*

This case presents several points of interest—namely, the progressive development of the pigmentation concurrently with continued uterine disorder and hysterical symptoms; the hyperæsthesia of the affected surfaces; the coincidence of the two forms of coloured stearrhœa; and the deposit of fat beneath the skin of the face when there was no deposit elsewhere—a peculiarity not unfrequently manifested in aggravated hysteria, and giving the facial expression a very deceptive character of health.

Utero-ovarian Stearrhœa Nigricans.—The case just detailed illustrates very well one of the neurose forms of the swarthy stearrhœa, and in this respect may be classed with Case XII. But just as is the case in the yellow, so in the swarthy it happens that the excretion may take place without any considerable derangement of the health. An instance of this kind is recorded in the 'Phil. Transactions for 1709,' by Mr. Yonge, of Plymouth. A young girl, aged sixteen, who had never menstruated, but healthy, although thin, had a few "hot pimples" on her cheeks, which "bleeding and a purge or two" cured. She continued very well until about a month after, when her face suddenly turned black, like that of a negro. The terror the change excited, and the curiosity, exorcisms, and prayers of which she was the subject, under the belief that she was bewitched, rendered her very hysterical. If washed off, the black matter reappeared from two to five or six times in the twenty-four hours. There was a little warm

* Dublin Quarterly Journal of Medical Science, vol. xix. p. 301.

flushing of the skin when it appeared, but no pain nor sickness. It felt unctuous to the touch, had no taste, and coloured the cloth used to wipe it off. Mr. E. Wilson, in his work 'On Diseases of the Skin,' refers to a case under Dr. Macintyre, of a similar kind, in which the pigment could be removed by washing, and reappeared in twelve hours. The same phenomena, with the same absence of important symptoms, have been also observed in blue stearrhœa. In the great proportion of the recorded cases, however, the skin was so painfully sensitive, that the patient would hardly permit it to be washed off. Thus the skin was so sensitive in Mr. Teevan's patient (a young lady with the blepharal form), that she was not only reluctant to attempt to wash away the pigment, but every fresh exudation was accompanied with a burning sensation and a pricking, and local applications induced on two occasions an erysipelas of the face.*

The connexion between these stearrhœal forms of pigment-excretion, and the disorders of the nervous system thus indicated, is also obvious from other particulars in the cases already quoted, and in others recorded.† Thus we find emotions of any kind are active excitants. The most common connexion of the discolorations in the way of causation is, however, with the functions of the uterus and ovaria. They seldom, if ever, occur in men or in old women, but in women either at the age when these functions are being established, or during pregnancy, or during states of health coinciding with disorder of menstruation.

It has been alleged that these cases of S. nigricans are cases of simulation, and that, in fact, the disease is not real; but this is a conclusion as to all cases by no means warranted by the facts when a number of cases are compared. At the same time, there can be no doubt the disease *may* be simulated by hysterical patients afflicted with the monomaniacal cunning not uncommon in young girls who have a desire to excite sympathy or wonder. Anything of the kind occurring in old women or in men would require great caution in this respect.‡

Blue Discoloration of the Eyelids (*stearrhœa cœrulea*).—Cases of pigmentation are on record in which all the other symptoms being almost the same, a blue pigment took the place of the yellow or black. This state has been termed by M. Billard of Angers *cyanopathia cutanea*, or cutaneous blue disease, to distinguish it from cyanosis, in which the blueness of the surface is nothing more than a lividity, due to the circulation of venous blood, in consequence of open foramen ovale. I have already referred to the case of cyanopathia recorded by M. Billard. In all essential points it corresponded with the recorded neurose cases of S. nigricans and S. flavescens, with which it may be therefore classed as *S. cœrulea.*

* Transactions of the Medico-Chirurgical Society of London, vol. xxviii.

† See original cases detailed by Dr. Leroy de Méricourt, in his Mémoire sur la Coloration partielle en noir ou en bleu de la Peau chez les Femmes: Arch. Gén. de Médecine, tom. c. p. 430.

‡ Professor Simpson has mentioned to me a case of simulation of blepharal melasma which came under his notice in a gentlewoman nearly sixty years old. Circumstances rendered it probable that morbid vanity was the cause of the artificial blackening.

This somewhat rare form of cutaneous discoloration is most interesting, from the fact that it has clearly a pathological relation with the excretion of carbon in the form of urinary pigments, and perhaps a physiological relation with the brighter colours of birds and other animals of brilliant tints. It is probable, indeed, that the pathological production of these pigments generally is connected with a function of the kidneys not hitherto investigated in its physiological or pathological relations—namely, that of eliminating carbon, the nitrogenous elements of the urine having attracted attention almost exclusively. The following history is an interesting illustration of this kind of case:—

Case XIV.—*Stearrhœa cœrulea; amenorrhœa; hæmatemesis; anœmia; splenic disease* (?)—A woman, aged thirty-three, who had not menstruated for fourteen years, and was previously very irregular in this respect, but with hæmatemesis, from time to time, had violent palpitation and difficulty of breathing, for which she had been bled at least sixty times. About a year and a half previously to coming under Dr. Penn's notice, she had a sharp "splenalgia," and later an attack of a tertian intermittent, with almost daily vomiting of blood. During the subsequent six months a blueness of the eyelids had been observed, which began with blue spots on the lower lid of both eyes, extending downwards on the cheek and upwards to the upper eyelids until it surrounded the orbits. It could be wiped off with a moist cloth, but reappeared next day, was of a tint brighter than indigo, but varied in intensity at periodic intervals. Examined under the microscope, the removed colouring matter was found to be an indigo-blue pigment, contained for the most part in epidermic cells, but existing also as free grains, lying singly or in masses. It was unchanged by acetic and nitric acids, caustic alkalies, and ether, but dissolved in concentrated sulphuric acid without previous change in colour. The urine was tested for cyanurin without success.*

Dr. Büchner has related a similar case as to the blue discoloration, the subject of which was a pregnant woman. A blue deposit has been observed in other cases, in which that of a black pigment might have been expected. A disease termed *carate*, of a diathetic character, is endemic in New Granada and the northern parts of South America, attacking chiefly the negroes, mulattos, and Indian half-breeds. It is characterized by various changes in colour, as copper colour, dull white, crimson red, or *dark blue.* It appears to be a cutaneous disease allied to syphilis, or to be a parasitic disorder, which affects the colour cells, and (to use the popular phraseology) blackens the whites and whitens the blacks. Alibert had the opportunity of seeing a case in the person of a surgeon, from the borders of the "Madeleine," whose face, chest, and limbs were dotted over with yellow, red, and blue spots, giving the skin a peculiarly marbled appearance. They began with a slight itching. Dr. G. Van

* Dr. Penn: Nederland. Week-blatt for 1855; and Canstatt's Jahresbericht for 1855, Band iii. p. 383.

Arcken has also lately described the disease.* The blue is the mildest form, attacking persons aged from fifteen to twenty-five years, and consisting in the appearance of round or oval blue spots on the face. The spots coalesce and extend down the neck or the chest where the ribs are often so distinctly marked as to give the patient the appearance of being striped like a zebra. The hands and lower portion of the tibia are commonly affected, and sometimes the glans penis. The white variety (leucopathia) is almost peculiar to women aged from thirty to forty, with uterine or ovarian disease.

Chemical composition of pigments, and mode of production.—It is not necessary to multiply examples of this kind, the preceding illustrations being sufficient to indicate the relation in common of these pathological pigments. Numerous researches have been lately made into their chemical composition, especially since cases of the *blue urine* were investigated by Heller. The black, yellow, red, purple, and blue pigments of the urine have, in especial, been examined as urerythrine, uroxanthine, urrhodine, purpurine, uroglaucine, cyanurine, indican, indigo blue, and indigo red.† The black pigments and melanotic deposits have also been analysed, and found to vary much in the proportion of carbon, the pulmonary black pigment having the largest amount. This may, however, be due to the fact, that coal and charcoal dust will and does accumulate in the lungs, and it is not improbable, even in the bronchial glands. Schmidt and Melsens found it in two analyses to be composed as follows :—

	C.	H.	N.	O.
Schmidt's analyses	66·77	7·33?	8·29	17·61
	72·95	4·75	3·89	18·41
Melsens' analyses‡	70·80	3·33	3·	?
	86·6	0·83	—	—

Schwerer ridiculed Melsens' second analysis by saying that it showed diamonds might be formed in the organism, but Virchow adopted the jest, and held that such a notion was not so improbable, as Sir D. Brewster and others had already given reasons for the opinion that the diamond was a vegetable product.§

As compared with hæmatin, the black pathological pigment has more carbon and much less hydrogen. Mulder's formula for hæmatin is 44 C, 44 H, 6 N, 6 O. It has also more carbon than the pigmentum nigrum of the eye, which, according to Schwerer, is 58·0 C, 13·7 N. The same chemist makes cholepyrrhin to have more carbon than the pigmentum nigrum, the formula of its composition being 68·19 C, 7·47 H, 7·07 N, 17 26 O. The hæmaphæin of the urine belongs to the same group.

All these pigments are, therefore, eminently carbonaceous, containing from sixty to seventy per cent. of carbon, and all seem to have the same

* American Medical Monthly Journal, April, 1858; and British and Foreign Medico-Chirurgical Review, July, 1858, p. 261.

† A good historical account is given by Dr. Thudichum, in his excellent Treatise on the Pathology of the Urine. 1858.

‡ Comptes Rendus, 1844, tom. xix. p. 1292. § Archiv für Physiol., Band i. p. 445.

origin; namely, in transformations or metamorphoses of the tissues and the blood-corpuscles. Hence a whole series of physiological and pathological relations of these pigments to the blood-corpuscles, to each other, to carbon-excreting organs, and to carbonaceous foods. Fundamentally, the entire series of phenomena in which pigmentation is a leading characteristic may be regarded as having reference to the excretion of carbon, after it has served its purposes in the economy, whether in animal or vegetable organisms. Thus the colours of plants may be considered as the products of physiological processes in which carbon is freely formed. The most highly-evolved activity of plants appears to be during inflorescence and the development and fertilization of the ovum; and therewith there is the most rapid production of carbon as colouring matter. The influence of the sun's rays in stimulating vital activity generally, and that of those portions of the organism more particularly exposed to them, is shown by both the luxuriance of life and the brilliance of colour witnessed within the tropics. Moleschott has well illustrated the influence of light in the production of carbon in plants.*

The pathological production of pigments may be looked at from three points of view: First, as the result of imperfect oxidation of carbon, so that it is not eliminated as carbonic acid, lactic acid, hæmaphein, &c.; secondly, as the result of imperfect elimination of carbon proper, when that is the normal excretion, as in the hair and epidermic scales; and thirdly, as the result of the excessive production of carbon, from highly carbonaceous foods. In all these there is a close analogy between the carbonaceous excreta as morbid pigments, and the nitrogenous excreta, as morbid deposits of urates, &c. As to the first, it is obvious that all modifications in the blood-corpuscles which impair their function of oxygen-carriers, will tend to imperfect oxidation of the carbon-waste. In this way we can understand how carbon may be substituted for carbonic acid and lactic acid, in cases of leukæmia, leucocytosis, the anæmia of chlorosis, Bright's disease, and all cachectic states in which the blood-corpuscles are defective in oxygenating power. In like manner we can perceive why pigment-deposits occur in certain states of the lungs in which the oxygenation of the blood is impaired, and there is at the same time no vicarious elimination of carbon as hæmaphein, purpurine, &c., by the kidneys. This is particularly the case in old age, in asthenia, and the like.

As to the forms of imperfect elimination, these are numerous. Defective action of the lungs, so that the production and elimination of carbonic acid is impaired, is a common cause; so, also, defective elimination of the colouring matter of bile, or of urine, arising from morbid states of the liver and kidneys. In this way we can understand how supra-renal, renal, and hepàtic diseases may be manifested by morbid pigment-deposits rather than by pallor, or an icteric tint and bilious blood, or by uræmia. It seems highly probable that the hair is a medium of excretion in both man and the lower animals, for morbid pigment-deposits are not uncommonly associated with cessation of pig-

* Comptes Rendus, &c., vol. xli. p. 363. 1855.

mentation in the hair. Thus large masses of melanotic matter have been found with such frequency in the bodies of grey horses, as to lead to the conclusion that there is the relation of cause and effect between the defective colour in the hair and the melanotic deposit. It seems probable, however, with reference to the latter (which is not an excretion), that the local deposit of carbon is due to a defect in the eremacausis of the tissues themselves under such conditions of both the blood and tissues as favour the production of pure carbon instead of its oxides. This conclusion follows from the well ascertained fact in melanosis, that the carbon-deposit is like that of the urates in gout; that is to say, is not a morbid product of the nature of cancer, as was at one time supposed, but non-malignant, and taking place, under various conditions of local mal-nutrition, when other general conditions coincide.

The deposit of pigment in connexion with morbid states of the genito-urinary organs is somewhat complex as to causation. It is probable that the menstrual fluid, so highly carbonaceous as it is, serves other purposes than those usually assigned to it, and that, in fact, the discharge is, in part at least, a carbon-excretion. Be this as it may, recent researches have shown that less carbonic acid is eliminated by the lungs during the menstrual period. Probably the pregnant state predisposes to pigment-deposit because of the condition of the blood, which is one of excess of white corpuscles (leucocytosis); and this is perhaps the case also in utero-ovarian chlorosis. It is to be remembered, however, that the reproductive organs are in intimate relation with the kidneys through the nervous system, and that in many cases of hysteria, especially in girls predisposed diathetically or hereditarily to gouty diseases, the urinary excretion is much modified. Hence a great variety of *renal* cases of hysteria with their consecutive urinary phenomena. To this head may be referred probably the cases of supra-renal melasma, for we have seen that the pigment-deposit has relations with the sexual hair and cutaneous glands; and it is a fair inference at least, that the diseased capsules may, in certain cases, influence both the kidneys and the ovaria and testes through the nervous system of the sacro-lumbar region.

Pigment Deposit in Gouty and Rheumatic Cases.—That there is a connexion between gout and rheumatism and these discolorations has been already fully shown. Assuming, in explanation of this, that the connexion between the excessive production of lactic acid and the phenomena of rheumatic fever has been established as a general fact, we can take it as a starting-point, although much remains to be done by way of special elucidation. According to some chemists,* the carbon of flesh-eremacausis appears in the juices of flesh as lactic acid, and this, after being further oxidized, is excreted as carbonic acid and water. Thus one atom of lactic acid and twelve atoms of oxygen may produce six atoms of carbonic acid and five of water. The ease and rapidity with which it is oxidized in the organism is well known. After considerable quantities of alkaline lactates have been injected into the

* Golding Bird on Urinary Deposits.

stomach of animals, or directly into the blood, the urine is found to be strongly alkaline from the carbonates of the alkalies introduced. The vital burning up of lactid acid into carbonaceous matter is analogous, according to these views, to the ordinary combustion of ligneous fibre. And from this theory we can deduce our explanation. If the supply of oxygen to the system be not sufficient for the transformation of lactic acid into carbonic acid and water (as in emphysema, for example), and it undergoes no further changes, then it appears in the urine; but if the carbon of the flesh be not oxidized into lactic acid, then it is excreted as colouring matter in some form or other.

All these bio-chemical theories are notoriously unstable, but the facts of the subjoined case are elucidated by the preceding.

CASE XV.—*Melasma of the face; recurrent attacks of rheumatic fever: melanæmia in the intervals of, but not during, the attacks.*—Lister, a woman, aged twenty-two, affected with chronic rheumatic arthritis, with recurrent acute attacks and cardiac disease of rheumatic origin, was admitted into clinical ward XI. in the summer of 1858. She had pigment deposit in the face, apparently associated with the *microsporon furfur*—a rather rare circumstance, as this parasitic fungus is almost invariably on the trunk. On repeated examinations pigment was found in the blood; but during the progress of an acute attack of rheumatism the quantity diminished, and eventually no pigment could be found. When convalescence was established, however, it again reappeared in the blood. The patient was dismissed when convalescent, and was readmitted after a few weeks labouring under another acute attack of rheumatism. The tint of the face had become deeper during the interval, but no pigment was found in the blood until convalescence was again established, when it reappeared as before.

In a girl, aged seventeen, named Hay, admitted into the same ward about the same time with acute bursal rheumatism, pigment was not present during the acute stage, but was found during convalescence. In seven cases of cardiac disease of rheumatic origin, or of chronic rheumatism, pigment-deposit on the skin was also observed, together with more or less pigment in the blood. (See also Case VII., in which neurose melasma was associated with chronic cardiac disease).*

Albinism and leucopathia have, however, been found to be associated with rheumatism as well as melasma. A young albino gentleman, aged seventeen, the son of a rheumatic parent, who has a sister like himself as to colour defect, was lately under my care for an affection of the brain, the sequel of rheumatic fever, of which he had had

* When in Paris, two or three years ago, I demonstrated blood-pigment to M. Aran at the Hôpital St. Antoine, in the presence of his clinical clerks, and some medical visitors. After examining the blood of several women with melasma, affected with various forms of utero-ovarian disease, we examined our own. The only one of the six or eight physicians present in whom blood-pigment was found was an Italian physician who had had rheumatic fever, and had a damaged heart. His face had the dusky pallor so peculiar to this class of cases. Morbid pigmentation is indeed a very common symptom in chronic rheumatic cases.

repeated attacks. The case of an Indian is recorded* whose skin, when he was aged sixty, began to change colour immediately after an attack of rheumatic fever, and at last became almost universally white, with all the sensibility of an albino skin. More recently a case has been published of a negro slave,† aged forty-five, who, born of black parents, was black until twelve years old, when he lost the sense of smell, and a portion of the skin encircling the cranium just within the edge of the hair changed to white, together with the hair of the part. Then a white spot appeared near the inner canthus of the left eye, from whence the leucopathia extended, until the negro became quite white. He was subject to chronic rheumatism, and had second attacks of hooping-cough, measles, and scarlatina after the change occurred.

These remarks apply to black pigment exclusively. But the other pigments are produced much more constantly—in the urine at least—in rheumatic fever and in inflammations of a rheumatic type or seat. Thus purpurine or urerythrine, which contains sixty to sixty-five per cent. of carbon, is abundant in the urine, in rheumatic fever, and rheumatic pericarditis, nephritis, pneumonia, pleurisy, and peritonitis.‡ The presence of indigo-blue, indican cyanurine, or uroglaucine, as demonstrated pathologically by Heller and physiologically by Dr. Schunk, indicates the source of the blue, red, and green discolorations of the skin. Further researches are required in regard to these, but the facts hitherto ascertained as to them clearly point to the operation of the same general law which rules the production of other pigments. Thus Dr. Schunk found indican abundantly in the urine of a publican while he sat idly at home drinking his own beer; but (as Dr. Schunk informed me) when he went out to work vigorously for a day in the harvest-field, it wholly disappeared from his urine.§ It is a good illustration of the doctrine which Dr. Schunk lays down, "that the occurrence of the indigo-producing body, as an excretion, is probably due to a disproportion between the oxygen absorbed by the system and the matter to be acted on by it." In short, we may say that the colouring or carbonaceous matters of the urine follow the law of transformations of the nitrogenous (for urea becomes uric acid when imperfectly oxidized), and that the two have analogous, albeit not similar relations to the tissues.

The relations of pigmentary discolorations to free black pigment in the blood (melanæmia), and to biliary pigments, and to the vicarious excretion of black and yellow pigments by the pulmonary and intestinal mucous surfaces, remain to be examined. To carry out the inquiry satisfactorily, a preliminary investigation of the functions of the spleen and liver, in reference to the blood-corpuscles and the using-up of carbon, would be needed, for which this is not the fitting occasion, if it were practicable in the present condition of physiological science.

* London Medical and Physical Journal, vol. xl. p. 391.

† American Journal of the Medical Sciences, Jan. 1852.

‡ Dr. Thudichum: Pathology of the Urine, p. 324.

§ Compare Dr. Schunk's paper in Memoirs of the Literary and Philosophical Society of Manchester, vol. xiv. p. 239 et seq.

It may be useful to remark here, however, that melasma and dinginess of tint are not due to the same conditions. The latter is caused by the tint of the blood, and is analogous in this respect to the yellowness of icterus; the former is due to an excretory and not a mere mechanical deposit from melanæmic blood. This may be a cause, however, of melasma, in so far as it is usually deficient in red corpuscles, and may even have an excess of white; or, in other words, there may be leukæmia with free pigment in the blood.*

ART. II.

On Sexual Limitation in Hereditary Disease. By WILLIAM SEDGWICK.

THE hereditary transmission of disease is a subject for inquiry so vast and so little understood that any fresh facts likely to throw light on only one of its subdivisions, cannot fail to be of some use, and to win perhaps for the whole subject a greater consideration than it has yet received; for so little progress appears to have been made in this field of science, that Mr. Darwin, one of our latest and most distinguished writers on the subject, states that "the laws governing inheritance are quite unknown." The following cases of sexual limitation in hereditary disease, which have occurred in my own practice, together with some of a similar kind recorded by others, may furnish material for further work; and as a seed-time must always precede the harvest, they may on some future occasion enable others to reap the matured results of what, at present, can be regarded as little more than an imperfect collection of facts.

With the exception of slight and passing remarks by a few writers on the hereditary transmission of disease,† and the occasional record of detached cases, hitherto no notice has been taken of sexual limitation in hereditary disease. Notwithstanding, however, this neglect, there is abundant evidence to prove that the occurrence of the restriction is neither infrequent nor capricious; but that, on the contrary, it is in some diseases so constant and so regular, whilst in others, in which it is only occasionally observed, it is so evidently the result of a strong controlling influence, that it may be safely referred to some law of their development, which, absolute in the one case, has to struggle as it were to express itself in the other; seeing that the well-defined limits of the restriction, and the circumstances attending its occurrence, point to something far more definite than what is vaguely called accidental coincidence, or the result of chance, for chance can account for nothing. The attempt, indeed, to explain natural pheno-

* See for a discussion of this question the recently published volume of the Sydenham Society, Frerichs on Diseases of the Liver, p. 314—"The Pigment-Liver."

† Petit, Dictionnaire des Sciences Méd., art., "Hérédité," p. 59. 1817. Histoire Générale et particulière des Anomalies de l'Organization chez l'Homme et les Animaux, par M. Isidore Geoffroy Saint-Hilaire, tom. iii. p. 378. Paris, 1832. Dr. J. Webster: Medico-Chirurgical Transactions, vol. xxxii. p. 118. 1849. Traité Philosophique et Physiologique de l'Hérédité Naturelle dans les états de Santé et de Maladie du Système Nerveux, &c., par le Dr. Prosper Lucas, tom. ii. pp. 165–6, 835–6, &c. 1860.

mena by the doctrine of chance must always be unsatisfactory because unreasonable; and therefore it appears better to ascribe the occurrence of this phenomenon to the influence of some natural though as yet unrecognised law of development, than to sink to a theory so inconsistent with reason as supposes that this or any other event in nature could occur without an efficient and a well-ordered cause. If, therefore, as we are justified in assuming, the development of all disease is the necessary result of natural laws, we may be quite sure that in diseases so strictly hereditary as those to which there will be occasion to refer, the undisturbed action of the law in their case must harmonize with order in the result.

The question which may be raised as to why a limitation so strictly observed should occur in connexion with some diseases and not with others, is a subject beyond our ability to discuss. It is referred to now simply for the purpose of stating that there is a difference in the sexual limitation of disease, which need not however be thought strange, for the absence of such a difference would be a far greater cause for surprise, seeing that there are corresponding limitations connected with other morbid phenomena, which are more fully recognised, but which have not yet been explained; such as why certain diseases should occur only once during life, and why some diseases should be hereditary and others not so. The non-recurrence of some diseases and the hereditariness of others, have ever been ranked among the mysteries of nature; and beside them may be placed the sexual limitation of disease, depending, like them, on causes which we may not strive to know.

It is not perhaps to be expected that we should be able to appreciate as yet the full extent of this inquiry, and its importance in connexion with social life; but in addition to any passing remarks which may occur in the course of this communication, it may be as well to suggest that there are two practical applications of it, which all will be ready to acknowledge—viz., its moral importance in connexion with marriage, and its commercial importance in connexion with the insurance of life; and in reference to the latter it may be said that some sound information, which can apparently be obtained from this source, on the probable duration of health and life, seems to be much needed by our life insurance companies.

The first case to which I will direct attention, is one of ichthyosis in a boy, aged fourteen years, belonging to a family in which the disease has been hereditary for three generations. It first occurred in the grandfather, who is still living, and who has the disease in a very severe form: it did not appear in him, or it was not at least noticed, till he was about seven or eight years old. This man has had three sons and three daughters. One son died at the age of five years, and one at the age of seven years, both of whom were free from the disease. The other son is living, and past middle age, but has shown no tendency to the disease. The three daughters have all lived to grow up and marry, and in them likewise the skin is unaffected. Two only of the three daughters have had children. The eldest daughter has had four, of whom the firstborn, a girl, has had no appearance of the disease ; the three other children are boys, of whom the eldest, aged fourteen years and the youngest, aged nine years, suffer from the disease, whilst the other son, aged eleven years, is free from it. The family of the other daughter consists of three children, the eldest of whom, aged six years, is, as in the former case, a girl, and free from the disease, whilst the two other children, who are boys, aged respectively three years and one year, have the skin very decidedly affected. It is to be noted that the disease in these grandchildren has in each case appeared within a few months after birth. There are two other important facts to which I would call attention before passing on ; one is, the non-development of the disease in the second generation, and its re-appearance in the third, which is an instance of *atavism,* for which no satisfactory reason can be assigned ; the other is, the sexual limitation of the disease, which in this case is complete in a double sense ; for whilst the females in the family have alone transmitted the disease, its appearance has been restricted to the males.

Dr. Elliotson* relates a case of ichthyosis occurring in his practice, in which two brothers presented the disease out of a family of three boys and one girl.

In the well-known Lambert or porcupine family,† the disease was hereditary for four successive generations, and was strictly limited to the male sex. Two brothers, John and Richard Lambert, lineal descendants from the celebrated porcupine man who was exhibited at one of the meetings of the Royal Society in 1731, visited Germany in 1802, when M. Tilesius of Leipsic published an account of their case. In the following year, M. Alibert had the opportunity of seeing them in Paris, and he has published some amusing details of an attempt made to gull the Parisians by some marvellous additions to the family history. The important feature in the Lambert case is that the males alone for four generations suffered from the disease, and that the two brothers referred to had seven sisters who were free from it.

Although in all of the foregoing cases the appearance of the disease has been limited to the male sex, it is not to be supposed that there is any peculiar inaptitude in the fair sex to this unsightly affection of the skin, for at one of the meetings of the Medico-Chirurgical Society in 1818 (vol. ix. pp. 52–3), the case of Mrs. Holden was brought forward, in which the disease was limited to the female sex, in the person of her daughter, Jane Holden, aged three years, in whom the disease had appeared when about three months old, the same age as that at which it had occurred in her mother ; and this case is the more remarkable from the fact that whilst the child inherited the disease of her mother, she inherited the features of her father.

Ichthyosis of a less severe form than the above appears to be of rather frequent occurrence in some continental countries. That form of it which the French call *Pellagre,* is stated by Alibert‡ to be often

* London Medical Gazette, vol. vii. p. 633. 1831.

† Philosophical Transactions for 1731, p. 299 ; and for 1755, p. 21. Mémoire sur les Frères Lambert, in Mémoires de la Société des Sciences de Strasbourg, tom. i. p. 327. 1811.

‡ Dictionnaire des Sciences Méd., tom. xxiii. p. 380.

hereditary, and that the disposition to it is transmitted from generation to generation among the peasants of Lombardy. The fact of the hereditary transmission of Pellagre is fully confirmed by Docteurs Ghiotti and Longhi, who state that "in 184 families containing individuals suffering from hereditary Pellagre, and numbering in all 1319 members, 671 individuals were healthy, and 648 suffered from this disease."* And to show that the disease is hereditary, and therefore not contagious, M. Buniva† inoculated himself with some discharge from the affected skin, with the patient's saliva, and with his blood, without being affected by it; and this experiment has been repeated on the lower animals.

The question respecting the hereditary nature of these and other allied affections of the skin, is at the present time exciting the deepest attention in Norway, as well as elsewhere; and the result of further researches will probably add to the importance of sexual limitation in the development and transmission of hereditary diseases of the skin. The proposed inquiries of M. Virchow on the subject of leprosy will probably be of much value; and notwithstanding the great confusion which has resulted from including several distinct diseases of the skin under the name of leprosy, there is no doubt that hereditariness is one of the characteristic features of the disease, for ichthyosis was formerly known only by this name, and I need not do more than allude to the leprosy of Gehazi (2 Kings, v. 27), which became hereditary in his seed for ever.

From diseases affecting the texture of the skin, we may proceed to those which affect its colour, such as occurs in albinoes; and as one of the most peculiar features in their case is the eyes, which suffer from congenital deficiency or absence of pigment, I avail myself of this opportunity for stating that I shall allude to their case only in connexion with the organ of vision; for the number of cases and the variety of diseases in which sexual limitation can be shown to prevail, so far exceeds the proper limits of my paper, that it is necessary to restrict the selection of cases: with this object in view, I shall select for consideration the diseases affecting two of the organs of sense—namely, sight and hearing; and refer to other cases only so far as may seem useful to illustrate any subsequent remarks I may have occasion to make on this subject; and I do this the more willingly as it offers me the opportunity of showing that if we take and examine in good faith any one class of diseases, or diseases affecting any one organ of the body, rather than pick out isolated cases from the whole range of pathology, that we may hope to form a better and a truer estimate of this phenomenon than we could otherwise expect to do.

The rose-coloured eyes of albinoes have been observed to be often hereditarily connected with one sex. The two well-known albinoes of Chamouni, referred to by M. de Saussure, in his 'Voyage dans les Alpes,' were brothers, whose sisters presented no unusual appearance. So also in Dr. Trail's case of three albino brothers in a family of six children,‡ the first and fifth children were girls, having the usual ap-

* Traité des Dégénérescences, &c., par B. A. Morel, note at p. 571. Paris, 1857.
† Alibert: Maladies de la Peau, tom. ii. p. 207. 1822.
‡ Nicholson's Journal, vol. xix. 180.

pearance; the second and sixth children were boys and albinoes; the third and fourth children were twins, and both were boys, but only one of them was an albino. A curious fact connected with this case is, that the mother had a cousin with "very fair skin, flaxen hair, and very weak light-blue eyes," showing a near approach to albinoism. Mr. Th. Jefferson, in his 'Notes on the State of Virginia,' relates the case of two albino sisters, who gave birth, the one to an infant who was an albino like herself, the other to a very black infant like the father.

The comparative rarity of albinoes in cold or temperate climates is probably the cause of so few cases of hereditary albinoism having been observed amongst us, for it is chiefly in tropical climates that this anomaly in development occurs. At the conquest of Mexico by the Spaniards, not only were there albino dependents in the palace of Montezuma, but many examples of albinoism were met with among the birds and other rare animals in the palace-gardens;* and albinoes are so common in Ceylon, that Buffon† speaks of them, though erroneously, as forming a distinct race in the interior of that island. Voltaire,‡ in like manner, with other writers, thought that nations of albinoes dwelt in the interior of Africa, which Livingstone's researches show not to be the case, and which is further opposed by the fact that the blacks are in the habit of killing them whenever an opportunity for doing so occurs. But although hereditary albinoism is of somewhat rare occurrence in Europe, there are some well-marked examples of it, from which I shall further select two cases illustrating my subject, related by M. Cornaz in the 'Annales d'Oculistiques' for 1850.

The first case is remarkable for the remoteness of the relationship between the albinoes, as well as for the sexual restriction of the anomaly. The two grandsons, Daniel and Baptiste Gillieron, of a family residing at the village of Ferlens, near Oppens (Switzerland), married, and had each of them two daughters, one affected with albinoism, and one exempt. These two albinoes, who are the great-granddaughters of the family referred to, and from whom the inheritance is derived, married: the one, who became by marriage Madame Devaux, has had no children; the other, who became the wife of an agriculturist named Pache, with black hair and brown eyes, is the mother of an infant who is an imperfect albino.

The second case, which is of a still more conclusive character, is that of a young Swiss girl, named Josephine Chassot, whose parentage unites her case with that of seven other albinoes. It appears that the great-grandfather of Josephine had two children, a son and a daughter. Josephine is the only one of the descendants of the first (the son), affected with albinoism. Among the descendants of the second (the daughter), who became by marriage Madame Rey, there have been seven albinoes. This Madame Rey had two sons, named James and Charles Rey, who married two sisters. The eldest, James Rey, had, among other children, three albinoes—namely, one son and two

* Dictionnaire de Médecine, tom. ii. p. 121.
† Traité de l'Homme, et l'Histoire Naturelle (le Supplément 4).
‡ Mélanges Philosophiques, chap. 18, et Essai sur les Mœurs, chap. 1.

daughters. Charles Rey had seven sons, five of whom married, but only one of these five had albino children, and he had four albino daughters out of a family of three sons and nine daughters. It is to be noticed in this case that whilst the three albino children of James Rey were on the same level in descent as Josephine Chapot, belonging, like her, to the fourth generation, the albino descendants of the old man's grandson, Charles Rey, did not appear till the fifth generation, owing, it would seem, to the fact that Charles Rey had seven sons, but no daughters; the defect being transmitted by one of those seven sons to four female children, who were the great-great-granddaughters of the common ancestor. The exceptional instance of one male albino in this family is not of sufficient importance to impair the value of the case which I have selected to illustrate my subject; for the remoteness of the descent, coupled with the fact that no albino intervened between the head of the family and the albino descendants of the fourth and fifth generations, is sufficient to outweigh such an objection.

From the pink eyes of albinoes we may pass to a case in which, for three generations, the eyes have resembled in miniature the markings on the back of a tortoiseshell cat, and in which the transmission of the defect has been restricted exclusively to the female sex. The following history of the case is published in the 'Dublin Medical Journal' for 1835, by Dr. Osborne, late President of the Royal College of Physicians in Ireland: John Murphy, aged fifty-two years, a native of County Wexford, had fifteen brothers and five sisters, all of whom possessed the family peculiarity of tortoiseshell-coloured eyes. The inheritance was derived from the mother, whose maiden name was Murray. She had three sisters and one brother, who were all similarly affected, and who inherited the peculiarity from their mother, whose maiden name was F——. It is to this latter family that the peculiarity belongs, insomuch that in the part of the county where they resided, they have been commonly recognised by this distinction, and celebrated for communicating it to their posterity. A titled individual well known in Ireland, but whose name Dr. Osborne did not think it prudent to publish, obtained the same peculiarity in consequence of an intermarriage between one of his ancestors and a daughter of the F——'s.

In like manner there may be hereditary differences in the colour of the two irides, as, for instance, where the iris of one eye has been hazel and of the other blue, or where the same iris has been of two colours and spotted. I am acquainted with a case in which the two irides are spotted like a leopard's skin; and Mr. Middlemore* states that these defects have been oftentimes hereditary, so that all or many of the children of the same family shall have had some singular spot upon the iris, different from its natural colour, or the two irides shall not correspond in their colour.

From defect of colour we may revert to defect of structure, as shown in the case of coloboma iridis, or cleft iris, which is the analogue of hare-

* A Treatise on Diseases of the Eye and its Appendages, vol. i. p. 763. 1835.

lip, and respecting which Mr. Streatfield,* in the following account, remarks that so many cases of coloboma iridis have come under his observation that he believes it a very common congenital defect, and one of much more frequent occurrence than the common hare-lip. The case selected is that of a boy who has a moderate-sized cleft of both irides in the common direction downwards. "His eldest brother has a similar defect in both eyes, and his youngest the same in one eye, and the symmetrical indication of the deformity in the other. The mother's father was said to have had the same defect in both eyes, and his brother also in both eyes, and her eldest brother was similarly affected, and his eldest son had the 'long sight' (as it was called in the family). . . . . The cleft iris seems to have belonged to the males of the mother's family. The mother herself had perfectly natural pupils, and four of her children (a boy and three girls) were also unaffected." The chief points of interest in the case are—1st. The transmission of the defect, without its being shared in by the mother. 2ndly. That whilst two of her three sons had the defect, her three daughters were free from it; and lastly, That the maternal grandfather, the maternal granduncle, the maternal uncle, and the son of this last named, all shared in the defect, which shows that the inheritance in this case extended to at least four generations.

Congenital absence of the iris may be also hereditary, as occurred in the case published by Professor Boek of Christiania,† in which the father, son, aunt, and niece had the defect. In the family of Christian Kehl, whose case is published in 'Ammon's Zeitschrift' (vol. i. No. 4), congenital absence of the iris was hereditary for three generations. Christian Kehl himself, and three sons out of a family of eight children, had the defect; but the limitation to the male sex in this case, which was maintained for two generations, failed in the third, when two of the granddaughters presented a deficiency of the iris.

Defects, either congenital or developed subsequent to birth, in the other structures forming the organ of vision, are sometimes hereditary—as congenital opacity of the cornea, which occurred in the case related by Mr. Farar,‡ where three children in succession had been born with this defect, but the sex is not stated. Cataract and amaurosis may be also hereditarily limited to one or the other sex. With reference to the first of these defects, the late Mr. Gibson,§ of Manchester, saw five or six children, the families of *two sisters*, who were all totally blind from cataract. The late Mr. Saunders‖ also observed many cases of cataract in the same family. One was that of two brothers, between whose ages there was a difference of six years, and who were both affected with congenital cataracts. In a second family, two brothers, twins, became blind with cataracts at the age of twenty-one months, each within a few days of the other; and Mr. Saunders adds—"that

* Ophthalmic Hospital Reports, p. 153. 1858.
† Annales d'Oculistiques, tom. xxxiii. pp. 93–4. 1855.
‡ Medical Communications, vol. ii. pp. 463–5. 1790.
§ Edinburgh Medical and Surgical Journal, vol. vii. p. 398. 1811.
‖ A Treatise on some Practical Points relating to the Diseases of the Eye. By the late John Cunningham Saunders, p. 134. 1811.

the four cataracts had precisely the same character." Lastly, Mr. Lawrence states that he had under his care "at St. Bartholomew's, two brothers, between two and four years old, with cataracts."*

With reference to amaurosis Mr. Middlemore† informs us that his practice has furnished him with many examples of hereditary amaurosis, and he gives references to several other writers who have observed the same fact. Mr. Lawrence‡ had under his care at the London Ophthalmic Infirmary, twin sisters, who were both amaurotic at the same time, and with exactly the same symptoms. Demours§ gives a case in which a father and son were both amaurotic; and another case, of a French lady, completely blind from amaurosis, whose daughter became blind from the same disease at the age of twenty-three years. Beer|| relates a case where the females for three generations became completely and incurably amaurotic as soon as menstruation ceased; but they who have borne children escape. The males in this family have exhibited only a tendency to amaurosis, without any of them having become actually blind. And sometimes amaurosis and cataract are hereditarily associated together, as occurred in the case of the Jew of Sienna and his son, related by the Italian translator of Portal's work on hereditary diseases.

In like manner microphthalmia, or congenital smallness of the eyes, which occurred in as many as 21 out of 7001 cases entered at the Eye Institution in Brussels, is sometimes hereditarily limited to one sex. M. Geschreifht¶ informs us that among the above patients were two brothers, microphthalmic on the *left* side, whose father had lost his *left* eye, fifteen years before his marriage, in consequence of purulent ophthalmia, whilst serving in the army of Holland.

Even congenital absence of the eyes themselves may occur as a hereditary defect, and be limited to one sex, as in the case published by Mr. Walker,** where congenital deficiency of both eyes occurred in two children, who were sisters, being the eldest and the youngest of a family of three children, in which the intermediate child was a boy, and free from the defect.

Defect in the muscular apparatus of the eye is sometimes also hereditary, even for many generations, as in the Montmorencies,†† in which illustrious family nearly all the members squint; and Prosper Lucas,‡‡ in referring to several cases of hereditary strabismus, remarks that such cases are too common to need further notice. Mr. Streatfield§§ has lately published an interesting case of the sexual limitation of this defect in a family of ten children, consisting of five boys and five girls, in which only the five boys squint; one of these boys had a twin sister who did not squint. The eldest boy, aged fifteen years, squints with the left eye; the second, aged thirteen years, squints with both eyes; the third, aged nine years, squints with both eyes; the fourth, aged

* A Treatise on Diseases of the Eye, Third Edition, pp. 650–1. 1844.

† Op. cit., vol. ii. p. 253, note. ‡ Op. cit., p. 534.

§ Traité des Maladies des Yeux, tom. i. p. 369, et tom. iii. pp. 92–3, Observation 332.

|| Lehre, vol. ii. p. 442, quoted by Lawrence, p. 534.

¶ Annales d'Oculistiques, tom. xiii. p. 33. 1845. ** Lancet, p. 169. 1831–2.

†† Considérations sur la Nature et le Traitement des Maladies de Familles et des Maladies Héréditaires, &c., par Antoine Portal, troisième édition, p. 1, note. Paris, 1814.

‡‡ Op. cit., tom. i. p. 392. §§ Ophthalmic Hospital Reports, pp. 153–4. 1858.

seven years, squints with the left eye; and the fifth, aged seven months, squints with both eyes. Neither the father nor the mother squint, but the latter has "a sister, who married long after herself, and has two children, a boy, aged seven years, and a girl, aged four years, who both squint with the left eye." In addition to the almost complete sexual limitation in this case, it will be noticed that where only one eye was affected, it was always the left, which tends to confirm the hereditary nature of the defect.

There remain to be considered the so-called functional defects of the organ of vision, which have been arranged under a separate head rather for the sake of convenience than of accuracy, as it is probable that no defect of function can occur hereditarily, without a correspondingly hereditary defect of structure. The first and most serious of these defects to be noticed is that of complete blindness, which may occur as a congenital defect; such was the case in one family of nine children, observed by M. Pauli,* who were all born blind; or this hereditary blindness may not occur till a later period of life, as in the case related by Sir Henry Holland,† where four out of five children became blind about the age of twelve years; and the hereditariness in this case was further confirmed by "the existence of a family monument long prior in date, where a female ancestor is represented with several children around her; the inscription recording that all the number were blind." In the family of Le Compte,‡ thirty-seven children and grandchildren became blind like himself, and the blindness in this case occurred about the age of seventeen or eighteen years, for three successive generations. Lastly, Mr. Wright§ records that "at Dover, some years ago, a lady was frightened by a ferret whilst in a state of pregnancy; the child when born had eyes precisely like that animal; every child after that had the same kind of eyes, and they all became blind, or nearly so, about the age of puberty." But in this class of cases, I am not acquainted with any well-marked examples of hereditary limitation to one sex.

Nyctalopia or day-blindness, and hemeralopia or night-blindness, are sometimes hereditary. All albinoes suffer from nyctalopia; and as regards hemeralopia, M. Cunier‖ relates a case in which this defect prevailed for two centuries, eighty-five members, belonging to six successive generations of the same family, having been hemeralopic; and there are two points of special interest to be noticed in this case; the one is, that among more than six hundred individuals in this family, there has not been a single example of hemeralopia in the children of parents who were themselves free from it. The other is, that the defect has been transmitted more by the women than by the men.

Myopia, or near-sightedness, is sometimes also a hereditary affection limited to one sex. The statistical researches of Dr. Furnari on affections of the eye have clearly demonstrated that the greater number of

* Annales d'Oculistiques, tom. xix. p. 38. 1848.

† Medical Notes and Reflections, Third Edition, p. 33. 1855.

‡ Baltimore Medical and Physical Register, 1809, quoted in the New England Medical and Physical Journal, vol. i. pp. 394–8. 1810–11.

§ Lancet, 1830–1, vol. ii. p. 464.

‖ Annales d'Oculistiques, tom. i. p. 31, 1839; quoted by Lawrence, op. cit., p. 587.

short-sighted persons were the sons or grandsons of short-sighted individuals. In the single family of Dr. L'Heritier, the short-sightedness was successively transmitted from the grandfather to the father, and from the father to the two sons. In the family of M——, the father became short-sighted at the age of twenty-four years, and two sons were also short-sighted like their father.*

Portal† has published an interesting account of what is termed "momentary blindness," in two brothers, aged fifteen and seventeen years, who, on bending their heads for five or six minutes, lose their sight, and do not regain it for a short interval. Their father had been subject to a like blindness all his life.

Lastly, in that curious defect of vision, known as colour-blindness, we have an instance of a hereditary defect‡ being so strictly limited to one sex, as to form one of the most characteristic features of the complaint. This imperfection of vision, which tends in all its phenomena to show that disease no less than health is under the orderly control of law, is of somewhat frequent occurrence, and has already attracted much notice, not, it would seem, as a well marked example of sexual limitation in disease, after the manner described in this paper, but as an instance of what may be called sexual preference in disease. A short reference to some of the many cases of colour-blindness which have been published, will perhaps be sufficient to prove that it is an error to refer the sexual limitation in such cases to a preference in the disease for one sex rather than the other, for even where this appears to be strongest, there is evidence to prove that it is weaker than the influence of sexual limitation, and that where the two are brought as it were in contact with each other, there ceases to be any longer a choice in the matter, for the inheritance of the disease is confined to that sex in which it was first developed, even in those cases where the other sex seems to have the greatest aptitude for it.

Colour-blindness, which is also called Daltonism, after the celebrated author of the atomic theory, who, with his brother, suffered from this inability to distinguish colours,§ affects all classes, from the highest to the lowest, and is far from being uncommon. Mr. Wardrop‖ informs us that "several members of a noble family in this country have been remarkable for having it;" and Miss Sedgwick, in her 'Letters from Abroad,' vol. i. p. 250, states that the historian Sismondi was a Daltonian. The Abbé Rozier, who wrote a paper on the subject in 1779, remarked even then that "it is not perhaps so rare as one thinks."¶ Dr. Dalton states that out of twenty-five pupils he once had, two were found to agree with him in vision; and on another similar occasion, one:

* Piorry: De l'Hérédité dans les Maladies, p. 120; quoted by Prosper Lucas, tom. i. p. 393.

† Op. cit. p. 87.

‡ Prof. Wartmann, of Lausanne, in an elaborate memoir on Daltonism or Colour-Blindness states that it is not always hereditary, and that it does not always date from birth: see Taylor's Scientific Memoirs, vol. iv. p. 186. 1846.

§ Memoirs of the Literary and Philosophical Society of Manchester, vol. v. part 1, pp. 28–45. 1798.

‖ Morbid Anatomy of the Human Eye, vol. ii. p. 199.

¶ Observations sur la Physique, &c., pp. 86–8. 1779.

and it has been shown, that some individuals known to suffer from this defect are unwilling to allow of any notice of their case. In the description of a case of a Quaker shoemaker, named Harris, in the 'Philosophical Transactions' for 1777, pp. 260–5, who, as a child, could not distinguish cherries on a tree, because they were to him of the same colour as the leaves, and two of whose brothers, out of a family of four brothers and one sister, had the same defect of vision, reference is made to the case of a gentleman in good position, who was well known to suffer from colour-blindness, but who refused to satisfy any inquiries on the subject. As no disinclination of this kind has been shown by the members of our profession who have suffered from this defect, either personally or in their families, I shall notice briefly the case of Dr. Whitlock Nicholl, which was brought before the notice of the Medico-Chirurgical Society in 1816, by Mr. (the present Sir Benjamin) Brodie, vol. vii. pp. 476; and the case of Dr. Pliny Earle, published in the 'American Journal of Medical Sciences' for 1845, vol. xxxv. pp. 346–354.

The subject of the first case was a boy, aged eleven years, a relative of Dr. Nicholl's. This boy had four sisters, in none of whom was vision so affected, and one infant brother, of whose vision nothing could, of course, then be known. The peculiarity of sight as to colours was derived from the maternal grandfather, who was similarly affected. This gentleman had two brothers and three sisters; one of the brothers had the same defect of vision, the other brother and the three sisters were free from it. The second generation consisted of only two daughters, the mother and the aunt of the boy referred to, both of whom were free from this defect of vision. The essential facts in this case closely correspond with those related in my case of ichthyosis, the hereditary disease being in each case limited to the males of the first and third generations, and transmitted by the females of the second generation, who were themselves unaffected by it.

Concerning the second case, Dr. Pliny Earle states, "my maternal grandfather and two of his brothers were characterized by it; and among the descendants of the first-mentioned, there are seventeen persons in whom it is found. I have not been able to extend my inquiries among the collateral branches of the family, but have heard of one individual, a female, in one of them, who was similarly affected." The defect in this case prevailed in eight distinct families, of which one was in the second generation, five in the fourth, and two in the fifth generation. In the first group, consisting of one family, only the males were affected; in the second group, out of five families affected, the sexual restriction prevailed in four; whilst in the third group, consisting of two families, only the males had the defect. And although in most of the families in this case, including those in which the defect did not, as well as those in which the defect did prevail, the number of females exceeded that of the males—being in one family, eight females to seven males; in another, seven females to three males; in another, five females to four males; in another, four females to three males; in another, three females to two males (in which family both the males were affected, but none of the three females); there was only one family in which the defect was not strictly limited to the male sex. The total number of persons in the eight families affected was sixty-one, of whom thirty-two were males, and twenty-nine females; of the former, nine-sixteenths were affected, and of the latter only one-fifteenth.

In a case recorded by Dr. Combe,* of Mr. Milne, who suffered from colour-blindness, "the maternal grandfather was affected; . . . . also his two brothers and a second cousin."

Dr. Bronner,† in a paper on this subject, read at the German Medical Society in Paris, relates the case of a learned German chemist who suffered from colour-blindness. This gentleman had two children, who were daughters, and could distinguish colours perfectly well; the eldest daughter had three sons, of whom only the second was colour-blind; the youngest daughter also had three sons, all of whom were colour-blind. Two of the sons in this last family have children; one of them has five—namely, one boy and four girls, who have no imperfection of sight; the other has four children, the sex of each is not stated, but only one of them, a boy, aged seven years, inherits his father's defect.

The tendency to which I have already referred, of hereditary diseases being often limited in their appearance to one sex, and limited in their transmission to the other, is a prominent feature in most of the above cases; and to show further that this double peculiarity, when it does occur, is not accidental, I may cite a case related by M. Cornaz,‡ of colour-blindness in two brothers, born of the same mother, but of different fathers by two successive marriages. By her first marriage with a Frenchman she had three children, of whom only a son had this defect of vision; by her second marriage with a Swiss she had two sons and a daughter, of whom one son had the defect.

It may be noticed, that in all the above cases, the defect of vision has been limited almost exclusively to the male sex; and it is necessary for me to state, that this apparent preference for the one sex rather than for the other occurs in a very large majority of the cases of colour-blindness hitherto observed; for an analysis of upwards of 200 cases shows that the proportion of males affected is nine-tenths of the whole.§ But, as I had occasion to state with reference to the same point in ichthyosis, this apparent preference for the male sex is not due to any peculiar inaptitude in the female sex to the defect; for where it has primarily affected the latter, its sexual limitation is complete, as in the interesting case published by M. Cunier,‖ where the defect occurred in thirteen individuals, belonging to five generations of one family, all of whom were females. Respecting this case M. Cunier informs us, that Madame Th——, aged fifty-eight

* Transactions of Phrenological Society, p. 222.

† Medical Times and Gazette, pp. 359–361. April 12th, 185 .

‡ Annales d'Oculistiques, tom. xxiii. p. 43. 1850.

§ Cyclopædia of Anatomy and Physiology, art. "Vision" (White Cooper), p. 1454.

‖ Annales d'Oculistiques, tom. i. pp. 417–420. 1839.

years, had six children, of whom five were girls and one a boy; this lady and her five daughters could not distinguish colours, the son had no such difficulty. The grandmother of Madame Th—— had the same defect as these female descendants; this old lady had one son and two daughters—the son was free from the defect, the two daughters had it. Both these daughters married; one died in giving birth to Madame Th——, the other had only a son, who was free from the defect. Of Madame Th——'s five daughters, four are married, the eldest has four children, of whom two are daughters unable to distinguish colours; the second has two children, a son and a daughter, of whom the last only has the same defect as the mother; the third daughter of Madame Th—— has only a son, aged eleven years, who can perfectly distinguish colours. The father of this lad, a distinguished painter at Brussels, was for some time afraid that he might be affected with the infirmity of his mother. The fourth daughter of Madame Th—— is not married. The fifth and youngest married a government officer at Surinam, and has only had one child, a boy, who can distinguish even between colours and shades of colour. And to show how completely what are supposed to be the ordinary rules of development in these cases have been on this occasion upset, it may be mentioned as a curious fact in connexion with the above case, that whilst most observers have shown that the malady is more common in men than in women, and in persons with light eyes than in those with dark eyes, both these conditions are reversed in the family of Madame Th——.

Before passing from the consideration of colour-blindness, which in some persons is so great that they can only perceive one colour in the rainbow, I may remark that the subject has already attracted much notice as an occasional source of danger in connexion with our present system of railway and other signals;* and that it will perhaps shortly receive further attention, for among the additional topics announced at a late meeting of "the Sanitary Section of the International Statistical Congress," was one by Sir David Brewster, which we may hope to see carried out, suggesting that returns regarding colour-blindness should be obtained by the section.

(*To be concluded in our next.*)

* Mr. W. H. Tyndal: Athenæum, Jan. 1853. Dr. George Wilson: Association Medical Journal, 1853. Researches on Colour-Blindness, with a Supplement on the Danger attending the present system of Railway and Marine Coloured Signals, by George Wilson, M.D., p. 180. 1855.

Art. III.

Obscure Trunk Pains; or, Chronic Pains in the Abdominal and Thoracic Walls. By James Jago, M.D. Oxon, Physician to the Cornwall Infirmary.

The great multitude of patients who seek advice on account of obstinate pains in the walls of the chest or of the abdomen, and the obscurity that frequently hangs over the origin of their sufferings, must always claim the indulgence of the physician for any attempt to throw another ray of light on their modes of production. I shall, therefore, venture a few remarks on the subject, premising that these will be of an elementary character, chiefly aiming to show that the weight of the upper portion of the frame often contributes to engender sufferings in those parts, in ways not commonly suspected; and that thus there may be simple means at hand of affording relief in cases beyond the reach of drugs. Whilst such is my main purpose, I may be tempted to illustrate the views submitted from collateral sources.

It is an old remark* that rheumatism is oftener found associated with the tendinous and aponeurotic continuations of muscles, especially at the junction of these with bone, than with the muscular fibre itself. And Dr. Inman, of Liverpool, has ably maintained† that much of the pains experienced in the structures now mentioned are due to a stretching of fibrous tissue by the contraction of muscle, or by its excessive tension produced by any other means. He elaborately distinguishes between the pains thus engendered in the muscular fibre, as cramp or spasm, and those resulting from strain along the line of osseous attachment, or line of union between the muscular and tendinous fibres. And he insists that in a weakened condition of the animal system the muscles are more liable to cramp than in a robust, and pain more likely to be felt along the said lines of junction.

Now, I have no further exception to take against this doctrine, but that I do not think that it embraces the whole series of such kind of pains as it aims at accounting for. I would affirm rather, in general language, that all the tissues of the body in their several degrees become painful when they are either stretched or *compressed;* in other words, that there is a state of equilibrium for the structures throughout the animal frame, which is the only one in which they remain free from pain: that the amount of suffering consequent upon the disturbance of this balance of opposing forces depends ordinarily upon the extent of the displacement, or the time during which it is kept up, that is, making allowance for the higher nervous endowment of one part than another, and taking for granted that in such a case the structural points to which the nerves of common sensation, or their ultimate subdivisions are distributed, alone to be squeezed or stretched: for if a nervous trunk be thus affected, painful phenomena will,

* Copland's Medical Dictionary, vol. iii. pp. 616–9.

† Spinal Irritation, or Myalgia, 8vo, 1858; second edition, 1860.

of course, accrue, radiating beyond the point mechanically interfered with.

To explain what I have just stated by an example. In a position of ease, the hand rests pretty much flexed. But, if we spread it flat upon a table, we soon begin to feel inconvenience from the tension of its skin and other tissues. And if we close the fingers tightly upon the palm, we soon find their tops in the fist becoming sore.

Or I may take another example, which may be regarded as furnishing an epitome of the principles I wish to apply in this paper. The eyeball is a globe suspended, as it were, by its muscles, in a fatty bed, and rotated so gently and equably as to preserve usually its proper form. If we make the slightest pressure of the finger through the eyelid over the sclerotic coat, we become informed of the compression of the retina along the line of greatest flexure by a luminous image of the contour of the applied portion of the finger: whilst we feel pain along the corresponding portion of the ocular globe—the visual sentient elements in which the optic nerve ends, manifesting in this way the effects we experience in the tactile sentients. In like manner if we strain the eye by resolutely staring in a given direction, the orbital muscles warp the globe out of form, false impressions of light result, and the power of seeing external objects is almost obliterated, whilst the orbital muscles are thrown into spasm, insomuch that we have a feeling of soreness and fatigue in them, with a tenderness of the ball itself, for many an hour after the experiment. Even any lengthened exertion of the eyes in our habitual occupations is attended with such results in an appreciable manner.

These illustrations will, I hope, help to a readier apprehension of our peculiar liability to sufferings of this order in the regions of the thoracic and abdominal walls, if it shall be made to appear that these are very variously exposed to be forced out of the position of structural balance, and that force applied hereabouts in one part often entails a displacement of the greatest magnitude in some others, in a manner that is not usually thought of.

Let us imagine, as a rough approximation, all the human body, exclusive of the lower extremities, in the sitting posture, as a heavy lever or a beam revolving about the *tubera ischiorum*, as an axis or fulcrum. And let us assume that the centre of gravity of this beam lies between the middles of the *scapulæ*. Then in the upright sitting position the whole weight of the beam falls upon the *tubera*. In the horizontal the weight will be borne up—distributed—along the whole length from the head to the buttock. If we recline, so that the back rests at some point against a prop, we may find the portion of the weight supported by the prop, by drawing a perpendicular to the beam at the point of contact with the prop, and then a perpendicular to this line from the fulcrum, as also a perpendicular from the fulcrum to a vertical line passing through the centre of gravity: when the pressure upon the prop will be to the whole weight of the beam, as the latter of the perpendiculars let fall from the fulcrum to the former. Thus, the nearer to the buttock the prop is placed, and the more the body is made to recline, the greater pressure falls upon the prop. Whilst it is apparent that should we, whilst we sit, lean pretty much back against a single prop meeting the spinal column at any point whatever, a considerable pressure must be endured by that part of the body in contact with the prop, which is a general statement sufficiently accurate for the purpose of a practical essay like the present.

Now let us further assume that the back whilst we sit is supported entirely by a *couple* of props of equal height, and several inches asunder, bearing each, similarly, against one of a pair of ribs. In this instance pain from compression would be felt at the points of contact, as is obvious. But there result also certain secondary effects which, I think, we are apt to overlook. The two ribs are made to revolve, by the pressure, upon their vertebral articulations, and the numerous ligaments that bind them to the vertebræ and neighbouring ribs are stretched, and pain is occasioned in those parts. In addition to which, this movement causes an alteration in the shape of the ring made—with the interventions of the vertebral column, their cartilages, and the sternum—by the pair of ribs. Had such a ring as this been of uniform texture and shape all round, one effect of letting it bear merely its own weight upon two of its under points in the manner here imagined, would be that of flattening the ring above. In such a combination of structures as do exist in these rings, the result must somewhat differ. Yet we may be sure of this much, that, in the case before us, much displacement must occur along the most pliant part of the ring—that is, along the cartilaginous portion, and at the articulations of the cartilages with the sternum: for the costal rotation at the spine tends to bring the sternal ends of the ribs nearer to each other, that is, to bend them at their junction with the sternum, independent of the weight of the upper portion of the ring depressing that region. In other words, pain would be developed at other parts than those where the direct pressure is applied.

Had we placed the two props much further apart, and so as to take the weight of the trunk at the back part of a lower rib on one side, and of an upper one on the other, consequences very similar to those detailed in the case of a *pair* of ribs must have ensued. The ribs would have been pressed in upon the lungs behind, the cartilages would have been bent, and the sternum itself—in young subjects at least—somewhere along the imaginary line drawn diagonally across it, that joins the articulations with the sternum of the two ribs selected. This may be regarded as a general sketch of what takes place in such an experiment; but it must not be forgotten that a pressure received by any rib will in some degree be transmitted to adjacent ones. And it can scarce need to be added that if our back be supported at more than two points, or even along its whole length, the effects here described will only be diminished in amount, and not be entirely obviated.

Now, in the sitting posture, if the support of the back happen to be a flat surface, as in a church pew or box in a theatre, the back rests against the shoulder-blades, that is, virtually the upper ribs; and, *à fortiori*, if the support for the back consists, as in most chairs, of a series of a few horizontal concave bars, the ribs are likely to have to bear, at a distance from their vertebral articulations, a large amount of pressure. In weakly persons such a posture produces—not to dwell on that occurring posteriorly—suffering in the *anterior* part of the chest, especially along the margin of the sternum. For not only from what has been stated must this happen, but, in addition, the thrusting forward of the shoulders renders the pectoral and great serrate muscles lax, and thus deprives the ribs of the support due to the elasticity of these inspiratory muscles, increasing the liability to the kind of inconvenience we are discussing; whilst a confined play of the respiratory movements is induced, entailing other evils. Even lying upon the back in bed—more completely still if so on a hard mattrass without elasticity enough to expand itself decidedly against the spine along the interscapular gutter—will leave the scapulæ—that is, the upper ribs—to bear a weight at least equal to that of the whole thorax, and will occasion a strain along either margin of the sternal gutter.

Having considered some effects upon the thoracic walls of flexure in a plane perpendicular to the spine, let us now turn our attention awhile to flexure in planes passing through its axis.

We will take first the case of a person sitting on a chair whose back does not rise to his shoulder-blades. As he reclines, the centre of gravity of the beam falls above the point of support, and the beam is only prevented from lifting at the bottom by the weight of the lower extremities. Indeed, did we propose to break a stick over the back of the chair, we should put it under the same conditions, confine the end of the shorter branch, and apply our strength (the weight of the head and shoulders in the example of the beam) at the end of the longer one. We see the stick split on the side opposite to that which rests on the fulcrum, because the forces applied tend to lengthen that side, and strain most those fibres which are most remote from the fulcrum. Just so the whole line of tissues along the paths of the recti abdominis muscles and the sternum, from the pubis to the clavicles, are laid under extreme tension; the ensiform cartilage and the whole sternum is depressed towards the spine, and the pleural cartilages warped. And pain again befals the region.

It may be appended that the actual *fixed* point at which the trunk curves backwards is not where the spines of the vertebræ are in contact with the back of the chair, but rather at the posterior margins of the bodies of the said vertebræ, and that the spines gather nearer together and the skin of the back into transverse folds, as we may have observed the skin covering the stick, being broken as above, to do on the side next the fulcrum, because the fixed point, or the unstretched longitudinal fibre, is at the back of the stick itself. Again, if the sole transverse portion of the chair-back consist of one bar at the height, say of the root of the sitter's neck, unless great muscular effort be made, the weight of the loins and thorax would cause the trunk to become convex behind, just as occurs when we stoop to pick anything from the ground; and we know how liable such a stretching of the lumbar muscles is to be followed by crick, and that it always takes a good interval to recover from the soreness resulting from a prolonged stooping posture. But as the line that undergoes, in this instance, no change in length, is somewhere along the anterior edges of the vertebræ, the lower end of the sternum is brought much nearer than before to the pubis, the contents of the abdomen pressed up towards the cavity of the chest, and the abdominal muscles project forwards and cover the ensiform cartilage, so that this, by their support, takes some of the weight of the head and neck, and may thus be flexed, depressed towards the spine, carrying with it the lower costal cartilages of the left side, rather than those of the right, which are supported by the liver. So that this very pain may be produced in the district so much exposed to this misfortune.

If we press upon the sternum directly, as in leaning over a table to write, we may push it towards the spine and bend the attached cartilages. If we curve the trunk laterally, as in leaning on a table sideways, we stretch the opposite oblique muscles, and may occasion pain either at their upper costal insertions or at their lower pelvic. The same effect may follow by lying in a bed; for on the side on which we lie, by the weight of the lumbar portion of the trunk, the natural concavity of the waist tends to become obliterated; for this portion of the body is now primarily supported at the brim of the pelvis and lower true ribs.

I must here conclude my anatomy of trunk-pains, as I believe I have indicated the fundamental way in which the commonest of the chronic kind are generated. It were an indefinite task to work at exhausting the subject. The weight of the arm may make the muscles of the shoulder ache; and it is conceivable that one might write a separate history of the sufferings of all the muscles of the trunk. Besides, the effect of the compression or weight of clothes might be expatiated upon. Such effects as these, I presume, are likely to be detected with little difficulty, by those familiarized with scrutinizing the sources of pains in the thoracic and abdominal walls, by first eliminating those that may arise in any of the modes above described. As far as I have here gone I am persuaded that I have not transgressed the bounds of clinical experience, and that there is not one of the ways of suffering just pointed out, which I have not met with in several instances, and seen relieved simply by precautions against the strain that occasioned it.

To give cases in detail would only be repeating in the form of narratives the foregoing explanations, so I will content myself with a simple sketch as a type of the class. I once had to think for a young gentleman of rather studious habits, of a slender symmetrical figure, but enjoying good health, except that he was rendered miserable by attacks of pain at the sternum, and along the cartilages of the ribs, commonly about the lower end of the sternum, though sometimes higher up—a condition of things that existed for two or three years in spite of tonic and other medical treatment. But he having himself observed that he had once been extremely afflicted after pressing the epigastrium against a table in drawing, it soon came to light that all his sufferings arose from posture. Even lying upon his back in bed was found to bring on a fit of pain, and resting his back against any support that threw the weight rather upon the ribs than the spinal column did the same. He now soon got rid of all his troubles by habitually supporting his back, when studying, at the spine. I omit to state how, as I mean to consider the best mode of doing this in another part of this paper.

Perhaps it may not be superfluous to subjoin, that such a relative depression of a rib with respect to those next it, as may happen when it has to bear undue pressure in some of the modes above treated of, may sometimes expose the trunk of an intercostal nerve to squeezing or stretching. These nerves are, indeed, curiously lodged in grooves along the lower borders of the ribs, which guard them from accident; but they traverse a short space from the spine to enter these, and do not seem to lie absolutely safe from mechanical injury. I am not sure that I have not met with cases of neuralgia shooting along their paths from this cause, wherein no spinal deformity existed. It is the nerves most unprotected against pressure and cold that appear most liable to neuralgia. I am confident I have known sciatica to be engendered by a habit of sitting sideways, in a partially recumbent manner, on a sofa or chair, so as to squeeze the sciatic nerve at its emergence from the pelvis into the thigh; and several instances in which a fit of tic douloureux has followed a nap upon a book or some hard thing for a pillow. Nevertheless, these nerves cited in illustration are oftener affected by their exposure to great differences of temperature, since even the sciatic, by careless sitting upon surfaces which rapidly conduct heat, are subjected to such vicissitudes.

In juxtaposition with the reflections with which this paper sets out, though I would be chary of applying them too far, I have reason for surmising that there are some ailments whose seats are among the abdominal or thoracic viscera themselves, instead of in the trunk walls, which can only be understood by such rudimentary considerations. I will touch upon two or three rarer examples which have fallen under my own notice.

A while since a woman of about thirty-six years of age became a dispensary patient of mine, for what, *primâ facie*, seemed to be a form of gastralgia common enough among such patients. She asseverated that she literally feared to eat a full meal, so greatly did she suffer pain at the pit of the stomach, which radiated along the lower border of the thorax for an hour or more after a meal; that she had become very thin in consequence of enforced abstinence, and that several medical men had failed to give her any relief. Though her affirmed loss of flesh seemed justified by her appearance, and her look of distress was great, yet she had not the aspect nor any of the prominent symptoms of organic disease of the stomach; there was no chain of evidence to convict it of scirrhus or ulcer. It was not until I had exhausted in vain all the current remedies for facilitating digestion that, upon a more deliberate sifting of the symptoms, I ascertained the curious fact that if she went to bed immediately after eating supper, this meal gave her but little inconvenience. Thereupon I directed her to lie down after all her meals, and to eat good solid dinners of animal food and vegetables, drinking bottled porter with them. With some difficulty I got her to persevere in this practice, and she was not many weeks before she was convalescent. Now, we cannot imagine that mere distention of the stomach occasioned the suffering, because the recumbent posture could not obviate such an effect; nor that there was an abrasion of the mucous membrane so placed in the organ as to be brought more into contact with the food in one posture of the body than another, for whatever may have been fancied to the contrary, the pressure of the atmosphere must keep the stomach always closed tightly upon its contents. It seems to me that the pain was evolved through the *weight* of the meal—by traction upon the diaphragm at the cardiac orifice possibly, but mainly by traction upon the liver by the lesser omentum, and thereby upon the xiphoid and costal cartilages uniting with it. I would compare the case with the two following.

About the same time I had under my care two cases parallel to each other in all essential particulars. The first was that of a miner who had been for more than half a year under treatment for what he described as a most severe pain along the margin of the cartilages of the lower ribs on the right side, extending from the edge of the liver downwards, through the abdominal wall, and penetrating, he fancied, inwards. He shrunk when I touched the abdomen over the parts referred to. It was only with difficulty he could stand upright, or walk, or rather crawl, about. Being unable to discover anything amiss with the liver, or indeed, at first view, any general sign of ill health, except lack of flesh, I commenced a devious examination of his body, and, to my surprise, found that he had effusion into the pleura, not on the side complained of, but on the left. Posteriorly and laterally the bottom of the chest on this side was much duller on percussion than on the corresponding portion of the right side, notwithstanding the hepatic dulness here existing. At the same time respiration on the left was barely audible at levels, where on the right, it was very so; and fremitus on the left was almost as barely perceptible, whilst the intercostal spaces protruded. By putting him under a succession of blisters over the diseased region, instead of, as had before been done, over the region of pain, with iodide of potassium, diuretics, and then tonics administered internally, the physical signs of effusion soon began to diminish, and with this the pains in his right side.

A few months afterwards a once stout and lusty smith, who had been working in London, returned to Cornwall in a very weak state, wasting and labouring under excessive pain at the right hypochondrium, and spreading over the abdomen, which pain he accused as the sole cause of his troubles. He had been perfectly well until about six months before, when, after an imprudent exposure to a piercing cold draught of air, he became suddenly ill, his suffering settling soon entirely in his right side. This man's story and attitude reminded me so forcibly of the foregoing case that I at once examined his chest, and discovered more strongly marked signs of pleuritic effusion on the left side than in that of the miner—a condition of things which, as in that instance, had been overlooked. He commenced, too, to recover from the moment the true seat of disease was besieged.

With respect to the propagation of the pain to the opposite side in this pair of cases, it seems to me that the mode might have been thus: The fluid in the left pleural sac would have nothing to sustain its weight beneath the diaphragm but yielding viscera, consequently the left half of that muscle would be depressed below its usual region; but this could not happen without dragging downwards the right half also—that is, not without pulling downwards upon the liver, and exerting a force to rotate it on its anterior border from the costal cartilages which cover it. In this way not only would these cartilages themselves be warped, but the ligaments of the liver would be made abnormally tense, and therefore their peritoneal continuations along the parietal abdominal wall be unduly stretched, as well as such tissues as it lines. I venture on this conjecture on taking a *common* survey of the three cases last introduced, under guidance of the more general principles on which this paper proceeds.

Infra-mammary Pain.—I must emphatically repudiate the idea that I would offer the preceding propositions as a key for opening the mysteries of *all* the aches we meet with in the thorax and adjacent parts. In such an affection as hysteria—for example, perturbations of all the other parts of the nervous system are far too transparent for me to go so far as to say that the tactile nerves, or such nerves, if they be other than these, whose office is to inform of violence done to the tissues, may not also be morbidly affected. Nevertheless, let us see whether the pain under the classic designation heading this paragraph admits of a plausible explanation by the principles submitted. In the autumn of 1858 this pain was made a topic of discussion in the medical weekly publications. Dr. Inman, Dr. Fuller, Mr. Holmes Coote, and others, took part in the debate. I do not know that I can do better than take the main points at issue from their statements.* The first of these writers, in reply to the second, who had asked him why the seat of a chronic pain is more commonly in the left than in the right side, remarks: "At one period I thought I could trace some connexion between the locality of the pain and the position commonly adopted by the sufferer; but after a more extended inquiry I have been obliged to consider the facts referred to as inexplicable in the present state of our knowledge."†

Mr. Coote‡ comments upon this correspondence. Premising that it is not to be doubted that it may have "more causes than one," he affirms that he has noticed it to be "one of the very earliest and commonest symptoms of incipient lateral curvature of the spine," and he reminds us that it had been recognised in this point of view since the days of Delpech (citing his 'Orthomorphie,' tome ii. p. 10,

* I have not access to the *second* edition of Dr. Inman's 'Myalgia;' I am therefore unaware if he has therein made any new observations upon this subject.

† British Medical Journal, November 27th. ‡ Ibid., December 4th.

1828); "a constant pain, somewhat vague in its seat, which takes place sometimes in the side of the chest below the mamma, sometimes in the epigastric region. This pain has no known cause; its duration is usually constant, but it is infinitely variable; its periods of calm and exacerbation have nothing regular; there is no disturbance of the functions of the organs in the seat of pain; nothing quiets it, nothing relieves it. It is accompanied by slow, progressive, and inexplicable deterioration of the state of health. It is evidently allied to something grave but quite clandestine." Delpech then instances a girl of eleven years thus afflicted, who, by means of a plumb-line, was found to have a slight lateral curvature. "The greater frequency of spinal curvature," Mr. Coote subjoins, "may explain in some measure the obedience of infra-mammary pain to the same law." Whilst thus suggesting that it may not improbably be muscular, he does not indicate by what mechanism the curvature or the pain inclines to one side rather than the other.

However, as Mr. Coote had (as recently as in the 'Lancet,' Oct. 23rd, of the same year) given a graphic sketch of the course usually taken by spinal contortion, I will continue to follow him:—"The curvature generally commences in the upper dorsal region, and extends directly to the right in one even sweep up to the junction of the lower dorsal with the lumbar vertebræ; then the direction of the articulating surfaces is altered, and the movements of the spine change from the lateral inclination to the antero-posterior movement, as in springing; a second curve forms in the lower region to the left, accompanied with a rotation and twisting of the vertebræ." He then proceeds to quote from the plates and descriptions of M. Bouvier (Physician to the Children's Hospital, Paris), explanatory of the packing of the viscera found on dissection of cases of confirmed lateral curvature of the spine. Thus, in what he calls *le premier plan*, that of the above type, the latter says the "lungs are apparently not much deformed, when viewed anteriorly. But the right is reduced in height, its base being pressed by the abdominal viscera: the left has its inferior lobe compressed between the ribs and the heart. The heart is voluminous, and closer than natural to the ribs of the left side. The liver is much deformed, deeply fissured, and, as it were, mounted (*à cheval*) on the crest of the ilium."

Now, it is worthy of consideration whether we may, by chance, find the cause of the preference of the line of lateral curvature for one side of the body rather than the other, in a difference in the compressibility of the viscera that are lodged against the parietes of the respective sides. If there be any just foundation in this notion, the only organ within that seems at all capable of affording a relatively greater support is the liver. This is a firm, resisting mass, filling the whole of the right hypochondrium; and if the spine becomes drooping and weak, or its muscles no longer able to sustain the upper portion of the body erect; if the vertebral column begins to yield, so that a weight that it should carry becomes cast upon the viscera, it seems not absurd to regard it as not improbable that the circumstance of the left lower ribs and cartilages covering compressible lung and stomach, whilst the right are supported all along within by a much firmer body, may be the means of causing the chest to arch inwards on the left rather than the right.

The details of M. Bouvier give strong countenance to the idea that the left pleural cartilages are more liable than the right to be warped out of shape by any of the modes suggested in the beginning of this paper. And thus the left side will be more frequently the seat of pain than the other. By whatever cause of ill-health the general strength fails, this liability will be more remarked. This seems a necessary consequence of the manner of stowing the more massive viscera in the chest and belly. We may assert this much without pushing our conclusion so far as to assume that infra-mammary pains can be of no other order. I can well conceive that the site of the liver may determine that we are not left but *right* handed, because the pectoral and other muscles that move the arm or shoulder upon the trunk have, through the hepatic support, steadier points of resistance on this side than the other; for this organ manifestly plays an important mechanical as well as a more purely glandular part, in the economy.

Having so nearly approached the subject, perhaps I may be permitted to say a word or two on the thoracic distortions in rickets. Dr. Jenner* speaks thus:—"The deformity of the greatest interest to the physician is that of the thorax. The back is flattened. The ribs are bent at an acute angle where the dorsal and lateral regions unite. At that part the lateral diameter of the thorax is the greatest. From it the ribs pass forwards and inwards to the point where they unite with the cartilage; on that line the lateral diameter of the thorax is the least, the cartilages curving outwards before turning in to unite themselves to the sternum. The sternum is thrown forwards, and the antero-posterior diameter of the thorax is abnormally great."

"The great determining cause," he afterwards subjoins, "of the thoracic deformity is the atmospheric pressure; this is aided by the elasticity of the lung," and the fact that the more resistant viscera underneath are obstacles to recession of the chest-walls where they lie. It appears to me difficult to account for the "acute angle where the dorsal and lateral regions unite," or the flattened back, by such diffusive pressure as must be produced between the air without and the air-containing elastic lung within. On the other hand, it is certain that if the ricketty child be let to lie much on its back, which, from its inability to stand and sit upright, would inevitably happen, the weight of the chest must so press upon the softened ribs as to tend to bring the spines of the vertebræ and those of the shoulder blades on the same level, or to flatten the back. Again the mere weight of the costal cage of bone and cartilage tends to flatten it in front (above), so that this cage would tend to fall flat in front and to crack sharp off at the sides. Now, when we take into consideration with these facts the obstacle to recession of the cage in front furnished by such solid parts as the liver, heart, and spleen, as Dr. Jenner himself does, I think it may be worthy

* Medical Times and Gazette, March 17th, 1860.

of reflection whether this is not the simple history of the development of the deformity in question. Allowing that some deviations must be expected to be produced in the form of the chest by weights thrown upon it when other postures are assumed, the principle involved in this explanation would be in accordance with Dr. Jenner's general mode of accounting for ricketty distortions in the limbs, which he ascribes to their weight or the weight of other parts bearing upon them. He remarks,—"in excluding muscular action from all direct share in the production of curvature of the long bones in rickets, I am, so far as I know, unsupported by any authority;" this observation, it may be implied, he would extend to the chest, as he cites Rokitansky's opinion of the thoracic deformity being caused by a want of power in the inspiratory muscles, and moots no other hypothesis but his own.

Finally, returning from these collateral meditations, I will venture a word as to the practical use of such considerations as form the fundamental reasonings of this paper. If suffering is occasioned in a patient by a disturbance of structural balance in any part, the obvious indication of cure is to remove the cause of the disturbance. If any body by pressing upon the eyeball deprives it of its sphericity, remove the body that does so. If a patient leans against his sternum in writing till he begets suffering thereabout, take care he ceases to do so. Each case requires its special precaution so clearly that it were idle to iterate the fact. But I hardly think it unprofitable to invite attention to a very homely topic before I take leave of this essay.

It has been shown how important it is that the structural balance of the trunk should be ordinarily preserved. When we walk, this equilibrium is stable, and when we lie it is approximately just. But we spend a great portion of our time in the sitting posture, and what provisions have we that the balance shall be easily kept in this attitude? In plain English, on what principles are the backs and other upright supports of our chairs and sofas constructed? Our easy chairs and couches, not to say chairs for general use, show no conformity in the principles upon which those uprights are conceived. All looks as if there were no knowledge of comfort in such things. The majority seem made with a view to torture rather than ease. I cannot therefore deem it a thankless task to endeavour to ascertain what are the requisites of such structures. At least, I will give a device for what I have concluded to be the best form of chair as an example. I will do so in dimensions to fit the use of a man of middling size. The chair shall be of wood, for if it be easy in bare wood, it cannot fail to be so when cushioned.

Let the seat be 18 inches from the floor and of the shape of a semicircle of a diameter of 21 inches, whose straight edge is the front of the seat, and let it, in a style which is common, be scooped toward the back to fit the nates. Again, in the usual style of elbow chairs, let there be a horizontal semicircular bar for the elbows to rest upon, 11 inches above the seat and of its diameter, and let this elbow-bar be supported on either hand laterally by four or five upright bars with equal intervals.

In the middle of the back let the support for the elbow-arch be a flat plank of $5\frac{1}{2}$ inches' breadth, or rather, let two short elbow-bars be let into such a plank rising from the middle of the posterior border of the seat, to the height of 22 inches, and let this plank be curved to the lateral contour of the loins, as may be seen in the cushions of railway carriages. Or in definite measurements let the plank, at vertical heights of 2, 5, 10, 15, 22 inches, be gradually inclined backwards to the horizontal extent of $\frac{1}{4}$, $\frac{3}{4}$, $2\frac{1}{2}$, $3\frac{1}{4}$, 4 inches respectively. Beginning at the top of the plank, let a longitudinal groove be cut a foot down the anterior face of the plank, of 1 inch wide and $\frac{3}{4}$ inch deep; let its edges be bevelled away, as also other sharp edges of the plank. Below the elbow-arch the plank may be hollowed in the horizontal direction to coincide in curvature with the seat.

Graceful appearance apart, we have here the elements of a comfortable chair. The lower portion of the back will form a cradle for the pelvis, and prevent its weight from effecting pressure upon higher parts of the trunk; the curved portion rising therefrom will bear up the loins and lower part of the chest. The back piece will allow the middle channel of the back to rest against it at any and every point, whilst its longitudinal groove will afford an escape from pressure for the thinly-covered vertebral spines, leaving this to be encountered by the bed of muscle at their sides. When any pressure that might be extended to the ribs would not affect the form of the chest owing to the short leverage at the vertebral articulations of the ribs along the channels at the sides of the spine. The shoulders play backwards and forwards without impediment; and since the trunk preserves its natural form as in a standing position, the respiratory muscles meet with no hinderment in their office. The arms, resting upon the elbow-bars, do not cast their weight on the shoulders, whilst these are steadied.

But the combination of contrivances fulfils more recondite conditions. Not only do the ribs and abdominal surface ebb and flow as we breathe, but the curvature of the spine oscillates in degree. In this oscillation the backpiece allows the vertebral column to *roll*, as it were, along it—it is a sort of involute, for the chain of vertebræ, as the evolute, to wind and unwind upon. For these reasons I have placed persons suffering from pains about the sternum, which have been acquired by pressure habitually thrown upon the shoulders, to sit against a narrow pillar or vertical plank, as may be met with in panelling, and the ready expedient has answered my expectations.

People do not sit long in one posture, and this chair allows a considerable degree of shuffling about without the sacrifice of its presumed advantages. If to change the point of chief pressure the nates be slid forwards, the backpiece will still take spine, and chiefly at another spot, so as to relieve the points of pressure along the back also; and the semicircular shape of the back-cradle will give the sitter a fitting support for his back, however he leans about.

Though it has been laid down as a rule that the shoulders should not rest against any framework, it would not be amiss if there were such a framework forming a portion of a larger circle than the framework described, and crossing the upright bar at the top posteriorly, so that this bar be let into the upmost crosspiece in front, and that it project for an inch beyond it. Thus this extra framework, which may be partially formed by prolongations of the hinder upright bars that support the elbow-arch, would form recesses where the shoulders may be brought to bear a little to steady them laterally, and that they may take a little of the pressure of the trunk at our pleasure.

I have given the above description in stiff-lines for the sake of being explicit and intelligible, but there is no reason why elegance should not be realized in the design. The elbows may be curved along their upper margin, the bars replaced by ornamental carving or wicker-work, the backpiece extended in a curved fashion below, or perforated for ornament, and may be undulating upwards, spreading over the top of the shoulders on either hand. Cushioned chairs may be so stuffed as to afford a backpiece as here indicated, as well as the transverse projection for the loins as they now commonly have; and if carried as high as the head, bear a cushion projecting well *forwards*, to take it, without the neck being unnaturally thrown back.

I have thus devoted some attention to the analysis of the conditions essential to comfort in an article in daily use by every healthy person who is not destitute or a bed-lier; and whatever be thought of these reflections, it will not be gainsaid that they treat of questions of concern to the healthy, and of vast interest to a host of invalids.

Art. IV.

On the Nature and Cause of the Respiratory Murmur. By Hyde Salter, M.D., F.R.S., F.R.C.P., Assistant-Physician to the Charing Cross Hospital.

The necessity of physiological as a foundation for pathological knowledge is in no way better illustrated than in the physical diagnosis of chest disease. Not only is it necessary that we should be thoroughly acquainted with the chest-sounds in health in order that we may have a fixed point to start from, and at once detect any aberration from their natural standard; but since morbid heart and lung-sounds are merely modifications of the normal, and are generated by the same machinery, a knowledge of the mechanism of the sounds in health, and of the way in which the different conditions that produce them contribute to their production, is absolutely necessary, in order that we may be able not only to detect but to interpret their modifications in disease, and to perceive at once what particular aberration from the conditions of health the morbid sound we hear necessarily implies. To one who has made himself master of the *rationale* of the sounds of health, who knows exactly what produces them and how, the sounds of disease become self-interpreting—they explain themselves. Without this knowledge they merely assert their existence, and give us no clue to their meaning; they say that something is wrong, but they do not say what or how. I have often been struck, when listening to the morbid modifications of the respiratory murmur in disease, with the thought, how incomprehensible they would be, and how utterly they would fail to tell their story if the *rationale* of the healthy sound was not understood (correctly appreciated). It is considerations of this kind that impart to many physiological questions in relation to respiration an interest and an importance that of themselves they would hardly seem to deserve.

One of these questions is: What is the cause of the respiratory murmur; where and how is it generated?

That this question is a difficult one is sufficiently shown by the many answers that have been given to it; that it is an important one every one must feel who is in the daily practice of auscultation, for it is the fundamental basis, the starting-point from whence diverge all morbid breath-sounds, and on the views that we hold of its nature and seat must depend our interpretation of those conditions in which it is modified, obscured, or altogether lost.

It may not be amiss for me, before discussing the question, to mention a few of the opinions that have been entertained on it by distinguished authorities.

M. Beau* has advocated the view that the respiratory sound heard at the surface of the chest is not produced in the lungs, but at the pharynx, and simply depends on the transmission to those organs of the pharyngeal sounds attending the entrance and exit of air through the pharynx.

Dr. Spittal† has advocated similar views, only he makes the seat of the sound the glottis instead of the pharynx.

Laennec considered its immediate cause to be the entrance of air into and expulsion from the air-cells.

Dr. Herbert Davies assigns the murmur to air-friction in the entire bronchial system; he thinks the inequalities produced by the cartilaginous rings have something to do with its production; and that the relative loudness of inspiration is due in part to the splitting of the currents of inspired air by the angles of junction of the tubes, in part to the resistance the contractile lung-tissue offers to its ingress.

Skoda is evidently of opinion that the bronchial vibrations contribute, by their conduction through the ramifying columns of air, to the respiratory murmur; but he is not very definite, and gives no details of the mechanism of the transformation of the one sound into the other.

Dr. Walshe attributes the sound to the entrance and exit of air to and from the air-cells, and believes that the terminal portions of the bronchial tubes are also concerned in its production. He believes also that there is a greater resistance of the textures during inspiration, and that this contributes to the excess of inspiratory over expiratory sound.

Dr. Blackiston, failing to recognise any other adequate cause for the greater loudness of inspiration than expiration, adopts the opinion that the ultimate bronchiæ contract at each inspiration, as an adjuvant

* Archives Générales de Médecine, 1834.
† Edinburgh Medical and Surgical Journal, vol. xli.

power to help the air into the air-cells, and in this way produce a greater rush of air, and therefore louder sound, at inspiration; and, relaxing at expiration, allow the air to pass noiselessly out.

In discussing this subject what we have to explain is a sound of a certain peculiar character, audible, (as I am inclined to believe,) in the tranquil and unconscious breathing of a healthy adult, *only at inspiration*,* but immediately becoming audible on expiration when the breathing is at all forced, and changed in a certain way or entirely destroyed by certain disturbing conditions.

Our theoretical difficulty depends on our having to offer an explanation consistent with these peculiar features and circumstances of the sound; our practical difficulty depends on our being unable to imitate artificially those conditions which generate the sound in the living body. Our only elements of evidence for the solution of the problem are derived from the character and attributes of the sound itself, from a knowledge of the structure and working of the parts concerned in its generation, and from the way in which the sound is modified in disease.

One thing is certain—that the sound must be generated in the immediate neighbourhood of the superficies of the lung; its seat must

* In stating my belief that in the tranquil, *unconscious* respiration of health, no sound is audible at expiration, I believe I stand alone (although others admit that it is sometimes so), and in the face of the universal recognition and description of a normal expiratory murmur it may be rather rash in me to express such a belief; but the more I examine the subject, the more convinced am I that such is the case; the opinion forced itself upon me, in the first place, as a result of simple observation, and in spite, of course, of my acquired opinions.

But I admit that in any case in which the normal condition of the breathing is affected by any disturbing element, expiration becomes immediately audible, and therefore that, in nineteen cases out of twenty that come under our medical observation, expiration *is* audible, because in such a percentage of cases is there some source of disturbance present. In the first line of this note it will be observed I have put the word "unconscious" in italics, and I have done so because consciousness is of itself an element of disturbance; directly the breathing becomes conscious and the will is directed upon it, it becomes unnatural, and is carried on at too high a pitch, and expiration becomes audible. Now when a patient's chest is examined (or a person in health, it is the same thing), his attention is immediately directed to his breathing—he breathes voluntarily, and very often (with the perhaps unconscious purpose of enabling you to hear better) breathes much deeper than natural. So greatly does this element of consciousness disturb the passive tranquillity of respiration, that we cannot for a single minute direct our attention to our breathing, and supervise it by our will, without inducing a peculiar kind of breathlessness, which will not subside till the attention is directed to some other subject, and respiration left to regulate itself.

If, then, this slight exaltation of respiration, produced by the influence of the will, renders expiration audible, much more will it become so if the subject of the examination is emotionally excited, if from recent exertion or coughing the breathing is quickened, if it is embarrassed by palpitation or heart disease, or if there is such disease of the lung elsewhere as throws on the lung at the part listened to more than its work, and renders the breathing compensatory.

But take a healthy man in a state of rest, and while listening to his breathing, divert his attention to other subjects, or auscult him when sleeping, and I feel confident it would be very difficult to assert that any sound whatever is audible except at inspiration. Not that it is *necessary* to divert the attention in auscultating, for without doing so expiration will constantly be found soundless, but I am sure that when it is audible, it will very often become inaudible when the attention *is* so diverted.

Of course, when we are speaking of the sounds of respiration, we are speaking of them, not as they are under exceptional circumstances, but as they are as a rule, not merely when we are listening to them, but at all other times.

be a very thin layer immediately beneath the pulmonary pleura. And why? Because it reaches the ear without conduction by the lung, and therefore must be a surface sound. That the lung is not concerned in its conduction we know, because the moment even a thin portion of the pulmonary structure acquires conducting power by condensation, we hear sounds that before were inaudible. Or stating it conversely, which perhaps may be clearer, the moment even a thin lamella of lung becomes a conductor by solidification, we hear sounds previously inaudible; the conducting power of the lung is therefore *nil;* the conducting power of the lung being *nil,* any healthy lung-sound must be a surface-sound; the respiratory murmur is therefore a surface-sound.*

Now, what structures have we immediately subjacent to the surface of the lung? The ultimate bronchial tubes, the intercellular passages, and the air-cells. One or all of these, then, must be the seat of the sound—Which? To answer this question we must understand the exact structure and relation of these parts.

The air-cells are minute alveoli, or recesses, clothing the sides and extremities of the lobular or intercellular passages. Each lobular bronchial tube, on entering its lobule, divides from four to nine times, according to the size of the lobule, and reaches the smallness of from $\frac{1}{15}$th to $\frac{1}{30}$th of an inch; its branches then undergo no further reduction in size, but even enlarge a little and diverge in different directions towards the surface of the lobule, under the name of the lobular or intercellular passages; it is these terminal air passages that are clothed with and terminate in the shallow pullulations called air-cells. The septa projecting between these air-cells, which in fact form them, are of course, like the cells themselves, shallow, and the arrangement gives to the inner surface of the lobular passage a pitted or honeycombed condition. Each ultimate element of the lung must, of course, vary in size during respiration in just the same proportion as the varying capacity of the whole organ. It has been calculated that the lung contains after an ordinary expiration, 170 cubic inches of air; and as at each respiration 20 cubic inches of air are taken in, the capacity of the chest in ordinary breathing must vary from 170 to 190 cubic inches. Twenty of a hundred and ninety is rather more than a tenth part; each air-cell is therefore a tenth larger at inspiration than expiration.

Now, is it conceivable that this slight variation in the capacity of these shallow open concavities should be attended with any sound? I cannot conceive it physically possible. For be it remembered that the air-cells are not nearly-closed cavities communicating by constricted orifices with the general cavity of the lobular passage, but wide-mouthed and patulous, like a tea-cup. And be it remembered,

* I do not mean to say that the conducting power of the lung is *absolutely nil,* that the sound must be *at* the surface and no way beneath it. The effect of the non-conducting power of the lung would probably be, and no doubt is, first to make a sound muffled, and then inaudible; we do not know the exact limits beneath the surface at which a sound would cease to be heard. But we *do* know that lung condensation, sufficient to give a bronchial character to the breathing, need extend only a very little way in.

too, that in respiration the air is not pumped out of and into these cells, but, as they undergo this slight change of volume, a small part of their contents passes just without them, and then again, on their recovering their capacity, from without just within them—if one can speak of "within" and "without" in reference to such slight interchanges of situation. For, really, the renovation of the air in the recesses of the lung does not depend on its actual removal, but upon the law of the diffusion of gases. The purpose of the movements of respiration (independent of their influence on the intrathoracic circulation) is to carry the air into bronchial tubes of a certain degree of smallness (I do not know exactly what), and beyond that, all renovation is entrusted to diffusion action; and were it not for this the air would simply oscillate across a plane corresponding to the orifice of each alveolus, and no renovation would take place. We have only, I think, to realize what I have been endeavouring to describe, to be convinced that the air-cells themselves cannot be the seat of a breath-sound.

But, while the movement of the air at each alveolus would be so slight, so almost inappreciable, the collective expansion of all the alveoli common to a lobular passage, and the consequent abstraction of air from the general cavity, would be considerable, and would create a considerable rush of air into the lobular passage to supply its place; for the modicum of air, however small, appropriated by each dilating air-cell, would, of course, be multiplied by the number of cells communicating with the common axial cavity of the lobular passage. In the same way, the abstraction of air by all the lobular passages branching from a single ultimate bronchule would create a still more considerable abstraction of air from *it*, and consequently rush of air into it to supply the place of that taken. Conversely, in expiration the contribution of a tithe of their contents to the general cavity, on the part of all the alveoli common to a lobular passage, would expel a considerable amount from it, and the air thus contributed by all the lobular passages common to one ultimate bronchule would occasion a still greater rush of air into and through it, and so on. In fact, the law of the branching of bronchial tubes—that the sum of the squares of the diameters of the branches is greater than the square of the diameter of the trunk from which they proceed—necessitates the continual retardation of the movement of the air the nearer we approach the ultimate bronchial ramifications, the movement having its maximum at the trachea, and its minimum, almost perfect rest, at the plane of the orifices of the alveoli. To the same law is attributable the same result in the sanguiferous circulation, where we find the movement of the blood in the carotids to be nine inches a second, and in the capillaries $\frac{1}{30}$th of an inch in the same time. To this increasingly rapid rush of air, and not merely to the increasing size of the tube, is to be attributed the more and more blowing character that marks the bronchial breathing of larger and larger bronchi.

We see, then, that while we cannot refer any sound to the movement of the air in the air-cells themselves, we very reasonably can to that of the air to and from the lobular passages, still more to that in the ultimate bronchiæ, still more to that in the lobular bronchiæ. To how much of this terminal system of air-tubules are we to assign the respiratory murmur? This is a question that we do not at present possess the means of answering, because we do not at present know, as I mentioned just now, the limit of the conducting power of lung tissue. We have the results of no good experiments on this subject, nor do I think that it is one on which accurate experiments could be made, because I do not think it one in which it would be possible to imitate artificially the conditions of life. We must fall back upon clinical observation and upon the evidence afforded by critical post-mortem examinations following upon accurate auscultation during life. But this much is certain, that the lamina of lung yielding the sound must be, as I said before, exceedingly thin, and that the inaudibleness of any sound would increase in proportion to the depth from the surface. Possibly it may be restricted to the lobular passages subtending the subpleural air-cells; but I would not by any means deny the participation of the ultimate and even the lobular bronchiæ in its production, and I should not consider the non-tubularity of the sound as any evidence against this possibility, because I think it doubtful if a sound of a tubular or bronchial character could be elicited from tubes of such a degree of fineness, and because, even if it could, a sound from a single source becomes so much changed when it is multiplied by numbers, just as a single human voice differs from the hum of a crowd. But I would not say anything certain as to the distal limit of the respiratory murmur—distal, I mean, from the ear of the listener. What I wish particularly to enforce is that its proximal limit must be the lobular passages, and that the air-cells proper cannot generate it. All we know about its distal limit is, that if the bronchiæ are at all concerned in producing it (i.e., immediately concerned in producing it as we hear it), clinical observation and the physics of the lungs both teach us that it can only be those of the finest calibre.

When I speak of air-cells, lobular passages, &c., of course I refer only to those of the superficies of the lung, as similar sounds generated in similar structures in deeper parts of the organ must necessarily be inaudible.

There is another reason, to which I have not referred, which makes me think that the respiratory murmur must have a tubular or quasi-tubular seat, and cannot be formed in the air-cells; it is, that fine crepitation, such as that of pneumonia, *supplants* it; it does not merely drown it, it supplants it, the two do not co-exist. Now large mucous rhonchus, or subcrepitant rhonchus, or even the much smaller crepitation of the capillary bronchitis of children, does not *supplant* the respiratory murmur; you may hear the murmur through them. These moist sounds, therefore, do not destroy the normal breath-sound, when they are not at its seat. I think we may therefore fairly conclude that a sound that *does* supplant it—that takes its place, does so because it is generated in an identical situation, and so destroys the conditions for the generation of the natural sound. I have never known true pneumonic crepitation co-exist in the same spot with the respiratory murmur, unless the crepitous patch was so small that the normal breath sound could be conducted from the neighbourhood.

Now, those who believe that both these sounds have their seat in the air-cells, will no doubt at once adopt my conclusion (that the supplanting of the one by the other shows identity of situation), and will see in the power of pneumonic crepitation to efface the respiratory murmur an additional proof that this latter has its seat in the air-cells. But I cannot but believe that any one who analyses the sound of crepitation, and has made himself familiar with all the grades of moist sounds, from bubbling mucous rhonchus to the crepitus of infantile capillary bronchitis, will at once perceive that these and pneumonic crepitation are sounds of identical nature; all of them true moist sounds, depending on the interrupted passage of air through fluid, bubbling rhonchus being at one extreme of the series, and crepitation at the other. I have often been struck, when listening to the capillary crepitus of infantile bronchitis, with its close approximation, amounting almost to identity, to the fine crackle of pneumonia. In some specimens I should really have felt some difficulty in distinguishing the one from the other. In the resolution of pneumonia, too, when the exuded matter is passing along from the ultimate bronchial twigs to tubes of larger calibre, the original fine sound passes into the coarser kinds by insensible degrees, every gradation of size being filled up.

If, then, pneumonic crepitation is a veritable tube-sound, and its seat the microscopical tubes immediately subtending the air-cells, the supplanting and destruction of the respiratory murmur by it would show that this latter has an identical seat, and is therefore a tube-sound.

And now, as to the question of the actual generation of the sound, and why it is, *par excellence*, an inspiration sound.

I think there can be no doubt that the sound depends, like all healthy breath sounds (tracheal, bronchial, puerile), upon the friction of air and tube-wall, the vibrations being modified and intensified by the special acoustic properties peculiar to the tube form. The same conditions for the production of sound exist in the ultimate bronchial ramifications as in the windpipe and the larger bronchi, modified only by the extreme smallness of the tubes, and the comparative slowness of the movement of the air in these recesses of the lung.

But is the sound *generated* at the point at which it is audible—that is, in the sub-pleural stratum of the minutest air-tubes, the only source of the respiratory murmur? Is the air-friction in these minute tubes all that we hear? I cannot conceive but that the sound of which we know the whole bronchial system to be the seat (under the name of tracheal and bronchial breathing), must also contribute to its production. It is impossible that the vibrations in the larger trunks should not be conducted to the ultimate bronchial ramifications, and contribute their quota to the sound audible at the superficies of the chest.

For believing that these vibrations are thus conducted by the ramifying columns of air in the bronchial tubes to the chest wall, we have not only antecedent reasonableness on physical grounds; we have positive proof of it in the existence of vocal resonance and vocal fremitus; we have proof in these that the laryngeal vibrations are conducted through all the ramifications of the bronchial tree, and registered at the thoracic parietes. And if this were considered insufficient proof that the lighter and more delicate vibrations of air-and-wall friction were equally conducted, we might adduce the fact that the superaddition of the faucial and glottic sound in breathing may be distinctly appreciated at the thoracic parietes. I am quite sure that these sounds *are* audible at the surface of the chest, for I have often found them so mask the true respiratory murmur that I have been obliged to tell the patient to shut his mouth and breathe through his nose, or "cease making that hissing noise in his throat," before I could clearly hear the pure and unmixed respiratory sound. The moment, however, the patient's mouth was shut, or he ceased to breathe with a narrowed glottis, the adventitious sound was cleared away, and the natural murmur emerged in its purity. These sounds are exactly analogous in their mode of production to the tracheal and bronchial sounds (air-rush through a channel), and if *they* are conveyed by the terminal lung elements to the thoracic parietes, *à fortiori* would the bronchial sound, whose point of generation is so much nearer the seat of the respiratory murmur. I conceive it therefore impossible, in examining the causation of the respiratory murmur, and analysing the elements that contribute to it, to ignore the vibrations of which the entire bronchial system is the seat.

"But," it may be objected, "how unlike are these two sounds; how unlike are bronchial and vesicular breathing! The one harsh, tubular, and blowing; the other soft, faint, and breezy: how different their *timbre*, how different their pitch, how different their duration, and relation to inspiration and expiration!" All this I admit, and yet I think that the one enters into the composition of the other, that it furnishes a part of the basis or raw material of the sound, although so entirely modified as not to be recognisable as the same.

The modifying circumstances which, in my opinion, transform the sound we hear over a large bronchus into that which we hear in health, at every inspiration, over every part of the chest's surface are:—

1. The reduction of the calibre of the tubes.
2. The repeated change of direction of the tubes.
3. The softening effect of the intervening film of lung.
4. The multitude of sources of sound.
5. The influence of convection in restricting the sound to inspiration.

If it can be shown that these agencies would necessarily so modify the sound as to convert the bronchial into the vesicular, it would suffice to remove the objections of those who may see in their extreme unlikeness a reason for believing that the one cannot have anything to do with the other. Let me then say a few words in explanation of how these agencies would act.

1. *Reduction of calibre.*—Sound can only be conducted unchanged as long as the conductor is uniform; when the conductor is changed, a proportionate change takes place in the sound conducted, especially if the conductor is one possessing special acoustic properties, and these are involved in the change. The conductor that, according to my theory, conducts the bronchial breath-sound to the superficies of the lung, and delivers it there as a co-efficient of the respiratory murmur, is the ramifying column of air continuous from the large bronchi to the sub-pleural air-cells. This column of air is undergoing continuous diminution, till from a diameter of $\frac{3}{4}$ of an inch, that of the windpipe, it reaches a diameter of $\frac{1}{100}$th of an inch, that, namely, of the ultimate bronchules. Each bronchial ramification that it passes through in this decreasing series has its own pitch of vibration, and will vibrate in no other; the pitch must therefore be constantly undergoing change, not being the same for the length of an inch; and in this way a good deal of what physicists call *interference* must take place, and a great consequent destruction of vibrations and loss of sound. Every day's observation, independent of theory, tells us how different is the pitch of bronchial breathing, according to the size of the tube listened to. How different, for instance, is the bronchial breathing heard over the roots of the lungs, where the largest bronchi are, from that heard at the inferior border of the lung, when a fringe of condensation in that situation furnishes bronchial breathing of the smallest kind; the one is hollow, resonant, and large—the other small, flat, and open; the one with a tubular character, and the sound of *oo*—the other with but little tubular character, and the sound of *ah*, pronounced with the mouth wide open and the soft palate dropped a little on the root of the tongue. Now, carry this modification a little further, to the smallest tubes of all, and who shall say that the sound shall not be so changed as to reach the pitch, feebleness, and non-tubularity of the respiration-sound?

2. *Change of direction.*—But the tubes, and therefore the vibrating columns of air, are constantly, at each ramification, undergoing change of direction, and therefore reduction of sound; for sound, like light, cannot be bent out of its course without loss. This, too, would largely contribute to the enfeeblement of the bronchial sound ere it reached the terminal bronchules.

3. *The softening effect of the intervening lung-parenchyma.* — In healthy lung we never hear bronchial breathing (barring, of course, the upper sternal and interscapular sound), and when it *is* brought within the ken of the ear, it is so by some solid conductor—tubercle, or hepatized lung, for example. Not so with the respiratory murmur: *that* is so superficial, that the ear reaches it without the intervention of any solid conductor; and that the ear does so reach it, shows that the lung is not a perfect isolator of sound—that it has a certain amount of conducting power. For if it were an absolute non-conductor, the slightest removal below the pleural surface would suffice to render any

sound inaudible. The effect of all imperfect isolators of sound is first to muffle and deaden the sound, and then to shut it off altogether, according to the thickness of the isolating medium. This is exactly what the lung does. Before, therefore, we reach, in passing into the lung, a point at which sound is altogether inaudible, we shall pass through a stratum in which it will be becoming increasingly indistinct. This sub-pleural stratum of lung-tissue, bounded externally by the pleura, and internally by the plane at which all sound vanishes, is the immediate seat of the respiratory murmur, and the sound it yields must necessarily be softened and muffled by the damping influence of this thin stratum of lung.

4. *The multitude of sources of sound.*—This I am sure has a great deal to do with the soft, murmurous, rustling character of the sound, and with the strong contrast it presents to the single souffle of a bronchus.

5. *The influence of convection in restricting the sound to inspiration.*—When the intervention of a good conductor, a tubercle or hepatized lung, between the parietes of the chest and a bronchus, brings to the surface what is taking place in the tube, we hear a double blowing sound, louder in general at expiration than inspiration; but when we listen over healthy lung, we hear that the respiratory murmur is either confined to inspiration, or faintly audible at expiration. If, then, this murmur is in any degree furnished by the bronchial vibrations, how is the expiratory element destroyed while the inspiratory is retained? By convection, and thus. In inspiration, the current of air sets towards the surface, and the bronchial vibrations are carried towards the ear of the listener; moreover, the flow of the air surfaceward favours the propagation of the vibrations *by conduction* in this direction; whereas in expiration the air is rushing *from* the ear of the auscultator, carrying with it the vibrations of which it is the seat, and offering an opposing force to the conduction of those vibrations towards the surface. In expiration, some of the air within the range of the ear would be transferred beyond it, and carry its vibrations with it; in inspiration, that beyond the reach of the ear would pass the frontier, and its vibrations become audible. But I believe the influence of convection depends more upon its power of antagonizing or favouring conduction, than upon its actual transfer of the vibrating air, just as a distant ring of bells will at one moment be loud, and at the next inaudible, according to the direction of the wind, though, from the rapidity with which sound is conducted, the actual movement of the air can have little to do with its conveyance.* Dr. Walshe assigns to convection a share in giving predominance to the inspiratory sound, but he also assigns this result in part to "the greater resistance of the textures during inspiration," an opinion in which I cannot

* The influence of convection in determining the direction of sound is very well shown in heart-disease. In obstructive aortic disease the sound will be conveyed along the arteries, in the direction in which the blood is flowing, and inaudible over the ventricle, while, if the lesion is regurgitant, the sound, though generated at the same orifice, will be inaudible at the arteries, but conveyed back in the direction of the ventricle by the reflux of the vibrating blood. Indeed, I have sometimes heard a regurgitant aortic *bruit* clearer over the ventricle than over the orifice where it was generated.

coincide. I cannot but conceive that the air must experience *less* resistance in inspiration than expiration, for it is by the expansion of the lung and the removal of the pressure on the air, that an additional volume of air is drawn in, and it is by the resilient collapse of the lung, and the pressure it exercises on the contained air, that the expired volume is driven forth.

But is there no other cause for the relative loudness of inspiration? I think there is; and its consideration will bring me to the conclusion of this part of my subject.

In inspiration, the air is rushing from trunk to branches, and is being constantly thrown off in a new direction by the channel it is entering. In obedience to the universal law of motion, the tendency of the air rushing into the air-passages at inspiration is to move in a straight line; but it is continually deflected from the straight line by the change of direction of the tubes into which it is impelled. And what is it that so deflects it? Manifestly that side of the divergent branch it is entering on which it would impinge if it were continued in a straight line, and on which it really *does* impinge and by which it is deflected. Thus, when a main trunk divides into two branches, the central stream of air is impelled against the central spur or fork which forms the *point de depart* of the two tubes. This splits it and throws it off into two branches, part rushing into the one, and part into the other, just as the promontory of an island in the mid-stream of a river divides the water into two channels.* It is evident that here there is a more forcible impact and a more intense friction of air against the tube-wall than elsewhere. This will be more clearly understood by reference to the accompanying diagram. Let the two dotted lines, x x, represent the mid-stream of air rushing down a considerable bronchus, dividing into two branches at a; it will impinge on the angle a, and be thrown off by its sides, and assuming a new direction, pass down the secondary bronchi accompanied by the outer portions of air which have not impinged at a, represented by the dotted lines z z: the portion of air represented by these lines, x and z, will constitute the axial column of air of the secondary bronchi, and impinge on the angles b b, and be by them split and thrown off into the tertiary bronchi, whose direction they will thus have acquired; and so on. This will be repeated over and over again at every branching, and thus the

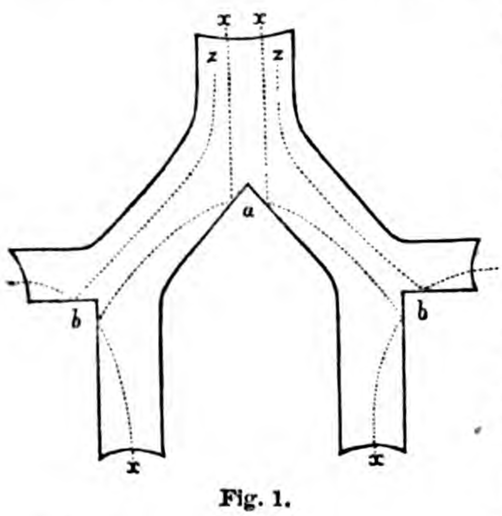

Fig. 1.

* Dr. Quain has happily illustrated the effect of a thin edge in dividing a stream of air, and the sound produced thereby, by passing the edge of a piece of paper across a stream of air blown from the mouth.

ingress of air into the bronchial system is a constant succession of impacts of air against the spurs or angles of tube-wall at which the tubes divaricate.

In expiration the case is very different. There the stream coming from one branch is met by the stream coming from another, and the two, reacting one upon the other, and exercising a mutual lateral pressure, pass on in an intermediate direction, the resultant of the former two; and the current tending constantly to mid-stream, the wall is relieved as much as possible from pressure or friction. This will be at once understood by reference to the accompanying diagram.

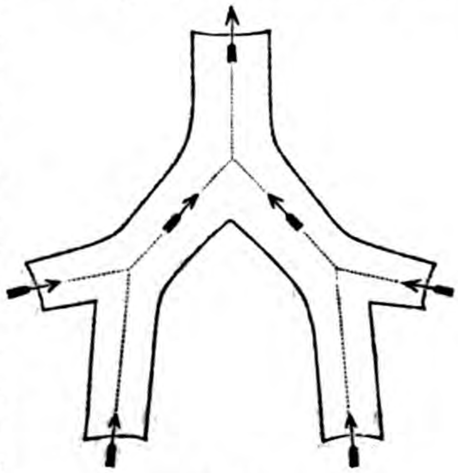

Fig. 2.

Thus, in inspiration, the stream is deflected by the resistance of the tube-wall to the moving column of air; in expiration, by the mutual lateral pressure of two columns of air moving in the same direction. There can be no doubt as to which would produce the greatest amount of friction sound.

But, it may be urged, this principle would operate equally in the largest and the smallest tubes, wherever the same principle of branching obtains; it would therefore not only be a cause tending to give predominance to inspiration in the natural surface breath-sound, but would make inspiration louder than expiration in bronchial breathing; whereas the reverse has just now been stated to be the case.

Both these facts are true; I admit them both, inconsistent as they appear. The simplest physics prove the one, and I would appeal to the experience of all observant auscultators in support of the other—whether bronchial breathing is not commonly—I do not say always—louder and more blowing at expiration than inspiration. The only way in which these two incompatible facts can be explained, is by supposing that there is some other principle in operation, some *tertium quid*, tending to augment the expiratory sound, and which more than countervails the extra friction involved by the splitting of the streams of air at inspiration.

Such a principle I have long puzzled myself in search of, for I have for years noticed the relative loudness of expiration in bronchial breathing, though I have felt myself quite unable to explain it. But the other day, what I fancy must be its true explanation occurred to my mind, and though not relevant to my special object in this paper, I would suggest it to those interested in this subject for their consideration. It hinges entirely on this single anatomical fact—that the law of bronchial branching involves a continual augmentation of the aggregate volume of the tubes—that the sum of the calibre of the branches is greater than that of the trunk from which they spring.

Now, if we fuse the branches, and consider them as forming a single tube, it is evident that the shape of such a tube would be that of a very elongated funnel, or cone, the narrow end of the funnel being at the windpipe, and the wide end at the periphery of the bronchial system.

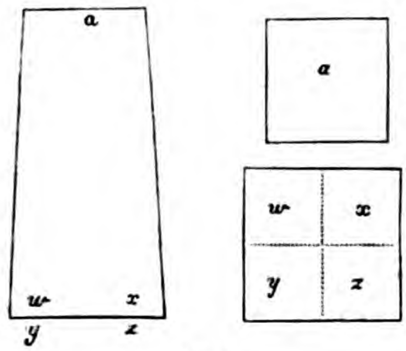

Fig. 3.

This is shown by the accompanying diagram (Fig. 3), where *a* represents the section of a single bronchus, and *w*, *x*, *y*, *z*, that of four in which its branching would result: it is manifest that if fused and viewed in profile, they would form a truncated cone. At inspiration, therefore, the air would be flowing from the narrow to the wide end of the tube, and at expiration from the wide end to the narrow. The effect of this on the pressure exercised by the air on the tube walls is evident. Inspiration and expiration are like a river whose stream first widens and then contracts again. In expiration, the air is forced into what is in effect a constantly narrowing channel. As the air struggles through its straightened passage it exercises increased lateral pressure upon its walls, which react in proportion; in fact, it is this increased reaction of the walls which, together with the sustained *vis a tergo*, imparts the increased velocity to the stream of air, and makes its movement so much more rapid in the larger bronchial tubes and windpipe than in the smaller ones. In inspiration the state of things is of course exactly reversed. In the one, friction is greater than that of fluid flowing through a cylinder, in the other, it is less.

The principle *must* obtain, and of the fact endeavoured to be explained by it I have no doubt. I will leave others to determine the adequacy of the one for the explanation of the other.

There is yet one more point in which the respiratory murmur differs from bronchial breathing; it is in the relative length and rhythm of expiration and inspiration. In the respiratory murmur (when expiration *is* audible) the inspiratory sound is at least three times as long as the expiratory, and expiration succeeds inspiration so immediately that the two sounds may almost be said to be continuous; in bronchial breathing the expiratory is nearly as long as the inspiratory sound (the larger the bronchus the more equal are they), and there is an appreciable interval between them. This difference depends on these two facts—that air is an elastic fluid, and that the air in the ultimate lung structure is that nearest to, and first acted on by, the motive power—the moving thoracic parietes.

The moment the thoracic parietes, costal and diaphragmatic, cease to expand, and they and the lung are left to their tendency to collapse, expiration commences and its attendant sound, the air at once passing back from the recesses of the lung it has just entered. Not so the air in the large bronchi; that is in full inward rush when the parietes are brought to a stand and re-act on their contents. If air were perfectly inelastic, that in the large bronchi would be as instantly brought to a stand, and as instantly recoil; but being elastic, it undergoes a certain compression, the pressure re-acting on the in-rushing air, first bringing it to a stop and then driving it forth. Just at the turn of inspiration, then, the air is for an instant in a state of compression, and it is just at this moment, when the compression is turning the air in the bronchial tubes, that the pause between the inspiratory and expiratory bronchial sound occurs. The same principle that produces this pause in the smaller bronchi, of course prolongs it in the larger. Again, the sudden compression of the air which the collapse of the lung produces, is slowly dealt out in the bronchial tubes till an equilibrium of pressure is reached, and then it ceases. Thus we see that the movement of the air in the bronchial tubes is always rather behind that in the lung parenchyma, and that the "respiratory" rhythm is converted into the "bronchial" rhythm by inserting a pause between inspiration and expiration, lengthening the expiration and shortening the post-expiratory rest.

I think I have said enough to show that the distant bronchial sound, conducted and convected to the surface by the ramifying columns of air that have a sub-pleural distribution, may contribute to the inspiration-sound of ordinary breathing; at any rate, I have stated and explained my belief, and shown that the differences of character of bronchial and vesicular breathing is no argument against the latter owing something of its raw material of sound to the former, inasmuch as those differences are just such as the modifying influences in operation would produce. If my views are accepted, an element of sound will have been added to the respiratory murmur not before indicated, and some of the difficulties of the subject will, I hope, have been cleared away.

I conclude, then—

1. That the air-cells are structurally incapable of producing a respiratory murmur by their slight dilatation.

2. That the respiratory murmur is, immediately and essentially, a fine-tube sound.

3. That the lobular passages and ultimate bronchial radicles are probably its immediate seat.

4. That while the lung parenchyma, from its heterogeneous constitution, completely muffles all deep-seated sound, the unbroken column of air in the bronchial tree is an excellent conductor; that thus the respiratory murmur has a double cause, and is in part the result of air- and wall-friction at the spot, and in part the bronchial sound of the larger tubes (although essentially modified) conducted by the unbroken column of air to the ultimate bronchial twigs.

5. That the restriction of the sound to inspiration (excess of inspiratory over expiratory sound, *authors*) depends mainly on convection, but probably also in part on the great amount of air and wall friction produced by the impact of the air at the points where the tubes divaricate.

(To be continued.)

PART FOURTH.

Chronicle of Medical Science

(CHIEFLY FOREIGN AND CONTEMPORARY).

HALF-YEARLY REPORT ON MICROLOGY.

By John W. Ogle, M.A., M.D. Oxon, F.R.C.P.
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Part I.—Physiological Micrology.

NERVOUS SYSTEM.

On the Intimate Structure of the Olfactory Lobe in Mammalia. By Ph. Owsjannikow.*—The preparation for exhibiting the character of the olfactory lobe is described as being effected with some difficulty, and young animals must be selected, owing to the bones being softer and more easily removed. When removed, the part is to be preserved in a solution of chromic acid or bichromate of potash. Fine longitudinal and transverse slices are to be made and rendered more transparent by glycerin, nitric or sulphuric acid. In the middle of a transverse section an opening can then be seen, the cavity of the lobe corresponding in dimensions to that of the entire lobe, and lined, in the case of all mammals, fish, and the amphibia, with cylindrical epithelium; the epithelial cells being generally of a funnel shape. In the case of the pig the relation of the fine extremity of the epithelial cell to the substratum of the lobe is best observed, each cell appearing as if in continuous connexion by its small end with a fibre of the breadth of 0·001 millimetre, and at the point of union a longish swelling is visible, as if indeed one tubule were pushed into the other. The fibres appear further to pass into the areolar-tissue corpuscles of from 0·002 to 0·004 millimetre in diameter. In the frog the same thing may be seen. The epithelial cells are not ciliated. On passing in our examination from the epithelium inward, we find a layer, consisting entirely of areolar tissue and fine bloodvessels without nerve elements, of the same nature as that surrounding the central canal of the spinal cord. The layer next in order contains principally broad nerve-fibres, nearly parallel with the axis of the cavity, and seen in longitudinal sections to be arranged in strong bundles. Bloodvessels at this part are very scanty, and nerve-cells absent. From the outer side of this layer, small bundles separate, and in large animals are easily, on longitudinal section, to be seen with the naked eye; and from these bundles fibres proceed on various sides, become finer, and unite with small nerve-cells of from 0·001 to 0·003 millimetre long, and from 0·004 to 0·006 broad, which form the third layer. These cells contain a clear nucleus, are of a roundish, sometimes rather elongated form, and possess four or five very fine processes; that is, each cell is connected with four or five nerve-fibres. This layer, in fresh preparations, is of a whitish-grey colour, and the nerve-fibres in it are much finer than in the preceding layer. These three layers are very closely united together, but the fourth layer is very easily separated, owing to the presence of a large

* Reichert and Du Bois-Reymond's Archiv, Heft iv. p. 469. 1860.

number of bloodvessels, especially at the boundary line of the third layer; this may well be seen by colouring with carmine, or by ligaturing the jugular veins of the animal whilst alive, so as to produce an injection of the fine vessels. The nerve-fibres of the third layer for the most part extend beyond the boundary line of the fourth layer, and become finer, but yet possess their medullary substance and doubled contour. In the fourth layer they unite with small nerve-cells which are chiefly bi-polar, and generally 0·006 millimetre long; and after a time the fibres group into more or less strong bundles, which on transverse section are seen as round spots of various sizes. These bundles become surrounded and penetrated by very fine bloodvessels, and among them bi-polar nerve-cells are met with. Sometimes the bundles on section have a clavate form. Moreover, in some large animals single larger cells are visible, though rarely, along with a number of smaller ones, possessing a rather large number of processes, which may be seen passing into fine nerve-fibres.

The author warns against mistaking transverse sections of bloodvessels for nerve-cells, especially when their walls have become granular from the use of glycerin, and states that the parietes of the vessels are coloured by carmine in like manner as the nerve-cells, so that this colouring is a deceptive means of distinguishing nerve-cells; blood-cells, and also epithelial cells, areolar-tissue corpuscles, &c., being all coloured by carmine. It is only the nerves that possess medullary substance which remain uncoloured; the cylinder-axis, if uncovered, becomes coloured. After that the fibres are united with cells and grouped into bundles, they lie so close together in many cases that they appear to have a common investment.

Most observers look upon the olfactory nerves as destitute of medulla. Owsjannikow considers that that which covers the cylinder-axis is to be viewed as the nerve-medulla. This however becomes lost eventually, and appears to form nothing essential to the function of the nerve.

The olfactory nerve-fibres are distinguishable from other nerve-fibres, being bright, riband like, and adhering to each other. They are obviously connected with the cells and dark-rimmed fibres of the olfactory lobe.

In order to trace the olfactory nerves in the nasal mucous membrane, sections in young animals through the mucous membrane and adjoining cartilage, prepared by chromic acid, are to be made; or the mucous membrane of the opened nasal fossæ which has been prepared by chromic acid, may be treated with nitric acid and boiled, and so rendered transparent. In this way the bundles of the olfactory nerve may very easily be demonstrated, and the further they are pursued from the point of entrance the clearer they become. At last the fibres of 0·006 millimetre broad are seen breaking up into fine bright ones free from varicosities, and can better be traced on tearing up the structure with needles. They then are seen to terminate as follows. Some are found apparently to unite with the long epithelial cells found on the surface, which by many are rightly esteemed olfactory cells, and are distinguished from others by their length and small size. The cilia of these cells, which are very pale, short, and straight, can only be seen in moisture and in fresh preparations. The proper epithelial cells of the nasal mucous membrane have no very regular rod-like form, possess a nucleus, and have long, strong, and curved cilia. Other nerve-fibres after union with cells very like the bi-polar nerve-cells, pass between the epithelial cells. Here, according to Ecker and others, they end by free terminations, but Owsjannikow has often seen them unite with small funnel-shaped cells which possessed slender straight cilia: and where the cells were found absent, they undoubtedly had been removed. Moreover a free termination of the nerve-fibres of an organ of sense is inconceivable, in the present state of our knowledge.

Our author could not find any commissural fibres betwixt the two olfactory lobes, but on following the fibres of the second layer backwards, they are seen going towards the brain, and losing themselves in small nerve-cells, thus exhibiting a direct union between the brain and the olfactory cells.

On the Termination of Nerves in the Voluntary Muscles of Man and Vertebrate Animals. By Lionel Beale.*—Beale's researches have already been briefly referred to in our Half-Yearly Physiological Report in the January number, p. 235. We propose now to consider them more in detail. The paper, we believe, will be published with the drawings in the forthcoming number of the 'Philosophical Transactions.'

Beale has been led to conclude from recent observations, that every elementary fibre of voluntary muscle is abundantly supplied with nerves, which form a network, and lie upon the surface of the sarcolemma, with which membrane, at least in many cases, the delicate fibres seem to be incorporated. They do not penetrate through the sarcolemma. It is stated nerves never terminate in points, as is now generally supposed, neither can a single elementary fibre of voluntary muscle be found which is not abundantly supplied with nerve-fibres. The elementary fibres of the tongue and diaphragm of the white mouse are nearly covered with nerve-fibres and capillaries; the *sarcolemma* indeed appears to be principally composed of these structures. The muscular fibres of mammalia and birds receive a much larger supply of nerve-fibres than those of fishes and reptiles, but in insects the most wonderful structure exists on the surface of the muscle. In some muscles the entire surface is covered by some long, spindle-shaped, and very large nerve vesicles, which can be shown to be continuous with the nerves. This beautiful structure is completely destroyed very soon after death, and not a trace can be discovered if a little water comes into contact with the muscle.

In mammalia, the nerves are seen to run for a long distance with the arteries, and their ultimate divisions come into very close relation with the capillary vessels.

As the nerve-trunks approach their distribution, each individual fibre divides and subdivides, and the fibres resulting from this subdivision often pursue a very long and complicated course, running for some distance parallel with other fibres derived from different trunks, but it is not possible to follow any one individual fibre for any great distance.

Fine trunks, composed of from three to seven or eight fibres, can often be seen traversing the muscle. The fibres pursue different directions; some dip down between the elementary muscular fibres, some pass over the surface, and form with others, from a different source, small compound trunks, while others may be traced onwards for some distance; the individual fibres which gradually separate from each other being distributed to different parts, in succession, of several different elementary muscular fibres. When the finest nerve-fibres can be seen passing round the elementary muscular fibres, they clearly consist of very delicate flattened bands.

Of the Oval Bodies or Nuclei.—Connected with all nerves in every part of the body, *sensitive*, *motor*, *vascular*, and probably in all animals, are little oval bodies or nuclei, which are the organs by which the nerves are brought into the closest relation with other textures, and from them new branches are developed. The nerves multiply at their distribution by the division of these little bodies, and upon them their action and, in all probability, the manifestation of the nervous phenomena depend. A great number of these little bodies is associated with perfection of nervous actions, and *vicé versâ*. They are found very freely connected with the vascular nerves, and are abundant on those nerves near the ganglia from which they proceed, and in the ganglia themselves. These bodies, with the nuclei of capillary vessels and those of

* Proceedings of the Royal Society, No. 40, vol. x. p. 519.

fat vesicles, and probably other structures, with peculiar cells, which alone deserve the name, have been included under the term "areolar-tissue corpuscles" (*Bindegewebe-Körperchen*). As specimens are usually prepared, it is quite impossible to distinguish these structures from each other. Beale believes that the gelatinous fibres, or fibres of Remak, are, after all, real nerve-fibres, and not a peculiar modification of fibrous tissue, as is now generally believed.

The nerves and vessels, and with them, of course, the oval bodies, may be stripped off from the elementary muscular fibre.

Of the manner in which Nerves terminate.—The fibres connecting the oval bodies or nuclei form with them a network, the branches of which are, of course, continuous with the subdivisions of the nerve-fibres. The arrangement of the network, and especially the number and proximity of the nuclei to each other, differs materially in different localities. On sentient surfaces the meshes are very small and the nuclei close together; but from the complexity and great number of the fibres; from the fact that many fibres which appear to be single can be resolved into three or four individual fibres, and from the circumstance of the network being imbedded in most cases in the midst of fibrous tissue, it is very difficult to describe its exact relations and disposition. However, from the connexions of this network with the nerve-fibres, it would seem to follow that an impression made upon a given portion of a sentient surface might be transmitted to the nervous centre by contiguous fibres, as well as by the one which would form, so to say, the shortest route; and it is possible that impulses to motion may be conveyed to muscular fibres by a more or less circuitous path, as well as by a direct one.

Of the so-called Tubular Membrane.—This is a transparent structure in which the nerve-fibres are imbedded. It cannot strictly be called a membrane, because in many cases several fibres are imbedded in it, and often it is much thicker than the fibres it contains. By examination with high powers (700 diameters), many fibres which appear to be single when seen by lower powers, can be resolved into three or more, all enclosed in the same transparent tissue. As the nerve-fibres approach their distribution, this transparent structure becomes much spread out. It is intimately connected with nerve-fibres and capillaries, and with them forms a delicate expansion over the muscular fibres, and in other parts. Delicate fibres also, in connexion with the nerves and capillaries, may be observed in it. In some cases this expansion seems to be incorporated with the sarcolemma, and it is probable that in certain instances it is really the structure which has received that name.

Axis Cylinder and White Substance.—Beale has been led to conclude that, in consequence of the free division of the axis cylinder and white substance near the point of distribution of the nerve, a single fibre in the trunk of a nerve may carry impressions to or from a much larger extent of surface than is generally supposed. The white substance which surrounds the axis cylinder gradually diminishes, until, in the finer ramifications, it is impossible to say that a fibre consists of an axis cylinder and white substance; for its general appearance and refractive power are the same in every part, except where the nuclei are situated. Beale considers that the definite characters of the axis cylinder and white substance in the trunks of the nerve may be due to the gradual growth and altered relations of the fibres which occur during the development of the entire organism. In the ultimate ramifications the whole fibre seems to consist of a very transparent and perhaps delicately granular substance, but no *tubular membrane, medullary sheath, or axis cylinder*, can be demonstrated as distinct structures.

Of the Formation of New Fibres.—In connexion with the terminal ramifications, new fibres are being continually developed by the division of the nuclei, and old ones undergo removal. The remains of the latter may, however, be seen in the form of very delicate fibres, in connexion with active nerve fibres. Beale regards much of the so-called connective tissue between the elementary fibres of muscle and in some other situations, as of this nature,—as the remains of structures whose period of functional activity was past, and which have been removed, with the exception of this small quantity of insoluble material.

The method of preparing the specimens is then briefly described. Observations were conducted principally on white mice, which were injected with Prussian blue fluid immediately after death.* Beale concludes his paper, which will be published at length in the 'Philosophical Transactions,' with the following summary of the most important facts elucidated in the inquiry:—

"1. That nerve-fibres in muscle and in many other tissues, if not in all, may be traced into, and are directly continuous with, a network formed of oval nuclei and intermediate fibres.

"2. That the organs by which nerves are brought into relations with other textures, and the agents concerned in the development of nerves and the formation of new fibres, are the little oval bodies or nuclei which are present in considerable number in the terminal ramifications of all nerves. A great number of these bodies is associated with exalted nervous action; while, when they are sparingly found, we may infer that the nervous phenomena are only imperfectly manifested.

"3. That every elementary fibre of striped muscle is abundantly supplied with nerves, and that the fibres of some muscles receive a much larger supply than others.

"4. That the nerves lie with the capillaries, external to, but in close contact with the sarcolemma. They often cross the muscular fibre at right angles, so that one nerve-fibre may influence a great number of elementary muscular fibres. There is no evidence of their penetrating into the interior of the fibre."

The paper is illustrated with drawings, most of them magnified 700 diameters.

On the Termination of Nerves at their Periphery and in different Organs. By Jacobowitch.†—This author, from his observations, comes to the following conclusions:

1. That each nerve, of whatever nature, takes origin from a nervous cell in the central nervous organs, and terminates at the periphery or in the interior of an organ:—
   (*a*) Either in a nerve-cell, and, in the case of nerves of sense, in the nucleus itself;
   (*b*) or in the mass of a cell (*dans la masse d'une cellule*) in the interior of organs in the case of the ganglionic nerves;
   (*c*) or finally, by forming a nervous capillary network where the anatomical differences disappear, the axis cylinders passing the one into the other.
2. That the nervous system, central as well as peripheric, forms a whole which, like the sanguineous system, exists in every part of the organism, penetrating across different parts, and arriving at the ultimate elements without at all becoming lost in a vague or confused manner.
3. That the nervous elements, the cellules as well as the axis-cylinders, are always in course of development in the central organs as well as at the periphery.
4. That the part played by the cellules varies, for they either preside over

* The Microscope in its Application to Practical Medicine, p. 63

† In a Memoir communicated to the Académie des Sciences, May 7th, 1860; see Arch. Générales de Médecine, June, 1860, p. 760.

special functions, as in the case of the organs of sense, or subserve the preservation of the organs themselves, as in the case of glandular and nervous organs; the physiological function (properly speaking) of the organs arising from the connexion of these nervous cellules with the central nervous system.

5. That if the anatomical difference disappears in the capillary nervous network, owing to the axis cylinders being confused, the physiological difference remains.

On the presumed Nerve-fibre Terminal Plexus of the Intestine.—Reichert communicates some observations* by Hoyer of Warsaw, upon the above-mentioned structures, as described by Billroth.† It seems that Hoyer, whilst at Breslau, had shown to him a preparation made by Billroth himself, in which he at once recognised the apparently anastomosing nervous network as an artificial production. In the course of his investigations on the terminations of nerves, he made numerous different sections of hardened intestinal mucous membrane, and came to the conclusion that the apparent nerve-fibre plexus, with its enlargements and nucleus-like structures, consists of altered capillaries which have greatly contracted and become filled with coagulated blood-plasma, the blood-corpuscles becoming heaped up at the knotted parts. These altered capillaries are easily seen connected with the finest vessels, which, owing no doubt to the different character of their parietes, undergo these changes much less than the capillaries.

As a postscript to the above communication, Reichert adds that he has himself actually injected the nervous terminal plexus, with its ganglion cells, described as such by Billroth.

VASCULAR SYSTEM.

On the Development of Bloodvessels. By E. Rindfleisch.‡—The transparent border of the tadpole is best used for observation, and the most recent layer of connective-substance between the free margin and the outermost capillary loops is found to be that in which the new vascular formation proceeds. In certain places, blind projections take place which gradually grow, form an arch, and return to the original vessel, constituting a new capillary loop. But these blind terminations are found to pass into fine threads, and around are seen lying numerous stellate connective-tissue corpuscles. If the fine threads are in connexion with the latter, they may be looked upon as processes from them, and the capillary network as a metamorphosis of cellular elements; but if such a connexion does not exist, then the thread-like appendages must be looked upon as special formations preliminary to the future vessels. The author addressed himself with proper precautions to ascertain which of these opposite views was the right one, and came to the conclusion that the vascular outgrowths neither seek nor avoid the cellular elements. The same animal was examined several times in the course of two or three days. On one occasion, these vascular growths were seen to pass without any other connexion, so as touch the other vessels, to widen and acquire nuclei in their walls. Here and there slight zig-zag curves were seen at their termination. Some were also seen partly in evident connexion with a stellate cell, and in some cases there was undoubtedly extension of the cell-processes to the wall of a capillary vessel; then the cell-bodies were exceedingly distended, all processes being clearly visible. In the next observation, the contents appeared to be assimi-

* Reichert and Du Bois-Reymond's Archiv, 1860, Heft iv. p. 543.

† See our Report for Oct. 1860, p. 510.

‡ Dissert. Inaug. Berlin 1859: quoted from Virchow's Archiv, Band xx. Hefte 1, 2, p. 213.

lated, and a stronger process than the thread-like one was seen. In the case of two cells so concerned, if the direction of the process be in the region of a neighbouring cell, the same will participate in the formation of the vascular wall; but if it be external to the adjoining cell territories, then the vascular process takes an apparently isolated course.

MUSCULAR SYSTEM.

On the Presence of Muscular Fibres in the Parietes of the Pulmonary Vesicles. By J. Moleschott.*—As is well known, the results of observations concerning the minute structure of the pulmonary cells have differed extremely and attracted much attention. In 1845, Moleschott had asserted that their walls contained muscular fibres. Acknowledgment of this was refused by Rossignol, Adriani, Kölliker, Harting, Donders, Reichert, &c.; but Gerlach admitted the fact of their presence, and Moleschott, fortified by this opinion, addressed himself to re-verify what he had previously stated. Accordingly, the lungs of the pig, of the ox, and of man were examined, adult animals being found the best. The process is as follows. Portions are to be cut off and macerated in tolerably concentrated acetic acid for some months, then macerated for twenty-four hours in distilled water, and examined, moistened with very dilute acetic acid. In this way the walls of the pulmonary vesicles may be seen to contain smooth nucleated muscular fibres of a somewhat yellowish hue; or they may be seen by inflating and drying the lung, and then making thin sections, which must be left for three or four hours in very dilute acetic acid; and for more complete isolation of the fibres, they should be macerated for half-an-hour in a weak solution of potash. The first method of preparation best suits the lung of man and of the ox; the second suits best the pig's lung. In the case of the ox lung, fascicles of two, or four, or more muscular fibres in juxtaposition may be seen, but in man, two fibres are but seldom seen united. Moleschott gives minutely the form, situation, and chemical reactions of the fibres, as well as of their nuclei, and discriminates between them and other elements, such as fusiform epithelium of small arteries and elastic-tissue cellules, with which they might be confused.

GLANDULAR SYSTEM.

On the Malpighian Corpuscles of the Spleen. By N. Kowalewsky.†—In his observations, the author made use of the spleens of the dog and cat, either in a fresh state, or hardened in an aqueous solution of sesquichloride of iron, as used by Führer and Billroth. According to him, the Malpighian bodies lie in the neighbourhood of tolerably large arterial and venous vessels. They project with their free surface, which is covered with the venous epithelium (the splenic fibres of earlier authors) into the cavities of the cavernous network; the meshes, consisting of areolar tissue, go between the epithelial cells with the Malpighian bodies, and become narrower within them. On each Malpighian corpuscle, beneath the venous epithelium, runs a small arterial vessel, which becomes lost on its surface (in a tuft-like manner) in a great number of capillary twigs, many of which allow but a single blood corpuscle to pass. The capillary vessels pass from the surface into the interior of the Malpighian corpuscles, and become larger within, owing to the concurrence of many twigs. After their union in a central vein, this passes out into the surrounding cavernous network to embouchure into a larger vein. Sometimes, but rarely, smaller isolated veins also pass from these bodies in addition to the central vein. The large central vessels are evidently veins, not only from their emptying into larger veins, but also

* Gaz. Hébdom., tome vii. No. 32: quoted from the Wiener Med. Wochenschrift.

† Virchow's Archiv, Band xx. Hefte 1, 2, p. 203.

from their walls being thinner than those of arteries, and from their having the characteristic epithelial cells of splenic veins on their inner surface. The space between the meshes of the cavernous network in the splenic corpuscles is filled with white blood-corpuscles, which only appear to differ from those in the body generally by their being smaller, and having a somewhat clearer outline. This may be from the deficiency of fluid. On ligaturing the splenic artery of a dog, and killing it some days afterwards, it will be found that the central vein of each splenic corpuscle may be seen with the naked eye as a red point on a white ground, and the corpuscles appear enlarged. Moreover, the central vein may be seen very clearly in the spleen in cases wherein some time previous to death the sympathetic nerve addressed to the spleen has been divided.

GENERATIVE SYSTEM.

On the Development of the Follicles of the Ovaries and of the Ovum in the Mammalia. By Otto Spiegelberg.*—The following observations were conducted on the human embryo, as well as on the embryonic and newly-born rabbits, cats, dogs, and goats. The author was unable to recognise the tubes in which the ovarian follicles should be found, described by Valentin. At the time in which the sexual differentiation begins to proceed, and the male germ-glands possess seminal canals, the ovary is seen to be composed of large bright cells which, by reason of very delicate areolar-tissue-partitions and bloodvessels extending from the hilus to the periphery, are arranged into irregularly-shaped heaps. These germ-cells Spiegelberg terms primary ovarian cells, and their large vesicular nucleated nucleus he designates "germs." During the growth of the germ-cell the nucleus divides, at first into two, later on to a greater extent, and the wall of the mother-cell so originating appears to be doubly contoured. These mother-cells are the primordial follicles, and are very like the spermatic cells. As soon as they are quite filled with germ, one of them, generally the central one, greatly enlarges; and besides the nucleus, a delicate investment and a granular contents may be seen within. The germs surrounding also increase meanwhile and form into cells, and become covered by an investment. The first of these cells is the ovum, the last forms the element of the "membrana granulosa." Thus, according to our author, is the formation of the ovum in the higher animals assimilated to that observed by Meissner in the ascarides, &c. Soon after birth the formation of germ-cells into primordial follicles is complete, the latter being, later in life, no more formed anew. In newly-born animals the younger follicles are always found at the periphery of the ovary, the older at the deeper parts. By the growth of areolar tissue and vessels the mother-cells become isolated; very rarely only do two or more become fused into each other.

PART II.—PATHOLOGICAL MICROLOGY.

GENERAL MORBID GROWTHS, TUMOURS, CYSTS, ETC.

Observations on Cystic Disease and Cancer. By Dr. Collis.†—The author details four cases: one of cystic disease of the breast with endogenous growth, showing mode of origin of the chronic mammary or adenoïd tumour; a second, cystic disease of breast complicated with cancer; a third, single encysted hæmorrhagic or fibrinous tumour in the popliteal space; and a fourth, multiple encysted fibrinous deposits in the testis.

In case the *first*, that of a female, dissection after removal during life showed

* Kon: Ges: der Wiss. zu Göttingen, 1860, Juli, No. 20; as quoted in Virchow's Archiv, Band xx. Hefte 1, 2, p. 210.

† Dublin Quarterly Journal of Medical Science, Nov. 1860, p. 361.

multitudes of small cysts of the size of pins' heads or small peas, a few as large as a hazel nut, one the size of a hen's egg, &c. Some contained sero-sanguineous fluid, some were full of solid matter. In the largest cyst existed a solid growth attached by a pedicle to the lining of the cyst, and projecting into the centre; in others the cavities were quite filled up in this way, and in some this endogenous growth had by its pressure produced an atheromatous state of the walls of the cavity, and the fragmentary remains of other cysts could be made out around some other of these endogenous growths. These substances proved to consist of a certain amount of common fibrous stroma binding together a mass of simple fibrine cells, which did not deviate from the undeveloped cells of plastic lymph. They were arranged in acini, attached to a pedicle, like minute cauliflower growths, but firm, and devoid of any creamy juice. The mode of increase was evidently like that of a tree, not only by interstitial deposit, but by additional branches. The resemblance to the genuine structure of the mammary gland was confined to the mode of arrangement: there were no ducts, no cavities in the lobules, and no epithelial lining. It was evident the cyst-growth was being superseded by the fibrinous. The history of the patient indicates no tendency to return of disease. The following cases are all in strong contrast with the above.

In the *second* case, that of a female, the tumour, removed during life, was composed of the tough fibrous tissue of the breast, inflamed and condensed by large oily cancer cells stained with melanotic pigment. The remainder of the breast was studded with small cysts of simple form; many of them communicated with the ducts of the gland; they were filled with sero-sanguineous fluid, and some contained minute secondary cysts full of fluid, with slightly scolloped edges, attached by a pedicle to the lining membrane. There was no solid endogenous growth; nor were there any cysts in the cancerous part of the gland. Hence it was to be concluded that the two diseases, though coincident, were in reality independent in origin and progress—the cyst originating in inflammatory action on the ducts of the gland, and the cancer having its seat in the interstices of its fibrous stroma.

In case *three*, that of a labouring man who received a blow in the lower part of the thigh, a tumour arose and filled the popliteal space. It was painless, firm, and rolling loosely in the areolar tissue of the ham. There was no œdema, and the glands of the part were not affected. On removal and dissection of the tumour, the centre was found to be occupied by a black semi-fluid coagulum; outside this was a layer of soft dark-coloured lymph, the result of a previous clot in process of organization; outside this was another and another layer less and less dark and more thin and firm, the outermost being a dense, firm envelope beset with numerous vessels. Altogether, the appearance was like that of a cured aneurysm; but no special connexion could be traced with artery or vein. The author describes an exactly similar tumour which he has since met with in the testis, which, it seems, was also examined by Mr. Paget, who found it to consist of small nuclei and cells differing little from lymph-cells, and exhibiting no appearence of cancer.

In case *four*, that of a labourer, aged forty, a tumour of the size of a cocoa-nut formed in the testicle after a blow; the glands were not affected, and the skin was non-adherent. After removal the patient quite recovered. On section, the growth showed a number of cysts filled with lymph, organized like ordinary coagulum. The cysts contrasted by their glistening whiteness with their pink-coloured contents and with the grey tubular substance of the testis. The coagula inside the cysts, on hardening with Goadby's solution, could be easily turned out of the cysts to which they had a minute vascular attachment. The cells found by the microscope as being contained were "smaller than the nuclei of cancer cells, and unable to resist the action of dilute acetic acid as the latter do."

The author then gives a tabular statement containing the marks which will enable a careful observer to distinguish the adenoïd, the simple cystic, and the hæmorrhagic fibrinous tumours from the encephaloïd and the scirrhous forms of cancer. He points out that in most cases of scirrhus, if a good light be thrown upon the skin, minute dimples or depressions will be seen over or near the tumour, owing to the shortening of subcutaneous bands which run towards the tumour. These are the forerunners of infiltration of the skin by cancerous matter, and are absent in pure encephaloïd and in the more innocent growths. Dr. Collis then speaks as follows of growths which are not cancer, but which equally with it have their origin in the lymph-cell: "After reading most of what has been written about them, and having seen a good many, I am satisfied that this whole class may, for practical purposes, be included in the following formula: '*The nearer in form and power of development that the constituent cells of a tumour are to the healthy lymph-cell, the more innocent is the tumour: the further removed, the more destructive.*' To expand this a little, and make it intelligible, we find the healthy lymph-cell small, circular, slightly granular, with a little nucleus, and developing into a fibre. Our simplest tumours are composed of cells, scarcely, if at all, to be distinguished from the above; and these white fibrous or desmoïd tumours are the most innocent possible growths, as a general rule. A stray exception may occur now and then to prove the rule. We then come to fibroïd, fibro-nucleated, recurrent fibroïd, fibro-plastic, fibrinous tumours, named according to the fancy of writers, who recognise alike their similarity to simple fibrous tumours, and their divergence from them. These are of variable malignancy; they are of as variable minute construction. Not only do their constituent cell-elements differ more or less in form from the primary lymph-cell, but they also differ in power of development. Some remain always as cells, and never develop into fibres; these are the most recurrent. Some make attempts at development, and hence the caudate cell of various form; some appear as nuclei, only without external cell-wall. Again, power of development into fibrous forms is quite different from active reproduction; generally it is not associated in the same cells. The recurrent tumours are masses of rapidly-produced cells, or nuclei, with no attempts at the formation of fibre.

"Cancer itself can be brought under the above law. It is no heterologous or parasitic formation. It is simply a monstrously abnormal plastic growth; its cells differ as widely as possible from the healthy type. In acute cases they are rapidly produced, make scarcely an attempt at development, and die off with rapidity; in scirrhus they are formed more slowly, and in much smaller numbers, live longer, and make some attempt at caudation, but they are still farther removed in form from the typical cell of healthy tissues.

"The more I think over the subject of morbid products, the more am I convinced that, in the above formula, we have the expression of a law that includes most of their phenomena. It will, slightly modified, apply not only to large classes of tumours, but also to tubercle and to pus. Tubercle is a lymph-cell, of low vitality, incapable of development into healthy fibre, dying after a short existence, and generally becoming a foreign body. Pus may be described in words almost identical—their material difference being one, probably, more of chemical constitution than of vital power—for both are possessed of almost a minimum of vitality. Tumours are composed of cells whose vital force is greater than pus or tubercle; and this vital power is rather spent in reproduction than in development, as in the healthy cell. The ordinary plastic cell goes through certain phases, dies, and is removed; its place is taken by a new cell, developed, probably, from the nucleus of its predecessor. The abnormal cell fails to arrive at perfection, often becomes a monstrosity, and has a tendency not only to reproduce itself from its nucleus, but to generate in neighbouring lymph organisms similar to itself. Thus the constituent cell of the tumour has a certain independent vitality, similar to that of the entire tumour; or rather the converse is true—the tumour is composed of cells of independent vitality, and hence it possesses the same form of life with the cell. And as the life of the cell is of a low type, so is that of the tumour. I cannot but think that many of the able minds which have been engaged in studying cancer in its minute anatomy, have failed to make an adequate impression upon practical surgery, because they have been led away to look on cancer as a thing quite different from any of the ordinary structures of the body, instead of a perverted form of a natural structure. And I am sure that their views, carried out to their legitimate conclusion, would lead us to despair of any remedy for cancer but the knife, or other agents of destruction. Whereas, if we regard the cancer-cell merely as a perverted lymph-cell, we shall never rest until remedies are found which will influence it to a more healthy type."

The author concludes his remarks by certain observations upon quinine and the local treatment of tumours, especially after operations for their removal.

Epithelial Growths obstructing the Male Urethra. By Dr. Roger.*—The case was that of a man dying of consumption, to whom Dr. Roger was called a few hours before death to pass the catheter. The only history obtainable was that, from a child, the urine had been voided in a very small stream, but had never been quite arrested. The patient had never had any venereal disease. After death the urethra was much enlarged and distended by papillary structures, with thick and indurated walls, which extended from the bulb to the meatus; and an isolated patch was even found on the mucous membrane of the prostatic part. The papillary growths were globular, ramifying in the form of villosities, and varying from the size of a pea to that of a pin's head. They were formed at the expense of the mucous membrane, and were a kind of papillary hypertrophy, the greatest part of which was composed of nucleated and rounded epithelial cells.

Epithelioma of the Cerebral Dura Mater. By Dr. Warren, of Dorchester, America.†—It is rarely that we meet with cases of epithelial cancer of the coverings of the brain. The following is an interesting instance of this affection:—It was that of a man, aged forty-seven, who, when a child, fell into the fire and burnt the top of his head, so that adjoining portions of the parietal bone came away, leaving a suppurating opening, which remained until about seven years ago, when, after a severe scalp wound from a bear, the opening healed. Two years ago the wound was re-opened by the clawing of a monkey, and could not be again healed, as an epithelial growth quickly sprang from the dura mater, and extended as a large mass through the osseous opening to the distance of an inch, of a bright red colour, not unlike the crest of a fowl, easily bleeding on slight touch. The brain was seen to rise and fall, and during any violent effort the morbid growth rose half an inch above its usual height, and emitted a singular odour somewhat like that of burning phosphorus. Convulsions and hemiplegia preceded death. After death the cerebral membranes on the right side of the brain were found replaced by a dense white tissue firmly adherent to the brain, to the extent of half an inch, and beneath this was an abscess of one inch in diameter, which apparently communicated with the ventricles; whilst beneath the growth in question the dura mater was only slightly adherent to the arachnoïd beneath.

The diseased mass had a peculiar white appearance, and presented the well-known microscopical characteristics of epithelial cancer.

* Annales de la Soc. Anat. Pathol. de Bruxelles, No. 3, 1860.

† Boston Medical and Surgical Journal, Dec. 20th, 1860, p. 414.

MORBID STATES OF BLOOD.

Case of Leucocythemia; peculiar Forms in the Blood, Liver, Spleen, &c. By MM. Charcot and Vulpian.*—The case was that of a woman, aged fifty-eight, of whom no previous history was known. After death the liver was of enormous size, very friable, and of a chocolate hue. The spleen was very large and firm, and of a mahogany colour. The vena portæ, the mesenteric and splenic veins, &c., were greatly distended by semi-coagulated blood of a chocolate colour, and here and there clots were seen in the splenic veins quite white and discoloured. The kidneys were large, but not otherwise altered. The lymphatic glands were of natural character, as also the lungs. The heart was hypertrophied, containing chocolate-coloured clot. The whole body remained warm longer than usual after death. Microscopical examination showed that, even on the day of death, vibriones existed in the blood, which, even after many days, yielded no serum, only liquefying in fifteen days, and having the putrefactive smell only some days after that. The blood contained a great abundance of white corpuscles, of which some were nucleated, others apparently not so, the latter being less numerous than the first. The nucleated white globules contained also numerous fine granulations. These were rendered paler by acetic and lactic acids, thin nuclei becoming more distinct and contracted. The non-nucleated white corpuscles contained also granular matter, and in some cases were quite full of large granulations, not dissolving on addition of acetic acid, which in some brought out one, two, or three small nuclei generally grouped, whilst in others no such result followed. The white globules were visible in the blood, even after having been kept for a month, and some were also seen fifteen days later. The red corpuscles were most unusually irregular in size. In addition to the above-mentioned, crystals of the following kind were found on the day following the opening of the body. These increased in number in the blood in proportion to the time which it was kept. They were very slightly, if at all, coloured, were elongated, octohedral in shape, and of various sizes, insoluble in cold water, but soon dissolved in warm water, crystallizing out again on cooling. They were insoluble in hot and cold alcohol, glycerin, æther, chloroform; soluble in acetic, tartaric, sulphuric, and hydrochloric acids, as also in potash, soda, and ammonia. Chromic acid did not dissolve them, nor did nitric acid. On examining the liver, the octohedral crystals were also there seen. The hepatic cells were softened, and very full of fat and amorphous granular matter. The spleen showed less crystals than the liver, but contained many fusiform elements, and mucous forms analogous to the white globules of blood.

The observers quote a case of leucocythemia related to the Biological Society, by MM. Charot and Robin, in 1853, in which similar crystals to those above described were found in the spleen; they are not aware of any other similar observation. They also allude to a case in which similar crystals were met with in fibrinous concretions expectorated in a case of catarrh.

NERVOUS SYSTEM.

Morbid Condition of Nerve-tissue in 'Ulcus Noma.'—A. J. van Zadelhoff has described several cases of this disease,† in which the nerves appropriated to the affected parts were carefully examined. From his observations he infers that the predisposing cause of this malady is to be sought in a peculiar condition of the blood and solids, and that in this state the second branch of the fifth nerve is, as a rule, preferentially affected. In one case his description of the actual state of the nerves was as follows:—Two branches of the facial nerve directly lost in the gangrenous parts were swollen and red with injected

* Gaz. Hebdom., tome vii. No. 47.

† An Academic Thesis, Utrecht, 1860: from the Dublin Quart. Jour, Nov. 1860, p. 475.

vessels. The root of the fifth nerve inside the skull on the corresponding (the left) side was quite brown to its insertion into the pons Varolii, owing to blood, which quite filled the cavernous sinus. This brown colour extended over the Gasserian ganglion and its three branches. The sixth nerve was also very red, and the space between the two was filled with coagulated blood. Microscopical examination showed that the nerve fibres of the infra-orbital nerve were everywhere surrounded with fat granules, while the primitive filaments were all in a state of decided fatty degeneration. The nerve-root, which on the left side was of a brown colour, was in a state of fatty degeneration; on the right side this fatty state was not absent, but it was less in degree.

RESPIRATORY ORGANS.

Dermoïd Cyst in the Lungs.—This remarkable case, related by Dr. Cloetta of Zurich,* occurred in the person of a woman, aged twenty, who died after symptoms of chronic pulmonary phthisis. It was a curious fact that during life large quantities of hair were from time to time mixed with the expectorated materials: and it was supposed by some that a deception was being practised. On making a section of the lower lobe of the left lung, a cavity, filled with hair, epithelium, and fatty matter, was found, the posterior wall of the sac being firmly adherent to the vertebral column and thoracic walls. On close examination this sac was found to be composed of two compartments communicating with each other, of which the smaller contained the dermoïd tumour, and the larger was a cavity in the lung of secondary orifice. The dermoïd cyst was equal to a moderate-sized apple, and its walls, of considerable thickness, were found of a firm fibrous tissue, in which partly cartilage and partly bony fragments existed, but nothing like dental tissue. The inner surface of the walls of this smaller cavity was beset with irregular condyloma-like projections united to each other by thick bands, and upon this surface were found numbers of hairs of various sizes. The dermoïd cyst projected about one-third of its size into the lung-tissue, and adjoining it was a smooth-walled cavity in the lung, the base of which was found by the dilated left ventricle of the heart. Into this cavity opened a large-sized bronchus.

It was manifest that the dermoïd cyst was originally formed in the areolar tissues outside the pulmonary texture, and not in the lung-substance, and that the cavity in the lung was the result of perforation of the cyst-wall. The case is allied to that described by Mohr.†

Microscopical examination of the newly-formed cuticular tissue revealed the ordinary appearances found by Kohlrausch,‡ Steinlin,§ and others; with regard to the characters amd composition of cysts containing hair, &c., in the ovaries, we would here refer to the paper by Dr. R. Lee in the last volume (No. xliii.) of the 'Med.-Chir. Society's Transactions.'

HALF-YEARLY REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIENE.

By Benjamin W. Richardson, M.A., M.D.

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I. Toxicology.

Poisoning by Cyanide of Potassium.—Those who may take the trouble to read our Reports will be struck by the increasing number of cases of poisoning by

* Virchow's Archiv, Band xx. Hefte 1–2, p. 42.

† Medizin: Zeitung, Berlin, 1839, s. 130. ‡ Müller's Archiv, 1843.

§ Henle und Pfeuffer, Band ix. Heft 2.

the cyanide of potassium. The symptoms produced by the poison seem as a rule fairly marked, and the pathological characters equally uniform. Nevertheless, it is well to put every case on record. The following, by Dr. Huseman, is of interest, inasmuch as recovery took place. A healthy man, twenty-one years old, took a draught, a gulp, from a bottle containing a solution of cyanide of potassium, which had been prepared for photographic purposes. The man immediately fell, but after a few minutes, during which he vomited spontaneously, he was restored. Twenty minutes after the poison had been taken, Dr. Huseman found the patient sitting upon a chair, conscious, and capable of answering questions, though somewhat disconnectedly. He complained of continued giddiness, disturbance of the mind, singing in the ears, coldness, incapability of rising without falling afterwards, and difficulty in breathing; his mouth was open, his face cyanotic, his eyes fixed but bright. The temperature of the skin, especially at the extremities, was greatly reduced; the tongue was cool, the cardiac and arterial pulsations were feeble, almost imperceptible, and the pupils dilated. Sensibility was decreased over the whole body, but there was neither complete anæsthesia nor spasm. There was no smell of prussic acid in the breath, nor had his relatives remarked any such odour. An emetic brought away a small quantity only of fluid; the fluid contained prussic acid. Spirit of ammonia and chlorine water were administered. In about three hours diaphoresis occurred over the whole body, with subsidence of the anæsthesia and coldness of surface. It was impossible to determine the dose of the cyanide taken, but it must have been small.—*Deutsche Klinik*, No. 13, 1860.

Case of Poisoning by Atropin.—A strong young woman, aged twenty, who was suffering from epileptic fits, took all at once the remains of a solution of one grain of sulphate of atropin in two drachms of water (about three-quarters of a grain of atropin). The patient had suffered before from attacks of insanity, which had been relieved by a solution of tartar emetic. Soon after taking the atropin solution, the following symptoms appeared: the pupils dilated so that the iris entirely disappeared; the eyes were swollen and bright; the face reddish purple, shiny, puffed up, burning hot; the lips bluish red; the hands bright red, and turgescent; pulse full, hard, 120; there was neither difficulty in swallowing, nor increased flow of saliva; nor involuntary evacuation; it was impossible to ascertain whether she suffered from thirst or dryness of the throat, as she would not reply to any questions, but screamed loudly, looked wildly around her, and threw her arms about. After the administration of iodide of potassium and iodine in watery solution, and strong coffee, and especially after a cold plunge bath, the symptoms already stated began to abate, and vanished entirely in four days. The beating about and dilation of the pupils remained the longest.

The case in question is noticeable from the absence of difficulty in swallowing, of tonic spasm in particular groups of muscles, of sleep, of thirst, and of excessive flow of saliva. Dr. Lange obtained a beneficial result from the use of belladonna in mercurial ptyalism.—*Deutsche Klinik*, No. 19, 1860.

The Poisonous Effects resulting from the Use of Arsenic in the Arts.—Dr. Carey Lea, in a paper on this subject—in which, by the way, he does not supply much positive proof of the injurious effects of arsenic as it is used in the arts—gives the following method for the detection of the metal in fabrics in common use in the household, such as carpets, wall-papers, window-shades, and curtains. The suspected substance is immersed in liquid ammonia, and allowed to remain in contact with it for some hours. The resulting liquid is then to be filtered or decanted, and treated with pure nitric acid until it is rendered perfectly neutral. A very slight alkaline reaction is not important; but the liquid should not be acid. Any trace of arsenic is then made evident by the addition of nitrate of silver, which causes an immediate yellow precipitate. The only substance which affords a precipitate liable to be mistaken for that of arsenious acid is phosphoric acid, which also with nitrate of silver gives a lemon-yellow precipitate. To determine whether the precipitate is phosphate or arsenite of silver, a tube of glass which is free from lead, having a quarter of an inch bore, is to be drawn out so that its extremity for about an inch in length may have an internal diameter of about one-fifteenth of an inch; the end of the tube being closed in the lamp. The yellow precipitate having been washed and dried, a portion of it is placed at the bottom of the tube, and over it a fragment of charcoal not in powder. The charcoal being adjusted, the narrow part of the tube is placed in the flame of a spirit-lamp, so that the charcoal and not the substance is heated. When the charcoal is red-hot, the tube is inclined from its previous horizontal position, so as to bring the point of the tube where the substance lies into the flame, keeping still the charcoal in it also. The silver salt is immediately decomposed. If it contain arsenious acid, the latter is immediately reduced by the charcoal, and forms a greyish black ring in the cool part of the tube. Dr. Lea refers to some very good figures in Graham-Otto, 'Lehrbuch,' vol. iii. p. 508, as illustrating the steps of this process.—*American Journal of the Medical Sciences*, July, 1860.

Local Poisoning with Phosphorus.—An interesting case is recorded by J. Mertens, in which gangrene seemed to follow from the application of phosphorus to a wound. The facts briefly are these. A sickly man, aged forty-three, had a small cut on the right fore-finger. The wound had been exposed to contact with some moist and badly lighting phosphorus lucifer matches. When the patient was first visited the right hand was swollen to the wrist, and was tender and œdematous. The fore-finger was swollen, and was of a bluish-red colour at the base, at the end thickened and gangrenous. All sensation was nearly gone, and the whole structure was mummified, as in gangrene of old people. The effects were so serious that amputation of the first two joints was necessary. The operation resulted in a perfect cure. The most careful examination of the heart and of arteries of the arm failed to show that they were the seat of any disease. Mertens therefore believes that the gangrene was due to the inoculation of the wound with phosphorus.—*Journal de Chim. Méd.*, Mai, 1860.

Poisoning by Camphor.—Dr. Fenerly records that a woman, the mother of five children, and thirty-six years of age, took during her fourth month of pregnancy twelve grains of camphor in one dose, in a glass of brandy. Her object was to produce abortion. During the succeeding two hours there were intoxication, headache, redness of the face, and a sensation of burning in the stomach: these were the prominent symptoms. Eight hours later there was gradually increasing pain in the epigastrium, loins, and bowels, with strangury and vomiting. Three days later, Dr. Fenerly found the patient in the following condition. The face was pale and livid; the eyes hollow; the skin cold and insensible; the pulse weak and thready; the breathing laboured; the abdomen extremely sensitive to pain on pressure; there were violent cramps in the extremities, and retention of urine for twenty-four hours, the bladder being full. There was slight discharge of blood from the vagina; the os uteri was enlarged and very hot; and there was coma. The patient lived on for three days, abortion taking place on the day immediately preceding her death.—*Ibid.*, January, 1860.

Poisoning by Jessamine.—In our medico-legal studies we do not remember to have before met with a case of poisoning by jessamine. The annexed case occurred under the care of Dr. W. H. Hall, of Brooklyn, New York State. A lad, eleven years of age, having eaten the fruit or berries of the jessamine plant, presented the following symptoms. The patient was discovered lying upon the floor of his room in a "fainting fit," so called by his parents, and it was obvious there had been some slight emesis. On being called, Dr. Hall found the child in a comatose condition; his face pallid, surface cool, pupils immoderately dilated, and respiration somewhat rhonchous, but of usual frequency. The pulse was slow and feeble; there was perfect insensibility. Inquiring of the mother whether the child had swallowed anything of a poisonous nature, Dr. Hall was answered in the negative. Soon, however, the father returning home, informed him (Dr. Hall) that the patient had been eating jessamine berries. An emetic had been given, but had proved ineffectual; now other emetics were given, viz. and severally, sulphate of copper, ipecacuanha, mustard, and warm water, none of which acted. Muscular movements were observed first about the face and eyes, especially on the left side, towards which the eyes and facial muscles were directed. The hot bath was ordered: whilst it was preparing, these growing symptoms were gradually becoming more and more aggravated, going from the head to the left arm, then to the left lower extremity, till finally the whole body was thrown into the most violent convulsions. At one time the spasms were chiefly opisthotonic, when the whole surface was congested almost to a blackened hue. The bath relieved these symptoms entirely. Throughout there was most marked rigidity about the muscles of the head and throat. The jaws for the most part were firmly locked, trismus being complete, as is often seen in tetanus. On coming from the bath, the patient, tired and exhausted, fell into a quiet sleep, from which he awoke to relieve, by emesis, his over-burdened stomach. Nothing further was given, excepting a cathartic. The patient for a few days felt weak and almost helpless, but gradually recovered his accustomed health and strength.

Of the jessamine plant, Dr. Hall adds, there are some twenty or thirty varieties under the order *jasminaceæ*. The plant to which the history given above refers, is a shrub with a twining vine-like stem, and bearing a small red berry. The berry has a flavour somewhat similar to that of the tomato, and is said to afford on distillation an essential oil. Dunglison speaks of its use among the Italians, where it is employed as an embrocation to the paralytic, and for the cure of rheumatic pains.—*Philadelphia Medical and Surgical Reporter*, January 5th, 1861.

Effects of a large Dose of Acetate of Morphia.—Th. Salviat gives the particulars of a case, the history of which was accidentally omitted in our last Report, in which an apothecary, thirty years of age, took a full *gramme* (fifteen grains) of the acetate of morphia immediately after a meal. The man was attacked with symptoms of intoxication; but in thirty-six hours, after the administration of an emetic and a free venæsection, he was tolerably recovered. Salviat opines that the drug, being taken after a meal, was not absorbed in sufficient quantity to destroy life, and therefore, on the action of the emetic, the poison was thrown off; or again, that probably there was "a peculiar disposition of the organism," by which the effects of the poison were counteracted.—*Gazette des Hôpitaux*, No. 107, 1859.

The Narcotic Properties of Indian Hemp.—The properties of the Indian hemp are now becoming matter for general inquiry. The substance, little understood as yet either as a medicine or as a poison, has received from Dr. Fronmüller a careful examination. His attention was first favourably directed to it in a case where morphia had been unsuccessfully given. He has now administered it in 1000 cases of disease: in these the drug acted successfully in 530 cases, partially in 215 cases, and slightly or not at all in 255 cases. The cases in which the drug acted successfully were instances of tubercle, inflammation, some surgical cases, rheumatism, ophthalmic diseases, neuroses, and dropsy. In 155 cases out of 304 sleep followed in one hour after the administration. The drug, according to Fronmüller, is the nepenthes of Homer, and was the remedy, during the Crusades, of the Old Man of the Mountain. The conclusions of the author are as follows: 1. Of all known benumbing media, Indian hemp is that which produces narcotism most perfectly compensating natural sleep; causing no violent vascular excitement, no special check on the evacuations, and leaving behind no ill effects nor subsequent paralysis. 2. On the other hand, the drug is neither so powerful nor so sure as opium. 3. It may be given in all acute inflammatory diseases and in typhus. 4. It is specially qualified to be given alternately with opium, in cases where the latter fails. 5. The best way of administering Indian hemp is in small pills, which, in addition to the alcoholic extract, hold some powder of the substance. As the smallest dose for producing sleep, eight grains, in eight one-grain pills, may suffice: the dose frequently has speedily to be increased. 6. The previously mentioned effects of Indian hemp on the skin, kidneys, and sexual organs are of no practical value.—*Vierteljahrschrift für die praktische Heilkunde*, xvii. 1860.

Influence of slow Lead-Poisoning on the Products of Conception.—M. C. Paul has observed at the Hôtel-Dieu of Paris, that the influence of lead not only manifests itself on the individual subjected to it by the phenomena of lead-poisoning, but by a still more injurious influence on the products of conception (in cases where the pregnant woman is the recipient of the substance), by causing the death of the fœtus or the new-born child. This fact is shown by the metrorrhagia amongst the females who have been pregnant; by miscarriages at three or six months; by premature accouchements, in which the children are born either dead or dying; and by a mortality above the mean during the first three years of the lives of children born under the influence of the poison. M. Paul, in his first series of observations, notes that in the cases of four women who had represented fifteen pregnancies while under the effects, more or less serious, of lead, there were ten abortions, two premature deliveries, one still-born child, one child born alive but dying in the first twenty-four hours, and one child living fully. His second series comprised five women who had had two natural confinements before being exposed to the influence of lead. After this exposure, there followed in these thirty-six new pregnancies, of which twenty-six were abortions of from two to six months, one premature accouchement, two still births, and five born alive, four of which died in the first year. Thus there only remained out of the thirty-six pregnancies two children, of which one is puny and the other is now only in its third year. One woman working in lead, and who during five pregnancies had made five false accouchements, quitted her occupation because pregnant, and was confined at the proper time of a child well born, and who lives still. Another series of cases shows the same alternation in the issue of the pregnancies when a woman first quits and then resumes several times occupations in which there is exposure to the poison.

In another inquiry, M. Paul has established the same facts in cases where the father has worked in lead. In a series of seven observations in which are comprised thirty-two pregnancies occurring during the time that the fathers were exposed to the lead poison, eleven infants were aborted, and one was born dead. Twenty infants were born living, of whom eight died in the first year, four in the second, five in the third, one only living beyond this term. Finally, under another series of observations, he shows that the fœtus may die from the probable influence of lead, the mother presenting none of the phenomena of the poison. *En résumé,* 1. It is necessary to admit that there is a new variety of accidents hereditarily transmissible, which comprise diseases produced by an inorganic body. 2. Saturnine intoxication does not prevent fecundation, inasmuch as of 84 individuals, interrogated on this subject, 29 women while working in lead became pregnant 123 times, being an average of four pregnancies each, which is about the ordinary mean. 3. Although lead does not act either on fecundation or menstruation, it does act on the fœtus, since in the 123 pregnancies there were 64 abortions, 4 premature labours, 5 still births, 20 deaths of children in their first year, 8 in the second, 7 in the third, and 1 at a later period; 14 children remained alive, of whom only ten are above three years old. There were also 15 cases of metrorrhagia due no doubt to abortion.—*Archives Gén. de Méd.;* and *Annales d'Hygiène Publique,* Janvier 1861.

Poisoning by Strychnine.—Dr. Danvin records a case of poisoning by strychnine; the patient was a female child, aged seven years and a half, generally in good health, and of strong constitution. She took five *centigrammes* (about three-quarters of a grain) of a powder given to her by a *pharmacien* as santonine, which she was accustomed to take every month. She complained of its bitterness, slept about five minutes, and then awoke, crying "I am burning." Immediately afterwards she had convulsions, which ceased for a time, and then reappeared for three minutes. Dr. Leroy, who saw this last attack, describes it as ceasing and being again repeated, and presenting especially first nervous excitement, and then violent spasmodic contraction of the jaws, with grinding of the teeth, bending backwards of the head, rigidity of the trunk, forcible extension and inversion of the feet, and subsultus of the muscles of the calves of the legs. The temperature of the body was raised, and there was abundant sweat. The patient did not vomit, did not lose her consciousness, complained of no particular pain in the short intervals between the convulsions, but cried once. Death took place at the end of half-an-hour at most, during a convulsive attack. The post-mortem examination was made forty hours after death. The face was pale and calm, the jaws contracted, the pupils dilated; there was great cadaveric rigidity, especially in the lower limbs. The feet were forcibly extended and turned inwards. The abdominal organs were remarkably healthy, except that there was a well-defined sanguineous infiltration at the posterior wall of the stomach; the kidneys were more injected than usual. The lungs were of violet colour, gorged with blood: the heart was of deeper colour than natural, and contained fluid blood. The pleuræ and pericardium contained some sanguinolent serum. The cerebro-spinal system presented a state of congestion both in the meninges, in the sinuses, and even in the substance of the brain; but the most important alterations were observed in the spinal cord. Its coverings were injected, the spinal arachnoïd was red, and at the root of each spinal nerve there was distinctly seen a drop of effused bluish blood, which gave to the nerve a very remarkable symmetrically spotted appearance. Dr. Danvin and M. Locquet tested the contents of the stomach and duodenum by the process of Messrs. Rodgers and Girdwood, portions of the viscera by the process of M. Stas, and the paper which had contained the powder by the sulphuric acid and chromate of potassa process. The results at which they arrived were :—1st. That the contents of the intestinal canal distinctly contained strychnine. 2ndly. That a part of the stomach, the small intestines, half of one kidney, and a part of the liver and of the spleen, presented very feeble traces of strychnine. 3rd. That the paper evidently contained strychnine. Dr. Danvin, in commenting on this case in relation to an English trial where the strychine question was raised, remarks, that it is now incontestably proved that Nunneley, Herapath, Rodgers, and Letheby were right in stating that strychnine, even when given in extremely minute quantities (*à des doses infinitésimales*), is capable of detection.—*Annales d'Hygiène Publique,* January 1861.

Differential Test of Arsenic and Antimony, deposited as Mirrors from Marsh's Apparatus.—When antimony or arsenic is driven over in combination with hydrogen, and mirrors are deposited in a heated glass tube, it is sometimes difficult to determine positively the fact of arsenic in the presence of antimony. In cases of this kind the following process will serve to set at rest all possible doubt as to the presence or absence of arsenic. Heat the long tube through which the arsenetted hydrogen passes to redness in several parts, to produce distinct metallic mirrors; then transmit through the tube a very weak stream of dry hydrosulphuric acid gas, and heat the metallic mirrors with a common spirit lamp, proceeding from the outer towards the inner border. If arsenic alone is present, yellow tersulphide of arsenic is formed within the tube; if antimony alone is present, an orange red or black tersulphide of antimony is produced; and if the mirror consisted of both metals, the two sulphides appear side by side, the sulphide of arsenic as the more volatile, lying invariably before the sulphide of antimony. If one now transmits through the tube containing the sulphide of arsenic, sulphide of antimony, or both sulphides together, dry hydrochloric acid gas, without applying heat, no alteration will take place if sulphide of arsenic alone is present, even though the gas be transmitted through the tube for a considerable time. If sulphide of antimony alone is present, this will entirely disappear as already stated; and if both sulphides are present, the sulphide of antimony will immediately volatilize, whilst the yellow sulphide of arsenic will remain. If a small quantity of ammonia is now drawn into the tube, the sulphide of arsenic is dissolved, and may thus be readily distinguished from sulphur, which perhaps may have separated.—*Fresenius' Qualitative Chemical Analysis.* Edited by Lloyd Bullock. Fifth Edition. (pp. 117–118.)

For further valuable papers and observations we would refer the reader to the following. A notice of Professor Bunsen's investigations on Spectrum Analysis in the 'Pharmaceutical Journal' for December, 1860. A short but useful paper on the Detection of Strychnine and other Vegetable and Animal Principles by Thomas E. Jenkins, in the 'Chemical News' for October 6th, 1860. [In this paper Mr. Jenkins tabulates the colour reactions of fifty organic bodies under the action of sulphuric acid and bichromate of potassa. His researches confirm entirely the colour-tests for strychnia.] Another paper on the Detection of Strychnia by Mr. J. Horsley, in the same periodical, on October 13th, 1860. Translation of a paper by M. Vella on the Antagonism which exists between Strychnia and Curara.—*Ibid.* Oct. 20. [In this article Vella thinks himself authorized to conclude that curara can completely destroy the effects of a dose of strychnine, which would be mortal if injected into the stomach or the veins, and that curara is the true physiological remedy for strychnia.] A paper on the Influence of Arsenious Acid upon the Waste of the Animal Tissue.—*Ibid.*, Nov. 3rd, 1860. A note from an essay by Kurzak on Tannin as an Antidote to Strychnia. —*Ibid.*, November 10th, 1860. [Kurzak considers tannin an excellent antidote for strychnia. The insoluble tannate is formed. For one part of strychnia from twenty to twenty-five of tannin should be given or even more, because a considerable part of the tannin is precipitated by the contents of the stomach.] An elaborate essay, entitled, 'Note sur les Accidents Saturnins, observés chez les Ouvriers qui travaillent à l'Emaillage des Crochets en Fer destinés à supporter les Fils Télégraphiques,' in the 'Annales d'Hygiène Publique,' Janvier, 1861. An essay, entitled 'Accidents causés par de l'Eau contenant un Composé de Cuivre,' by A. Devergie, Gobley, and Robin.—*Ibid.*

II. Insanity.

Statistics of Insanity in the United States.—A paper on the above subject, by Dr. Richard J. Dunglison, gives a comprehensive review of the statistics of insanity in the great Republic. By comparison, the report tells us something more than the mere facts of the local manifestation of the disease: it indicates by what peculiarities, physical and moral, the mind of a nation is modified. After making some apologies for the necessary imperfection of statistical records, but using such as he possesses fairly, the author compares, in the first place, the numbers of the *insane*, of *idiots*, of deaf-mutes, and of the blind. According to this calculation, there is one person insane to 1485 of the general population, 1 idiot to 1469, 1 deaf-mute to 2365, 1 blind person to 2367. Thus the blind very nearly approach in numbers the deaf and dumb, the insane and idiots are nearly alike also in point of numbers, while the insane are nearly 60 per cent. more numerous than either the blind, or the deaf and dumb. Examples of deaf and dumb persons are more common amongst the native than the foreign population; while insanity is most prevalent in the foreign population. Insanity is also more common amongst the white and free coloured population than it is amongst the slaves. "The condition," says Dr. Dunglison, "of a *happy* slave, thinking only of the present, and not dreaming of the future, would appear to be that which would give rise very infrequently to causes of insanity. Intemperance, so fruitful a source of misery and unhappiness to the white and free coloured, is comparatively unknown among the slave population, every slaveholder striving to prohibit the use of alcoholic liquors, and dreading the proximity of taverns or drinking places, which can supply unlimited indulgence to his slave." From certain facts connected with the taking of the census in 1840 and 1850, difficulties arose in estimating whether insanity is or is not on the increase in America.

In 1815 there was in America only one separate independent institution for the insane; this was at Williamsburg, Virginia; at the present time there are fifty-one. An analysis of the facts gathered from the records of these institutions gives Dr. Dunglison the opportunity of bringing out some really valuable statements on the influence of sex, age, occupation, season, and other circumstances in relation to insanity. In a table constructed from 11,259 cases, he deduces that the physical causes of insanity are pre-eminent over the moral in the proportion of 6610 to 4649. Amongst the moral causes, domestic anxieties stand first, and religious anxiety second; political excitement stands lowest on the list of causes.

We would add one further fact connected with the tables before us. This has reference to the influence of season upon the admissions of patients into institutions for the insane. In 21,072 cases, 20·6 per cent. were admitted in the winter months, 26·6 per cent. in the spring months, 29·2 per cent. in the summer months, and 23·4 per cent. in the autumn. The month of June seems to be the one in which the greatest number of cases are admitted. During the summer and autumn the number of cases are represented pretty equally by men and women, but in the winter months a greater proportion of men, and in the summer months a greater proportion of women, are admitted.—*North American Medico-Chirurgical Review*, July, 1860.

III. Hygiene.

Tardieu, with his usual industry and precision, contributes an essay on 'Maladies Accidentally and Involuntarily produced by Imprudence, Negligence, or Transmission by Contagion.' The heads of the essay are: 1. "On diseases occurring from vitiated, altered, or falsified provisions." 2. "On accidental poisoning or asphyxia." 3. "Errors in the prescribing or the administration of remedies." 4. "Contagious maladies transmitted from animals to man." The following observations, given under the last head, are amongst the most interesting of the essay. The diseases which are transmitted from animals to man—carbuncle, hydrophobia, glanders, and farcy, which specially affect domestic animals—propagate themselves by circumstances of contact which are extremely various, but which it is easy to describe, and some of which occur either from want of forethought of those to whom the sick animals belong, or from the duties to which the labourer or the servant is subjected. In either case the proprietor and the master are alike responsible for the mischief produced; that is to say, for the transmitted disease and for its effects. While the application of these principles is extremely common—almost of daily occurrence—in relation to wounds and accidents, it is on the contrary very rare in contagious affections. Tardieu, who has had more than most of his *confrères* to occupy himself with cases of hydrophobia and glanders, has only within the last few months been called for the first time before a court of justice to give an opinion in prosecutions for homicide by imprudence, instituted on the facts of hydrophobia and farcy terminating in death. He passes on to relate examples in point. But the most singular case given by the author is included under the third head. In this case a nervous illness was attributed to animal magnetism. The following is an outline of the history: At a *table-d'hôte*, a traveller, M. X., dined in company with some friends. During a conversation on magnetism, this gentleman undertook to convince the incredulous by offering to magnetize the first individual who presented himself. A nephew of the master of the hotel, a child ten years of age, was subjected to the trial. The child was seated, and M. X. commenced to make passes and grimaces. The child slept: M. X. was astonished at the result, and was puzzled how to reawaken the subject. Now commenced the gravity of the affair: two physicians were called as experts. Their depositions were singularly remarkable. The first stated that he found the child, who had fallen suddenly ill, a prey to a violent convulsive attack: he struggled with energy, and uttered inarticulate sounds. In the midst of these occurrences there appeared phenomena of indigestion: soon the scene changed to a state of complete calm. The child appeared asleep; his eyes were closed; and nevertheless, under the influence of his daily occupations, he recited fragments of lessons, replied to questions which were put to him, and even wrote on the invitation of one of the persons present. He was, in a word, in a state of somnambulism. Afterwards the same physician saw in the same child two similar attacks without the interposition of any similar exciting, or apparently exciting, cause. The second physician described the patient as in a state of somnambulism. The patient recited his catechism, and the son of this physician saw the child at night and found him in the same condition; he was then conjugating the verb "pouvoir." Both physicians took the opinion that the symptoms presented were those only of nervous disturbance; they were neither the symptoms of epilepsy nor of catalepsy; and the first physician finally denied the belief that the symptoms were due to any hypothetical magnetic force communicated from M. X. to the child. The child seems to have recovered slowly. The tribunal pronounced against M. X. to the effect that he, the accused, by divers passes and manœuvres exercised imprudently, produced on the patient over-excitement and a nervous disorder, the attacks of which have recurred at different intervals. The tribunal therefore condemned the accused to payment of twenty-five francs as a fine; for damages, twelve hundred francs; and the costs.

Commenting on this case, M. Tardieu suggests that inquiry should be made whether amongst the numerous dupes who submit themselves to the somnambulists and magnetizers there are not some who experience evil effects from the experiments and treatment inflicted upon them. Tardieu proposes, in a succeeding paper, to discuss the subject of communication of disease from person to person, in its medico-legal bearings. To this we may refer in our next Report.—*Annales d'Hygiène Publique*, Janvier, 1861.

On the Climate of Algiers in Chronic Affections of the Chest.—Dr. Prosper de Pietra Santa gives in the treatise under notice the results of researches he was commissioned to make by the French Government; together with the experience of medical men long resident in Algiers. He bases an opinion of the special suitability of Algiers to those suffering from pulmonary complaints on the following facts: 1. Its extremely pure atmosphere, the blue cloudless sky. 2. The short duration of twilight. 3. The changes of temperature, although the variations in the seasons are little marked, and the mean annual temperature rises to 66·5° Fahr. 4. The moderate hygrometric state of the air. 5. Limited oscillations of the barometric column in its diurnal and annual motions. 6. Certain periodicity of the winds and rains, caused by special and perfectly determined conditions.

These remarks apply, perhaps, more particularly to the city of Algiers and its immediate vicinity, the climate of which the author considers to be the happy medium between the temperate and the tropical. The city of Algiers is built in the form of an amphitheatre, and may be represented by a triangle, the summit of which rises about 460 feet above the level of the sea; the base is bathed by the waters of the port. At a distance it looks like a white, confused, compact mass, but by degrees the principal arteries of circulation in this labyrinth of passages, bazaars, barracks, and mosques are distinguishable. The streets are narrow, and the roofs of the houses are so close that they keep the sun off the streets; others are like sombre tunnels, admitting the light at the extremities only. Algiers, sheltered from the south wind, is made still milder by the land wind, which, warmed by the sun, tempers the freshness of the night, and with the sea breezes helps to keep up a uniform temperature. Nothing can be more agreeable, picturesque, or salutary than the environs; thousands of villas rise as by enchantment from the midst of groves of olives, jujubs, and oranges. Going out of the Bab-el-Oued gate you come to the Marengo garden, the oasis of the ancient Deys; the savage beauties of the Bouzaréah; of the valley of the Consuls; beyond this growing suburb, the road leads along the side of a partly wooded hill to gardens filled with verdure and flowers—the picturesque promenade of point Pescate. On the other side is the garden of Essai, with its 4000 varieties of tropical and luxuriant plants; Hussen-Dey, an important storehouse for the tobacco-harvest. Further on the most brilliant panoramas unfold themselves—Staouéli and its industrious Trappists; Chéragas and its odoriferous plantations of geraniums: and still further on the tomb of the Christian and the point of Sidi-Ferruch (where the French flag was planted for the first time). According to Sulszky, the rocks of the Sahel are of tertiary formation, and the fossils of the rocks belong to the present shells of the Mediterranean: the mountain itself is carved into winding valleys of ravishing beauty, the calmness and picturesqueness of which are unrivalled.

The flora of the environs of Algiers resembles that of the South of France and Spain; the herbaceous vegetation is inferior in abundance and duration, but superior in strength and rapidity of development. The rains here descend in repeated and heavy showers: the watery vapour seems to form itself into a high bed of clouds, the atmosphere is suddenly obscured, and large drops pour down upon the town, but the streets are dry nearly as soon as the rain ceases, and the invalid may resume his walk in the open air.

In confirmation of his opinions relative to the salubrity of the climate, Dr. de Pietra Santa quotes Dr. Brousseau, who says, "this kind of disease (phthisis) is much less frequent in Africa than in France; the difference is so great that it can depend only on the climate; no secondary cause would explain a similar effect;" also Martin, Moreau, Foley, Mitchell, Bertherand, &c., &c. Dr. de Pietra Santa's *résumé* consists of the six following propositions:—

1st. The climatic conditions of the city of Algiers are very favourable to affections of the chest in general, and to phthisis in particular.

2nd. Phthisis exists in Algiers in immigrants as well as in natives, but the disease is much more rare than in France, and on the shores of the Mediterranean.

3rd. The increase of phthisis among the natives (Arabs, Negroes, Mussulmans, Israelites) is due to exceptional causes, and to circumstances independent of the climate.

4th. The beneficial influence of the climate of Algiers is very appreciable where it acts either by suppressing the predispositions or by combating the symptoms which constitute the first stage of phthisis.

5th. This influence is disputable in the second stage of tubercle, more particularly when the general symptoms predominate over local lesions.

6th. It is fatal in the third stage, so soon as the phenomena of ramollissement, softening, and disorganization appear.—*Du Climat d'Alger dans les Affections Chroniques de la Poitrine.* 2nd Edition. Paris, 1860.

The Acclimatization of European Troops on Service in India.—On this subject Dr. James Bird shows the following conclusions:—1. That the law of constitutional deterioration—greater mortality for age in hot climates, beyond the ratio of like age in temperate ones—can be greatly reduced by removing endemic sources of disease, and by selecting suitable dry localities. 2. That in the most insalubrious tropical climates, the selection of good positions, on dry elevated table lands, suffices often to secure for European troops a perfect sanitary state nearly equal to the more salubrious regions of the temperate zone: this salubrity varying, however, with the geographical latitudes or longitudes of the places. 3. That the increasing ratios of mortality for hot countries mainly depend on unremoved surface drainage, and the generation of miasmata in places occupied. 4. That the morbific influence of seasons and climates in producing disease is in direct relation to, and dependence on the quality of the soil, the latitude, the longitude, the elevation of the place, its northern or southern exposure, and the national temperaments and races of soldiers who may be there located.—*Journal of the United Service Institution*, vol. iii.

Hygiène of the Sewing Machine.—Dr. Gardner, of New York, commends the sewing machine in relation to its effects on health. He reports on this subject at length; and as his opinions are widely different from the common expression, we shall give the more important of them in his own words:

"The principal diseases said to be caused by the sewing machine are the so-called 'female diseases' and spinal complaints. I have had some practice in these diseases, and may be allowed as a matter of personal experience to state that I have never seen a single patient who gained her living by working a sewing machine who was affected with leucorrhœa, 'falling of the womb,' 'ulcerations of the womb,' or spinal difficulty—who ever had an abortion while using it, or who in any way could trace an injury from it. Neither have I had any patients in private practice with any diseases at all attributable to it. I have had many patients who have made up their family and children's clothing for the season, and their 'baby-linen' just before their lying-in, with no injurious effects.

"I am aware that the jar of the machine and the 'up and down' vibratory motion are stated to produce abortions, but this seems to me to be a most erroneous opinion, inasmuch as the 'jar' of the machine, if there is any, falls not upon the feet or lower extremities, in which it is not felt in the slightest degree, but entirely upon the arms of the operator resting upon the table; and from this undeniable reason, the alleged analogy between the hypothetical statement, that 'the vitality of hen's eggs carried in cars and subject to their vibratory and oscillatory movement, is so destroyed that not one in a score will hatch,' does not hold good, even if it can be proved that the human ovum in a healthy uterus is killed by this trembling movement, as is claimed by some. Upon this point I have also a word to say in a proper place. Overwork, and by one unaccustomed or disused to the sewing machine, may very probably in some cases produce abortion, and so will a long walk in the Central Park, a day's shopping, excessive laughing even, the eating of a bunch of grapes; yet shall these be denied the parturient woman? Shall we take the exception for the rule?

"With the view of learning the facts that actually exist, I have made as careful inquiries as I knew how, of those running large numbers of machines for manufacturing purposes, of the girls actually and for many years working upon them, for their own experience and observation of those working by their side in the same factories, of physicians whose peculiar practice would lead them to note any general amount of disease among this class of girls, and now offer the result and many of the details of the inquiry.

"Douglas and Sherwood, extensive manufacturers of skirts, for several years ran some two hundred and fifty of Wheeler and Wilson's machines constantly, and were, till a change in their business made less machine work necessary, in the daily use of more machines than any one else in the United States, and probably in the world. Mr. Sherwood, under whose daily supervision was this portion of the work, said to me, 'That he had yet to see the first injurious effect from working a machine. Many girls who had come into his employ pale and weak, complaining of pain in the back, and at first unable to do a day's work, speedily became able to work their full nine hours, and became free from pain, robust, and healthy. He has never seen but one girl (who has a curvature in the spine between the shoulders) who was unable to use the machine. Many with spinal affections and curvatures work full time without any bad results. The girls are rarely away from work from ill health. The girls, when they first come, after a day's work, are obliged to ride home from fatigue—but they soon walk home. Now, he finds that those who *sit sewing* in the old-fashioned way, are so tired by night that almost all of them ride home, but the machine workers and those on their feet all day, walking around the hoop-frame, bending in every posture, now almost invariably walk to and from their homes, several miles distant. His own sister, who was fearful to try the machine, on account of a 'weak back,' has been enabled to use it ten and twelve hours a day, not only without injury, but even with positive benefit, as her health has materially improved since commencing it.'

"One lady in a private family stated that she had found an attack of neuralgia, to which she was very susceptible, to always ensue from the withdrawal of the animal heat through the iron foot-plate, whenever she wore thin slippers, but on covering the plate with a thick bit of carpeting, such a result was never afterwards noted.

"I have never heard of an instance of muscular rheumatism or cramps, affections most probable to be produced by such unusual exercise, arising from the use of any machine.

"From a visit to the factory of Payan and Carhart, where fifty Wheeler and Wilson's, and fifty Singer's machines are in daily operation in the manufacture of clothing, I found that the heavy Singer's machines were worked by compressed air—that so much muscular force was required to carry the machine on at a *paying speed*, that pushing the needle through heavy beaver cloth and buckram was too much for the muscular power of the girls—but with machinery they were enabled to run them as fast as might be desired. The working of these heavy machines with the foot did not, however, produce any disease. Exhaustion from overwork in this, as in every other overwork, was necessarily felt. The unanimous testimony was, that the machine had wrought a benefit upon the labourer. In particular it had enabled work to be so systematized as to make the employment of a large number of operatives, in large and well aërated and salubrious rooms, not only mutually profitable, but the workman could, from the system enabled to be introduced, make more wages in the factory than at home; thus the day was not as heretofore spent in a small apartment, containing bed, cooking-stove, children, work-bench, &c., but after a healthy morning's walk in a pure atmosphere and amid cheerful companions, again to be refreshed by the walk home after the labours of the day were finished. It was the opinion of those who worked for years on the board as journeymen tailors, and several years at the machine, that the latter was far better for health and spirits; that the mind was sharpened by the stimulus of the machinery, and the machine-worker was intellectually brighter than the mere sewer."—*American Medical Times*, December 15th, 1860.

It remains for us, lastly, to direct attention to certain other papers which our space does not permit us to notice in this section. A paper on "The Military and Sanitary Institutions of the Roman Armies," by Dr. James Bird: 'Journal of the United Service Institution,' vol. ii. A letter on the "Ventilation and Disinfection of Sewers," by John Stenhouse, LL.D., F.R.S. An essay entitled "Souvenirs de la Campagne d'Italie," by M. Boudin: 'Ann. d'Hygiène Pub.,' Janvier, 1861. An admirable paper on "Adulteration of Food," by Wentworth L. Scott, Esq.: 'Journal of the Society of Arts,' Feb. 1, 1861. And, in the same journal for Feb. 8th, a paper on "The Water Supply of London," by S. R. Burnell, Esq., C.E.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *On an Affection of the Mucous Follicles of the Urethra in Gonorrhœa.* By M. Diday. (Gazette Hebdomadaire, 1860, No. 45.)

If we examine closely the orifice of the urethra in a subject of gonorrhœa, we may sometimes discover in its vicinity a narrow aperture, through which may be made to issue, by pressing the glans forward, a droplet of discharge. Passing a needle into this aperture, it penetrates to a depth of from three to six millimetres in a direction parallel with the urethra. The orifice of this aperture resembles that of the urethra in being red, tumefied, pale, or indolent, accordingly as the gonorrhœa is in its acute or chronic stage; and when the patient has gonorrhœa several times, this lesion will be found always reproduced. The lesion consists in the gonorrhœal condition of one of the mucous follicles of the urethra, and constitutes a highly troublesome complication of the ordinary urethral affection, as respects the curability of this latter and the prolongation of contagiosity. The contagious pus which issues from the follicle is small in quantity and intermittent in appearance, and the lesion may be easily overlooked. Unaffected by injections and balsamiferous urine which do not come in contact with it, this lesion may persist long after that of the urethra has been cured, and as long as it does persist, infection continues. After trying various means for its cure, M. Diday has come to the conclusion that the only one which is feasible is the obliteration of the abnormal cavity by means of the actual cautery. He at first effected this by introducing a small heated needle into the orifice, and carrying it to the end of the passage; but as the doing this is somewhat tedious, the needle had time to cool before reaching its destination. The plan he has finally arrived at, and the one which he recommends as quite successful, is to pass a small knitting-needle to the very bottom of the duct, and shielding the glans by means of a paper covering, to heat the needle by placing a candle under it until the orifice is whitened, and a grizzling sound is heard. This amount of cauterization suffices for the cure, and when performed thus slowly, is easily effected. All acute inflammatory action must have subsided before it is put into force.

M. Diday takes occasion to observe, that in gonorrhœa the inflammation does not occupy the surface of the canal, but is engaged with a number of follicles similar to the one mentioned above, and that our object in its treatment should be to retain medicinal agents in the urethra as long as possible, so that they may penetrate into these little recesses. This may be done, in the case of urine charged with copaiba or cubebs, by means of a forceps contrived by M. Diday, the urine being thus kept for several hours in contact with the parts. When injections are employed, too, pressure should be made by the fingers at the entrance of the urethra and behind the scrotum, the fluid thus imprisoned being then forced to and fro.

II. *On Gun-shot Wounds produced during the Loading of Artillery.* By Dr Cortese. (Omodei Annali Universali di Medicina, vol. clxxiv. pp. 56—76.)

Dr. Cortese having related five cases of gun-shot wounds received by artillerymen while engaged in loading their guns, and pointed out the peculiar severity of such injuries, thus sums up his subject:—1. No other blow of a projectile imparts like the present so great an amount of commotion to the entire limb. The state of muscular contraction prevailing at the time constitutes a kind of solidarity between the hand, fore-arm, and arm, which is the chief and necessary cause of this commotion. 2. This circumstance compels the surgeon to direct his attention to the entire limb, whatever amount of lesion may be manifest in the hand. A neglect of this precept may lead to gangrene gaining possession of a large portion of the limb, or to a generalized suppuration, while a diminished power of reaction in the injured parts may give rise to purulent infection, or render useless recourse to amputation. 3. In all those cases in which the hand is severely torn, its disarticulation, or even the amputation of the fore-arm, is insufficient to secure recovery. The surgeon's knife penetrates into infiltrated tissues, more or less destroyed in their intimate structure in consequence of the concussion they have been subjected to. So that independently of the fracture of the bone, and of the possible disjunction of the articulations of the ulna, the success of the reparative process would be very problematical. In such cases the arm should be amputated. 4. The sooner amputation is performed, the greater is the probability of a favourable result. 5. The rapid and very extensive tumefaction of the limb constitutes a sufficiently certain criterion of the severity of the derangements which are propagated along its whole extent. When fractures are not detected in the diaphysis of the bone, some lesion of contiguity or continuity in the ulnar articulation must be suspected. 6. When the lesion does not seem severe enough to call at once for amputation, we must always be prepared for secondary occurrences which will unfit the limb for its functions. (In two of the author's cases, paralysis of the limb remained.) Still, conservative treatment in such cases should be attempted.

III. *Removal of a Ball by means of the Trephine Twenty-two Months after its Penetration into the Cranium.* By M. Jobert. (Comptes Rendus, 1861, No. 6.)

Jules Gustin, aged twenty-one, was admitted into the Hôtel-Dieu, Feb. 19th, 1857. Forming one of a force posted in front of the Malakoff tower, 8th April, 1855, he was struck by a ball in the forehead, the projectile, prior to penetrating the cranium, having come in contact with the external surface of the vizor, leaving a semilunar depression as it passed over the external edge of this. The man immediately fell from the parapet into the trench, a depth of seven feet, and remained unconscious in the ambulance for twenty-four hours. At the end of a week he was sent to the military hospital at Constantinople, where he stayed during four months. He then requested permission to return to the Crimea, and did so, in spite of the suppuration, which had never ceased. He fought at Traktir, 16th August, 1855, and returning to Paris in December, he remained for six months longer in the regiment. He was, however, unfitted for active service, being obliged to pass a considerable time in hospital. The symptoms continued much the same from the first, and consisted in a sensation of heaviness of the head, an uncertainty in the attitudes, and a feeling, when stooping, as if the forehead were separating from the head, suppuration always persisting.

On his admission into the Hôtel Dieu, a clean, circular aperture, about the size of a franc, was observed in the centre of the forehead. Passing the finger around the circumference of the opening, "osseous granulations, partial ossifications formed by the periosteum," could be felt, and on the introduction of a probe to the bottom, a hard, resisting, metallic body was recognised. After a crucial incision had been made and the aperture caused by the ball was found not to be sufficiently large to admit of its extraction, a circle of bone was removed by means of the trephine. The ball was now extracted, and some indurated, blackish blood was removed. The movements of elevation and depression of the dura mater, isochronous with the pulse, were plainly perceptible. The projectile proved to be a leaden ball weighing twenty-five grammes (375 grains.) Its surface was smooth and spherical over only a small portion of its extent, the remainder being rough and irregular. The details of the recovery need not be pursued, it sufficing to say that this was complete, the man as late as October, 1860, having been seen perfectly well.

M. Jobert calls attention to the fact of the ball having remained for so long a period in contact with the dura mater without inducing any inflammatory action He also lays stress upon the circumstance of his having, after the completion of the operation, introduced the flaps formed by the crucial incision into the accidental opening. To this he attributes the non-occurrence of necrosis or osseous exfoliation usually observed after trephine operations when the parts are exposed to the air. Here, such exposure was prevented, immediate union of the raw flaps with the bleeding osseous surface being secured. The isochronous pulsations continued for a time visible, but they became more and more obscure as the tissues covering the aperture augmented in thickness.

IV. *On Pyæmia* By Professor Roser. (Archiv für Heilkunde, Jahrg. i. ss. 37, 193 und 329.)

The following observations form a brief abstract of some interesting papers on Pyæmia, from the pen of Professor Roser, of Marburg:—

1. *The Specific Nature of Pyæmia.*—We are indebted to the obstetrician rather than to the surgeon for any progress which has been made in our knowledge of the nature of pyæmia, he having established the miasmatic character of the pyæmia of puerperal women, and its identity with the pyæmia of the wounded. The doctrine of "Surgical Fever," so ably expounded by Simpson, has, however, made but little way in Germany, Virchow's views on thrombosis there predominating, as the doctrine of plebitis formerly did in France. Professor Roser shows, in some detail, that neither Hunter's theory of phlebitis, Rokitansky's disease of the bloodvessels, or Virchow's thrombosis, afford any sufficient explanation of pyæmia, conditions being assigned as causes

which are mere concomitants or effects. The attempt to explain its occurrence by the fact of the absorption of ill-conditioned pus also fails; for, although various analogous circumstances are producible by such absorption, these differ much from those of pyæmia, and may be expressed by the term septæmia. The two conditions may, indeed, be combined, and we may have a septic pyæmia, just as we may have a septic variola or scarlatina. If pyæmia be followed out through its various modes of manifestation, it will be found to exhibit a marked similarity to typhus and other zymoses; and just as in the case of these zymoses, while sometimes it appears epidemically and as the result of contagion, at others it arises spontaneously, without the prior presence of pus. This fact has long been known in lying-in hospitals, and careful observation will easily detect similar cases in surgical wards. Stromeyer has observed a whole series of such cases, and similar ones have come under the author's notice. The explanation of the occurrence of sporadic cases of pyæmia may be difficult, but it is no more so than is the explanation of sporadic typhus or cholera, or other zymoses.

Forms of Pyæmia.—Professor Roser confines his attention to some of these which have excited but little attention. 1. *Pyæmic Febricula:* Just as during the prevalence of typhus we find patients here and there exhibiting but slight symptoms, so in hospitals infected with pyæmia, a mild form of the disease is observable which may be termed febricula. It has been but little noted, as was indeed to be expected, by those who were on the look-out for phlebitis, sepsis, or thrombosis, as the initial phenomenon of pyæmia; but the author instances cases in his own and in Stromeyer's practice. In lying-in hospitals the febricula is termed "milk fever"—a term of doubtful propriety, seeing that the affection is observed sometimes in almost all the inmates, and at others in none of them. 2. *Pyæmic Erysipelas:* When, in a patient suffering from pyæmia, erysipelas appears, the natural conclusion is that the pyæmic blood-disease has localized itself in the skin, just as it might have done in the pleura or in a joint. When, however, in a subject of erysipelas symptoms of pyæmia appear, the question may arise whether during the erysipelas the blood-poison has become developed, whether the pyæmia has become added as a second special process of disease, or whether the erysipelas itself was only the first manifestation of the pyæmic condition. Lastly, erysipelas and pyæmia may co-exist, and although they may not often be met with in the same patient, they are frequently found prevailing among different patients in the same ward. The author's conclusion is, that hospital erysipelas is a consequence of pyæmic infection and its localization in the skin, although he admits that it is doubtful whether another variety of blood-poison may not also give rise to it. 3. *Pyæmic Diarrhœa:* This affection, well-known to clinical observers, has obtained but little notice in hand-books. There are cases in which no other symptom except the diarrhœa is present, but the author still regards these as pyæmic, occurring as they do simultaneously with other cases in which the diarrhœa has only been the first of the whole series of symptoms. When this diarrhœa is combined with the pyæmic erysipelas, the disease exhibits striking and rapid contagious properties; and in hospitals in which precautions against contagion are not taken, this hospital epidemic diarrhœa, though little spoken of, is of frequent occurrence.

Therapeutics.—Although the incurability of pyæmia is no longer believed in, practitioners in general are not aware of the number of recoveries which really do take place; and that even when excluding the slighter cases above alluded to, and admitting only examples of well-marked pyæmia. The number of recoveries has increased in proportion as the essential conditions of fresh air and a good diet have been appreciated. Supporting the patient's strength by means of wine, has replaced the former mischievous antiphlogistic treatment; and indeed, judging from the use made of alcohol in England, there is danger of the opposite extreme being fallen into. Quinine, though usually of no great utility, may in some cases be a valuable adjuvant; and morphia is an invaluable remedy, serving not only to check diarrhœa, abate pain, and diminish danger in peritonitis, &c., but also to tranquillize the excited and delirious patient. The most important agent in the treatment, however, is a frequent renewal of fresh air, and the removal of all objects likely to pollute it.

Prophylactics.—Under this head the author lays great stress upon simplicity in dressing wounds, observation of the strictest cleanliness, and checking the decomposition of pus by cold and chlorine applications. When the spread of pyæmia is to be guarded against, instruments and nursing appliances should not be used in common, no autopsy should be performed by those attending on the sick, and the surgeon, visiting his cases of pyæmia last, should change and ventilate his clothes before seeing other patients. In all cases of ill-conditioned suppuration at Marburg, weak chlorined water is employed, and after waiting on such patients, the nurses carefully wash their hands in the same fluid. Finally, Dr. Roser protests against the erecting hospitals with large surgical wards, unaccompanied by means for isolating the subjects of pyæmia. Small hospitals of even a very faulty construction, give rise sometimes to a less mortality than some magnificent structures in which the patients are assembled together in large numbers.

Juridical Relations.—Under this head the author discusses the question which may come before the surgeon in a court of law—viz., whether a fatal pyæmia following an injury not in itself necessarily fatal, should be regarded as an essential condition of such injury, or as an accidental and superadded circumstance; the exact determination of this point modifying in the German courts the amount of punishment to be awarded. He cites cases in which the pyæmic complication has been altogether overlooked, or has been wrongly interpreted, to the detriment of the accused. He refers also to two other conditions to which the same considerations apply—viz., diffuse inflammation and tetanus. Diffused inflammation after wounds of tendons, &c., so often observed in hospital, and so seldom in private practice, should not be attributed to the nature of the local injury, but to the presence of miasmata. In fact, it is of a pyæmic nature. With respect to tetanus, we know nothing concerning its causal connexion with wounds, and when questioned juridically, we should avow such ignorance. The author himself is disposed to regard it as a zymotic affection, not only because no causal connexion with the wound can be made out, but from its analogy to hydrophobia, which is a zymotic affection, and from its disposition to prevail as an endemic or epidemic.

V. *On the Employment of Chloroform in Operations upon the Eye.* By Professor Stoeber. (Bulletin de la Société de Chirurgie, tome x. pp. 281–295.)

The object of Professor Stoeber's paper is to recommend the more general use of chloroform during operations upon the eye, most practitioners confining such use to cases of extirpation, to the operation of strabismus in children, and for the removal of foreign bodies in very young or pusillanimous subjects. But in operations for cataract, artificial pupil, &c., anæsthetics have been but little resorted to. Among the objections advanced against their use are the following: 1. As the use of chloroform may prove fatal, it should not be had recourse to except in cases of absolute necessity. The author believes that danger may be avoided by causing the patient to inhale the chloroform from a distance, so that it may become mingled with air, by placing him in the horizontal posture, and by causing him to fast for five hours prior to the operation. 2. Most of the operations on the eye cause too little pain to render recourse to it necessary. The operations of extraction, artificial pupil, and removal of staphyloma, are, however, more painful than is generally believed; and although they do not cause the suffering of some other operations, they are accompanied by a great amount of apprehension. 3. The operation may be facilitated when the patient is not insensible. Thus the sitting position is more convenient than the horizontal, and it is in some operations of advantage for the patient to be able to direct the eye as required. There can be no doubt that the sitting position is the most convenient; but the author does not venture to imitate M. Juengken, who adopts it even with patients under chloroform. 4. In case of the patient becoming aroused prior to the completion of the operation, his involuntary movements may give rise to serious accidents. This should be prevented by carrying the anæsthesia to complete resolution. 5. Chloroform often gives rise to vomiting, which may lead to disastrous consequences after operations for cataract, staphyloma, &c. Vomiting is not of frequent occurrence when chloroform is administered to a fasting patient. The employment of ice prior and subsequent to inhalation, recommended by Juengken, is, however, no infallible preventive.

When the inhalation has been carried to the extent of producing complete resolution of the muscles of the extremities, the orbicularis still offers some resistance to opening the eyelids, and to produce its complete relaxation the chloroform has to be continued yet longer. The contraction of all the muscles of the eye and fixity of the globe, stated by Chassaignac to be a constant result of the inhalation, is really due to this having been continued for an insufficient time. The author, in his operations for cataract, has always found great flaccidity of these muscles; and after the operation the cornea often sinks back upon the vitreous body so as to leave a considerable depression anteriorly. Such relaxation is one of the most important effects to be sought for in extraction, preventing wounds of the iris and issue of the vitreous humour. In the less painful operations, muscular resolution is of far more importance than the anæsthesia properly so called. For the operation of extraction, chloroform has its advantages and inconveniences. The eyelids are opened with the greatest ease, the knife is easily passed in at the chosen spot, no aqueous humour flows out prior to the completion of the section, the lens requires gentle pressure for its disengagement, and no loss of vitreous humour occurs. These results are evidently due to the absence of contraction of muscles of the eye, joined to the horizontal posture assumed by the patient; and this absence of muscular contraction remarkably facilitates the operation in those difficult cases in which the eye lies deep in the orbit. On the other hand, to obtain this complete resolution of the muscles of the eye is sometimes very difficult, much time being required; and yet if this be imperfect, the contraction of the muscles will seriously impede the operation. Vomiting, too, which is especially likely to occur after prolonged inhalation, may seriously compromise the results of the operation. The patients also usually complain of some pain in their eyes on recovering from the anæsthesia, which is not the case with those who have not been submitted to this. Lastly, the eye being flaccid and the cornea not being tense, the operator, if he do not bear this in mind, may easily carry his incisions too far. The balance of advantages is in favour of the use of chloroform in pusillanimous and irritable subjects; while it is uncalled for, and may even be disadvantageous, in those of a calm temperament, and whose nervous system is not very impressionable.

VI. *On the Treatment of Flat-foot.* By Professor Roser. (Archiv für Heilkunde, 1 Jahrg., ss. 481–486.)

In this paper, Professor Roser repeats a statement formerly made by him, and now based upon some half-hundred cases, that the so-called atonic flat-foot (the *talipes valgus spurius* of Little) may be cured by the forcible rectification of the position of the foot under chloroform. All cases of flat-foot are not thus curable, but that form which is of most frequent occurrence, met with in young servants, apprentices, &c., and hitherto reputed well-nigh incurable, is just the one which gives rise to the most successful results. This statement may surprise those who place faith in the correctness of Stromeyer's theory of the production of the disease by relaxation; but Prof. Roser regards this theory as erroneous, and, from attentive observations, believes the following is the manner in which the disease is produced:

When the weight cast upon the tarsus is maintained too long, it gives rise to a stretching of the ligaments, and to a one-sided pressure upon the tarsal arch. This partial pressure made upon the parts in growing youths gives rise to a change in the form of the bones, and a consequent flattening of the arch; the change operating upon the corresponding surfaces of the head of the astragalus and the scaphoïd, and those of the os calcis and the cuboïd. Adduction becomes painful or impossible, and the tension of the muscles soon becomes permanent. By the help of chloroform the tension is relieved, and adduction can be performed and the foot may be rectified in position. If by the aid of the plaster of Paris bandage or other means it can be so retained for a while, the bones will assume their natural forms, and the flat-foot will be cured. During the remaining period of growth, care and precaution in employing the foot will only be required. There are cases in which the flat-foot becomes developed in the course of a few weeks; and owing to the pain, assistance is early sought for and the cure is rapidly obtained, even without the aid of the plaster of Paris bandage. In some of such cases mere confinement in bed for three or four weeks suffices alone as the means of treatment.

The author some time since explained the occurrence of *genu valgum* in an analogous manner. In consequence of the prolonged standing, the external condyle of the femur is impeded in its growth, and the internal condyle is relatively too long. If the knee be drawn outwards, and so maintained by the aid of a machine, the leg will again become straight.

VII. *On Dangerous Hæmorrhage during the Treatment of Old Stricture.* By Dr. Steiger. (Würzburger Medicinische Zeitschrift, Band i. s. 155.)

Dangerous and even fatal hæmorrhage, either from the bladder or rectum, is one of the untoward occurrences which may take place after the dilatation of an old and considerable stricture. Still, the occurrence must be a rare one, or it would have been alluded to by Pitha in his recent work; and the author has himself been much surprised at meeting with it just when he supposed that he had succeeded in treating a difficult case of stricture. He has met with two such cases. In the first of these, after a very bad stricture had been considerably dilated, the flow of urine became obstructed by a coagulum, which on removal was followed by others, and these by hæmorrhage, which nothing arrested. No post-mortem was permitted.

In the other case, copious hæmorrhages from the rectum took place after a bad stricture had been considerably ameliorated, and when no hæmorrhoïds or other apparent cause for it existed. The hæmorrhage was obviously not of a traumatic character, occurring only as it did when the worst of the treatment was over, and the author explains it as follows: When a stricture has existed for several years, the urine in the imperfectly emptied bladder exerts considerable pressure on the walls of the bladder and on the rectum, destroying more or less the power of resistance of these parts, and leading to a varicose condition of their veins. When, by the relief of the stricture, the urine obtains a free discharge, an enlarged vein of the relaxed parietes may give way, and hæmorrhage occur—an uræmic condition of the blood rendering this all the more probable.

VIII. *Digital Compression in a Case of Aneurism of the Temporal Artery.* By M. Mirault. (Gazette des Hôpitaux, No. 147, 1860.)

A lad, aged nine, had his temporal artery wounded by the beak of a cock, and when he came under the author's notice, at Angers, an aneurysm the size of a nut had formed in front of the right ear. Direct digital compression applied at the upper part of the tumour checked the pulsations; and this was continuously applied for, upon the average, nine hours and a half daily for nine days, making eight-five hours in all, the boy being allowed to sleep at night undisturbed. The cure was complete.

When this case was related at the Société de Chirurgie, M. Chassaignac observed, that although at first sight the cranium would seem to lend itself to direct pressure better than any part, yet, in point of fact, direct compression rarely influences the arteries of the scalp, so that this case of M. Mirault's is quite an exceptional one. M. Morel-Lavallée confirmed this observation, and referred to a case in which hæmorrhage from one of the arteries of the scalp, divided by a broken bottle, could not be arrested by the most methodical compression, and the ligature had to be applied. M. Broca could not regard this case as analogous to that related; while the possibility of arresting hæmorrhage by compression is shown by the ease with which this was done after arteriotomy, formerly frequently resorted to. M. Larrey observed upon the necessity of distinguishing three groups of cases in which compression has been resorted to—viz., tumours, hæmorrhages, and arteriotomy. The insufficiency of compression in most cases of sanguineous or pulsatile tumours of the head must be admitted; but this observation will not apply to traumatic hæmorrhages of the surface of the head, many examples of which he has seen, and which may in general be arrested not only by pressure methodically applied, but also by the simplest dressing, or almost by mere exposure to the air. Still less can M. Larrey admit the insufficiency of compression after arteriotomy, for in numerous cases of this mode of depletion ordered by his father, he never met with any difficulty in restraining the bleeding. M. Chassaignac adverted to the necessity of distinguishing between a hæmorrhage and a wound of an artery. When there is hæmorrhage from a wound, the vital properties of the tissues are modified, and congestion, with softening and inflammation, are present; while in a recent wound all the properties of the tissues are preserved. In the first case compression will not be tolerated, or if it is, it may lead to mischievous effects. As a general rule, speaking from clinical experience, direct compression is not serviceable for arresting hæmorrhages.

IX. *Report on 158 Operations for Stone performed in Persia.* By Dr. Polak. (Zeitschrift der Gesellschaft der Aerzte zu Wien, 1860, Nos. 42–45.)

In this Report, Dr. Polak, body-surgeon to the Shah, gives an account of 158 operations for stone which he has had occasion to perform in Persia between November, 1852, and June, 1860. He states that in some of the provinces of Persia urinary calculi are of very frequent occurrence; and he attributes the circumstance in a great degree to the acidity of the diet employed, such as the various preparations of sour milk, unripe grapes, acid drinks, and a variety of rhubarb containing much oxalic acid.

In accordance with the experience of other countries, females are much seldomer the subjects of operation than males, and of the 158 operations only nine were performed on them. Race, too, seems to exert its effect, for Jews and Armenians seem to be but little subjects of stone, while negroes are almost entirely exempted. Position of society exerts but little effect; for the diet consumed differs not in its nature, but only in its mode of preparation. The ages of the 158 subjects were as follow: 69, from one to seven years;

53, from eight to fourteen; 9, from fifteen to twenty-one; 16, from twenty-one to fifty; and 11 above fifty. Children are therefore chiefly the subjects in Persia, and, in fact, the passage of small calculi by young children is of daily occurrence. This proportionate frequency in childhood may probably be in part referrible to the prolonged lactation which prevails; and the rarity of the disease in the adult depends upon the fact that the disease really usually commences in childhood, the bulk of its victims dying before they reach man's estate.

Of these 158 operations, 121 consisted in lateral operations, 2 of recto-vesical operations, 2 in lithotrity operations, 9 in urethro-vesical incisions of the female urethra, and 24 in incisions of the male urethra. The operations were usually performed in the open air under the shade of a tree, or in a balcony, chloroform being administered without apparatus. With respect to the *lateral operation*, the author is convinced that a large opening into the bladder is very preferable to having to employ much traction, and to the neglect of this maxim he attributes the loss of two patients by subsequent peritonitis. Of the 121 patients, 114 recovered and 7 died. In 2 of the cases primary union, without suppuration, took place; while in the rest the healing process was one of remarkable rapidity, the patient being dismissed quite cured (or, as the Persian phrase is, "sent to the bath") by the fourteenth or fifteenth day. Children were often found playing on the floor by the fifth or sixth day. All European surgeons in Persia testify to the rapidity with which wounds, and especially those of the bladder, heal, so that operation for stone in children is there one of very slight danger. This disposition takes place at all temperatures, from 30° R. in summer to —3° R. in winter. The author attributes these successful results to the healthy constitutions of most of these patients, the performance of the operation and the after-treatment being conducted amidst the free access of air, the rarity of erysipelas, the dressing the wound with fresh cotton wool in place of charpie, and the dexterity which the frequent occurrence of the operation permits of being acquired; the isolation of the patient and his free exposure to the air being the most important of all these circumstances. Incontinence of urine, dependent upon irritation of the neck of the bladder, and only occasional, was the most frequent unfavourable occurrence, and this was successfully treated by wax bougies and the internal administration of sulphate of iron, combined with cubebs and the resin of the *terebinthina lentiscus*. The *fatal cases* were 7 in number, three patients dying from peritonitis, and the others being adults of enfeebled constitutions, varying in age from forty to seventy-five.

Prostocystotomy was performed in two cases in consequence of a portion of the calculus being engaged in the prostate, and projecting towards the rectum. Both patients are well, although recovery was delayed in the one case for two months, and in the other for three weeks.

Lithotripsy was also resorted to in two of the author's early cases, from ten to twelve sittings being required. Owing, however, to the want of time, proper instruments and assistants, and the want of dexterity acquired only by practice, (most of the patients also being children in whom lithotomy succeeded so well), the author did not repeat the operation in other cases.

Urethrotomy was performed in boys twenty-four times. The native Hakims endeavour to remove impacted calculi by pressing them forwards or by sucking the glans, and, aided by the *vis à tergo*, they often succeed. Were this not the case many children would die of retention. The operation, as performed by the author, for impacted calculi, was very simple, merely cutting down upon the stone, confined between the thumb and finger, and extracting it with a forceps. With two exceptions, healing took place immediately, so that by the second day the urine passed by the natural channel.

Operation on Females.—This was put into force in nine instances, at ages varying from two years and a half to fifteen, and consisted in slitting up the urethra throughout its whole length, including the neck of the bladder, directing the incision upwards and to the left side, so as to make an angle of 45° with the pubis. The forefinger was then passed in to ascertain the size of the stone, the incision being enlarged upon it if requisite, and the stone removed by means of a lithotomy or polypus forceps. In this way room is obtained for the removal of considerable calculi. Little hæmorrhage occurs, and as the vagina is not implicated, no urethro- or vesico-vaginal fistula is left. Incontinence of urine, however, in some cases persists for long, and must be treated as already directed.

Among the author's numerous obstacles in pursuing the practice of surgery in Persia, was that of a difficulty in securing the specimens of urinary calculi, inasmuch as they are, like bezoars, endowed in popular belief with valuable medicinal qualities. He was often obliged to content himself with only half a specimen. He adds to his essay a tabular view of the chemical characters and various other particulars of 67 vesical and 11 urethral calculi.

X. *Case of Successful Treatment of Ranula by Electricity.* By Dr. Scarenzio. (Omodei Annali Universali di Medicina, vol. clxxiv. p. 85.)

A woman, aged sixty-five, applied at the Mantua Hospital on account of a ranula situated on the right side of the frenum, and which having during three months attained the size of a walnut, caused an obstruction both to speech and mastication. There was some swelling of the submaxillary gland of that side, but this had diminished with the enlargement of the tumour of the mouth. The affection being uncomplicated, it was determined to resort to electricity in its treatment. Two pairs of Bunsen's pile were employed, and a steel needle attached to the negative pole was passed about four centimetres into the fluctuating tumour, the other extremity of the pole being brought into contact with the outer surface of the lower jaw. The current was allowed to act for almost a quarter of an hour, the patient only complaining of a slight smarting. At the seat of puncture an eschar, a centimetre in size, was formed, and on the removal of the needle a portion of the fluid was discharged, the whole afterwards flowing out during the movements of the mouth. Next day strong local reaction was set up, accompanied by considerable fever, which yielded to bleeding and purgatives. In a few days the eschar came away, and the granulating wound healed, without any further collection of fluid having taken place in the cyst, which contracted upon itself. The function of the orifice of the Whartonian duct was recovered, saliva being seen issuing therefrom. The woman was seen on other account more than fifteen months after the operation, and the cure was found to have held perfectly good.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D., F.R.C.P.

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I. The Non-Pregnant State.

1. *On Separation and Transplantation of the Ovary; and on Spontaneous Separation of Sub-peritoneal Fibrous Tumours of the Uterus.* By William Turner, M.B. Lond. (Edinburgh Medical Journal, January, 1861.)
2. *On the New Formation of Uterine Glands in Uterine and Ovarian Sarcoma.* By Rokitansky. (Monatsschr. f. Geburtsk., Nov., 1860.)

3. *Atresia of the Vagina, with Abnormal Menstrual-path.* By Dr. GRAF. (Virchow's Archiv, 1860.)
4. *Complete Absence of Vagina; Uterus performing its Functions.* By Dr. PATRY. (L'Union Médicale, December, 1860.)

1. IN our last Report a summary was given of a memoir by Rokitansky, on Twisting and Separation of the Ovary. Mr. Turner, Demonstrator of Anatomy in the University of Edinburgh, has now contributed a case of separation and transplantation of the ovary, which came under observation in the dissecting-room. In a woman, aged seventy-nine, a tumour was found in the brim of the pelvis, attached by its posterior surface to the peritoneum covering the anterior surfaces of the last lumbar and first sacral vertebræ, and by its upper surface to the omentum; these adhesions maintained the tumour *in situ.* The left Fallopian tube proceeded from its proper uterine angle towards the left sacro-iliac joint, and little more than an inch long, terminated in a fine pointed end. It contained in its interior a hard cretaceous style, the canal being obliterated. No trace of an ovary could be found in connexion with the broad ligament on this side. The tumour being minutely examined, was found to be an enlarged and encysted ovary, the other portion of the Fallopian tube being connected with it, and, like the uterine portion, having a central cretaceous style. The ovary, thus cut off from the Fallopian tube and broad ligaments, had derived a vascular supply from its peritoneal adhesions.

A detached sub-peritoneal fibrous tumour is also described; it was lying loose in the abdominal cavity, having been probably detached from the uterus.

The paper contains an excellent *résumé* of the subject, and embodies Rokitansky's observations.

2. In the so-called fibrous polypi of the uterus are found gland-like tubes, which correspond to the uterine glands. If these tubes are new formations, then, according to some, there is a uterine adenoïd sarcoma. If these gland-tubes degenerate into cysts, then there results a uterine adenoïd cysto-sarcoma. From minute examination of three uterine polypi, and of one ovarian cysto-sarcoma, Rokitansky lays down the following observations:

(1.) In many uterine polypi there are found gland-tubes, which are either elongated glands of the uterine mucous membrane or new formations.

(2.) These tubes may also degenerate into cysts.

(3.) In the space of these cystic degenerated tubes grows the mass of the sarcoma in the form of a papillary excrescence.

(4.) A sarcoma provided with uterine tubes also appears in the ovary.

(5.) The mucous membrane investing uterine polypi exhibits alterations of texture resembling those which the uterine mucous membrane suffers as the result of chronic catarrh.

3. Dr. Graf relates a case of closure of the vagina which took place in a young woman as the result of an ulcerative inflammation, in May, 1856, the cause and course of which were unknown. She denied pregnancy, but affirmed that a hard, bluish, pear-shaped tumour had been passed by the vagina. Adhesion of the greater part of the vagina followed. Retention and accumulation of the menstrual blood ensued, with vomiting of blood and obstinate constipation. In August there escaped, to the great temporary relief of the patient, a discharge of blood by the rectum; this was repeated regularly every month, bringing ease for the time, but the periodical exacerbations remained very severe. In March, 1857, Professor Busch, of Bonn, attempted an operation; a catheter was placed in the bladder, and the finger of an assistant in the rectum, to serve as guides for the knife and scissors. After opening up a portion of the vagina, but without reaching the uterus, the operation was given up. In October, 1857, Dr. Graf had charge of the patient. In the

period of exacerbation he felt the uterus distended and high above the symphysis, also through the rectum; this continued until the discharge of blood by rectum brought relief. A distinct opening between uterus and rectum could not be felt; it was a question whether the discharge might not be vicarious. Later this discharge stopped; the symptoms became much aggravated, and death appeared imminent. In February, 1858, Dr. Graf passed a uterine tube up the rectum, on account of the obstruction to the bowel caused by the pressure of the enormously-distended uterus; a copious extravasation of blood and swelling in the left half of the nates followed this operation; the patient was relieved. In March and April similar swellings recurred. In May the swelling, being very prominent, was opened, and an enormous quantity of dark syrupy blood escaped. The opening persisted, and became the permanent passage for the menstrual discharge. The previous distressing symptoms subsided.

4. Dr. Patry relates a case of the uterus performing its functions in the absence of vagina. A seamstress, aged seventeen, healthy, experienced for fourteen months periodical lumbar pains. For six or eight months her abdomen enlarged and breasts swelled. No trace of blood from vagina. Swelling with colic appeared. A hard, circumscribed tumour was felt rising two fingers' breadth above pubis. No vagina. By aid of a catheter in the bladder, an attempt was made to make an artificial vagina. The operation was resumed some days later. During forcing efforts of the patient a trocar was plunged into the tumour; a blackish fluid, not having a bad smell, escaped by canula, in quantity about two quarts. The abdomen became much less bulky. The artificial opening into the uterus became obliterated by neglect. Some weeks later another puncture was made, and the opening was enlarged by a bistoury. Symptoms of peritonitis arose, but were dispersed. The girl afterwards married, did not become pregnant, but experienced no further inconveniences.

II. Pregnancy.

1. *A Case of Pregnancy attended by Extraordinary Symptoms (Cervical Gestation).* By Dr. W. Hink. (L'Union Médicale, January, 1861.)
2. *A Case of Occlusion of the Os Uteri and Roof of the Vagina in a Pregnant Woman.* By Dr. Rotter. (Monatssch. f. Geburtsk., November, 1860.)
3. *On Complete Occlusion of the Cervical Neck in Pregnant Women, and the Necessary Operations.* By Dr. Depaul. (Gazette Méd. de Paris, No. 22, 1860.)
4. *A Case of Extra-Uterine Gestation.* By Dr. Nagel.
5. *A Case of Extra-Abdominal Gestation.* By Rektorzik. (Monatsschr. f. Geburtsk., December, 1860.)

1. The case related by Dr. Hink is a remarkable one; it illustrates the occurrence of gestation in a part of the genito-urinary canal which has very rarely been observed to serve that purpose. A woman, aged twenty-two, believed she was pregnant for the first time in December. She had enjoyed good health to that time, when, after a fit of violent anger, lumbar pains and metrorrhagia appeared. She was admitted into hospital with symptoms of puerperal metritis. Examination gave the impression of two tumours in the abdomen, one under the other. Through the os externum uteri the head of the child was distinctly felt. A fœtid discharge took place from the vagina, and irritative fever set in. Later, the patient felt something pass; a placenta the size of the palm of the hand, saturated with ichor, escaped. Attempts were made by the caoutchouc pessary in the vagina to provoke labour. An enormous quantity of fœtid ichor escaped when the pessary was withdrawn.

The fœtus was extracted by the hand; it was in a state of decomposition. The fingers then passed into a cavity full of ichor, larger than a child's head, situated in the middle of the abdomen. The patient only survived a few days. On dissection, the uterus was found consisting of two parts, quite distinct; the body of the volume of a hen's egg, which contained a remnant of placenta, and a cervix of which remained distinct only the posterior wall and the two lips of the vaginal portion; in the remainder of its periphery it was lost in the walls of the sac of ichor. The diagnosis pronounced by Rokitansky, to whom the particulars were submitted, was, "*Necrosis cervicis uteri dilatatæ et retento fœtu.*"

2. The following case illustrates the history and treatment of occlusion of the uterus:

Dr. Rotter's case. A woman, aged forty-eight, had borne five children normally. In the middle of the sixth pregnancy she was seized with pains in the region of the os uteri, and febrile movement. This continued without medical aid till term of pregnancy. She was then in a very cachectic state; the abdomen was enormously distended. It was ascertained that the uterus was quite occluded. Death followed in fourteen days. Dissection showed universal adhesion of peritoneum to uterus. The uterus contained eight quarts of purulent, stinking fluid, with many granular masses, leading to the belief that a fœtus had undergone fatty transformation. The inner surface of the uterus was excoriated; the mucous membrane easily separable; the os firmly closed, as well as the roof of the vagina. In the abdominal cavity was a collection of serous and plastic exudations.

3. M. Depaul, in relation to three cases of obliteration of the os uteri, considers the treatment proper to be adopted in this conjuncture. Two of his cases concerned pluriparæ, of whom one had been previously delivered by cephalotripsy, on account of pelvic contraction. Another suffered from vomiting; eclampsia set in; death following on the second day after labour.

The affection, according to Depaul, is always the result of adhesive inflammation of the lips of the os uteri, occasioned by preceding labours, surgical operations, cauteries, &c. The obliteration is found either at the inner or outer orifice of the uterus. Frequently the diagnosis can only be made after labour has begun.

The treatment is surgical, and must not be undertaken too late or too early. It consists in the formation of an artificial opening at the seat of the obliteration. By means of an ordinary hysterotome the uterine tissue is divided by light transverse cuts, the finger always guiding the depth of the incisions. The incisions must be widened by the finger. The operation is neither painful nor usually attended by much bleeding. Experience teaches that the artificially-made opening persists.

[It must be remembered that cases may occur where the Cæsarian section is preferable.—Rep.]

4. A woman, aged twenty-nine, had previously borne two children. She believed herself pregnant from the beginning of August, 1856, but six weeks later she perceived a great increase of size in abdomen, and difficulty in passing urine, which she had experienced in her first pregnancies; in the first half of October, discharge of blood, at first scanty, black, and lasting from four to six days at a time, took place; then in November the discharge was more profuse, with coagula and an offensive smell. This intermitting discharge of blood continued until January, 1857, when it became much less copious. From this date she had frequent acute pains in the abdomen, both in the upright and sitting postures; these were less on lying down. Lying on the back or on

the right side brought on violent pains in the hypogastric region. At the end of January, the patient felt the first fœtal movements. Pains likened to labour-pains frequently occurred, accompanied frequently by black, dirty blood, mixed with fibrinous and membranous shreds. On the 21st of March, she was admitted to hospital. She was in a hectic state. The abdomen was more enlarged on the right side; the umbilicus was drawn quite awry to the right; colostrum was squeezed from the breasts. The abdomen was excessively tender, and the abdominal muscles much stretched. These circumstances rendered minute examination impossible. Fœtal heart and uterine rush doubtfully heard, but fœtal movements plainly. The os uteri was directed to the right and forwards, lips soft, os open. At the posterior roof of vagina was felt a hard, round body, of the form, and apparently of the size, of a child's head; on firm pressure this body gave a crackling sensation, like a bladder blown out and then dried. No fœtal parts could be felt in the uterus. It was determined that the round body lay *behind* the uterus and before the rectum, a little to the left. It appeared that the body pushed the uterus forwards and to the right. By rectum the body was more completely felt; it filled the concavity of the sacrum. The diagnosis as to abdominal gestation was confirmed by Professor Virchow. Fever set in, and increased during the next two days, and the patient died on the 27th March. An abdominal section was made within half an hour. There flowed more than a quart of putrid fibrinous purulent exudation; there was seen a greyish-coloured sac projecting several inches above the pelvis. On opening this sac, no liquor amnii, but an intensely stinking gas escaped; the head of the dead fœtus lay in the pelvic cavity; the fœtus was female, fourteen inches long, and showed signs of decomposition. The autopsy was completed by Virchow. He found the sac continuously adherent to the abdominal wall; elsewhere, quite shut off from the abdominal cavity, it was adherent to the intestines. A large quantity of dirty, fœtid exudation in abdominal cavity. The uterus measured $5\frac{3}{8}$ inches in length, $3\frac{1}{2}$ inches in breadth at fundus, and $1\frac{3}{4}$ inch thick; the walls in high muscular development; the mucous membrane thick, blackish, and near the os internum beginning to be detached. The tubes and ovaries of both sides were enveloped in old adhesions, on the left, to the uterus; the left tube, much dilated, and strongly serpentine, ran directly into the sac.

5. Rektorzik relates a case of extra-abdominal pregnancy. A woman, aged thirty-two, remarked sometime after her fifth labour a painful swelling about the size of an egg in her right groin. This increased; menstruation stopped; and through pain the patient was obliged to keep her bed. In the beginning of June she felt movements in the swelling which she likened to the movements of a child. In the middle of October the normal term of gestation had arrived. The tumour then depended, somewhat stalked, from the groin, of an oval form, between the thighs down to the knees; it was forty-two centimetres long, twenty-five broad. At a spot near the right thigh a small body was definable, which was taken to be the right ovary. The parts of the fœtus and fœtal movements were plainly felt; the head lay in the upper part of the tumour. Auscultation revealed the dicrotous fœtal beat, and a sound synchronous with the mother's radial pulse. An incision 5″ long was made in the skin, whereupon the smooth surface of the peritoneal covering of the sac was exposed. Then an opening was made in the sac which served for uterus, and the child was extracted. The child cried, and survived. The placenta, on account of strong adhesions, could not be removed. There was no subsequent hæmorrhage. The mother was seized later in the day with vomiting, and died comatose in the evening. Dissection was refused. It was conjectured that the gestation had been performed in a uterine horn, which had fallen into a hernial sac.

III. Labour.

1. *Report of the Midwives' Lying-in Institution of Trier, from* 1854–60. By Dr. Fr. Birnbaum. (Monatsschr. f. Geb., Nov. 1860.)
2. *Report of the Lying-in Hospital of the Charité at Berlin during the two Winters* 1856–7, 1857–8. By Dr. Nagel. (Monatsschr. f. Geb., Dec. 1860.)
3. *Eclampsia during Labour; Pneumonia; Meningitis; Death.* By Dr. Gyoux. (Gaz. d. Hôp., No. 114. 1860.)
4. *A Case of Puerperal Convulsions.* By Professor Breslau. (Monatsschr. f. Geb., Dec. 1860.)
5. *A New Cranial Perforator.* By T. Gaillard Thomas, M.D. (Amer. Med. Monthly, Dec. 1860.)

1. Dr. Birnbaum gives an elaborate report of the Midwives' Lying-in Institution of Trier. During the period from January, 1854, to the 31st December, 1860, 565 pregnant women were admitted. The cases are elaborated with great statistical minuteness. We extract such observations as seem of special interest.

(1.) *As to still-born and living children:*

14 were born *dead-putrid.*
15 died during birth.
16 were feeble at birth.
38 apparently dead, recovered.
485 were born living.

[This statement is peculiarly interesting, as the information as to the causes and proportion of still-births is very scanty. This gives 1 still-birth to every 19 children born—a very unfavourable proportion.]

(2.) *Influence of nutrition of mother upon fœtus.*—The average weight of the children fell to the lowest point in the years 1855 and 1856—years of scarcity, when cider and wine failed, the prices of food and—through the impediments to water-transport—of firing rose to an extraordinary height, and the effects of the dearness were aggravated by scarcity of labour. The weight of the children rose again when the construction of a railway improved the rate of wages, and a concurrent fall in the price of wines and cider took place.

(3.) *Face-presentations.*—The face-presentations were about one per cent. Two proceeded naturally. Two were delivered by forceps; in both cases the forehead was forwards. In a primipara, a child weighing $8\frac{1}{2}$ lbs. was born alive, but died eight days later suddenly with a fracture of one parietal bone. In a primipara, a female child weighing 8 lbs. was born dead from spontaneous crushing of the skull and tearing of the liver. In this case there was a marked twisting of the whole skull. One was delivered by cephalotripsy. There was lordosis of the lumbar vertebræ with narrow pelvis.

Out of 18 face-births occurring from the foundation of the institution,

10 were in primiparæ, 8 in pluriparæ.
10 were boys 8 girls.

10 ran naturally, 1 with a putrid child, 1 with a dying one. Turning and extraction performed in 1 case, the child dying. The forceps was used in 6 cases, 2 children living, 4 dying. Cephalotripsy was performed once.

(4.) *The causes of death of children during labour.*—In 5 cases the bodies showed congestion in the cranium, pericranium, and meninges; also in the brain, with ecchymoses in the cranial bones, once with extravasation under the pericranium extending to the neck, with rupture of the sagittal suture from narrow pelvis. There was congestion with ecchymotic spots in the thymus, heart, and lungs; once with serous effusion in the pleura, similar injection of the intestines; in 2 cases, ecchymoses of the kidneys and mesentery. In all cases the bronchi were empty.

(5.) *Pelvic contractions.*—There were 46 cases of pelvic contraction, or in about eight per cent. of the whole. Twenty-seven labours ran a natural course; 5 were delivered by induction of premature labour; 2 by extraction by the feet; 1 by turning; 5 by the forceps; 2 by cephalotripsy. One mother died.

(6.) *The operations performed.*—Sixty-two operations of all kinds were performed upon 52 persons. Thus about 1 patient in 11 was the subject of an obstetric operation. The operations were:

Cutting dilatation of the os uteri	1
Scarification of the os uteri	5
Re-position of the cord	5
Induction of premature labour	5
Extraction by the pelvic extremity	8
Turning	4
Forceps to head	20
Cephalotripsy	2
Cæsarean section on the dead body	1
Detachment of the placenta	11

(7.) *Condition of the puerperæ, and mortality.*—*One* woman died of nervous apoplexy after severe face-labour. Fifty-three women suffered from various forms of pelvic inflammation; 9 of these died. The deaths are partly ascribed to the prevalence of an epidemic of puerperal fever of typhous character, and so contagious, that Dr. Birnbaum himself and a midwife were infected through the poison received from a post-mortem inspection.

2. During the summer, the Lying-in Hospital of the Charité, at Berlin, is devoted to clinical purposes, during the winter to the instruction of the female scholars of the Royal Institution for Midwives, of which Dr. Nagel is director. In the two winter half-years of 1856–7 and 1857–8 there were 672 labours, and 676 children were treated; 664 labours took place in the hospital. There were 633 varieties of head presentation, including two face; seventeen presentations of the pelvis or legs; and 4 shoulder presentations. Of 670 children, 65 were dead-born, or nearly 1 in 10 [a very large proportion]. Of these, 37 were boys and 28 girls. There died during the first ten days, 29 boys and 22 girls, or 1 in 13. 55 births were premature. The forceps was applied twenty times to the presenting head, and five times when head was born last. Cephalotripsy was resorted to once, the forceps having failed. The forceps was applied to the breech twice. Turning and extraction were carried out five times, and once turning was abandoned after unsuccessful trial. The labia majora were incised twice, to save the perineum from laceration. Thus the forceps was applied in 3 per cent. of the cases, turning in less than 1 per cent., and embryotomy only once in the 664 cases.

It is difficult to arrive at a clear view of the puerperal occurrences of the institution, owing to the practice of transferring patients in whom serious disease arises to other departments of the hospital, so that the conclusion of their history is lost sight of. It is stated that 16 women died. But it is not clear that this number includes all the deaths. We give a condensed history of some of the fatal cases.

1. *Hæmorrhage after Chloroform.*—A woman, aged twenty-nine, was admitted. The child presented in the first shoulder-position. Chloroform was administered, and turning effected by bringing down the right foot. Whilst the hand was still in the vagina there followed, under powerful contractions of the uterus, a profuse flooding, probably caused by premature separation of the placenta. The extraction of the child (which was dead) completed, the placenta was expelled, with a large quantity of blood. The uterus remained large and flaccid, and, spite of secale, acids, cold, &c., no contraction ensued, and the patient died an hour after delivery.

2. *Pyæmia.*—A woman, forty-eight hours after an easy labour, had severe shivering; uterus enlarged; pulse small, 130; ringing heart-sounds, the first sound protracted; on deep inspiration, acute pain in apex of heart; digitalis and acetate of potash given. Next day there was dulness in lower parts of the lungs; a brown sanies flowed from vagina; nymphæ were œdematous, and at introitus of vagina were puerperal ulcers; skin and eyes deeply icteric; next night delirium; the abdomen became distended, painful; then copious vomiting of a green matter. The patient died on fifth day after labour. Dissection showed a yellow, firm coagulum in right heart, with white points resembling pus; the lower part of left lung infiltrated; a coagulum in the arteries of the lower lobe of right lung; at fundus uteri gaping venous mouths; the veins filled with air, especially the left internal spermatic; a fibrinous purulent exudation on peritoneum.

3. *Metritis.*—In January, 1857, many cases of metritis occurred. They had all a typhoïd type, characterized by prostration, pelvic pains, slight affections of the peritoneum, stupid expression of countenance, ringing in the ears and deafness; the fever was comparatively slight, and was distinctly remittent. Only in three cases was life endangered. Two ended fatally. In both, bronchitic affections arose, and were followed by hypostatic pneumonia; exhausting diarrhœa appeared; and death on the ninth day. In one case, dissection showed purulent absorption through the uterine veins, leading to general purulent infection. Metastatic abscesses were not found.

3. The following case of eclampsia, related by Dr. Gyoux, illustrates the connexion between albuminuria and cerebral symptoms. The patient was seen in labour, with a pulse of 140; head presenting; eclamptic fits occurred to the number of thirty-seven in twenty-four hours; deep coma; pains ceased; she was bled; and delivered by forceps of a dead eight-months' child. The fits continued, but were at length allayed by chloroform. Three days later there was pneumonia of the left side; this was quickly complicated with meningitis, and death followed on the fourth day after delivery.

4. Professor Breslau relates a case of puerperal convulsions, of which we give a summary for the sake of the opportunity the case afforded of making an analysis of the blood. A woman, aged thirty-nine, was pregnant for the fourth time. She was robust and well-nourished. Her first three labours had been easy. This time considerable œdema appeared in the last month; at this time there was no trace of albumen in the urine. Professor Breslau kept her in bed, and made "involutions" with long rollers from below upwards, and ordered infusion of ipecacuanha with tartrate of borax. The swelling of the feet quickly diminished concurrently with increased diuresis, so that on the approach of labour there was but little œdema. On the 1st of May, a child, weighing $7\frac{3}{8}$ lbs., was born easily. The first five days passed well; the patient suckled her child. On the sixth day she left her bed for a few hours; headache followed, and bilious vomiting. At night the first eclamptic attack broke out. A second attack, with marked determination of blood to head, followed. Sixteen ounces of blood were drawn. Consciousness did not return, and fourteen eclamptic fits occurred. More blood was drawn by lancet and leeches on the temples. The urine, withdrawn by catheter, contained abundance of albumen, and deposited lithic acid. The blood had an alkaline reaction; no evidence of ammoniacal salts, especially of carbonate of ammonia; no excess of urea; sugar in minute quantity.

On the eighth day consciousness returned; the urine of this period contained *scarcely any albumen.* Next day there was none. On the eighteenth day the patient had recovered; but the left pupil was still much dilated.

5. Dr. Gaillard Thomas describes a new cranial perforator. This

instrument is in fact a screw with a guarded perforator, the principle being adapted from the *lithotome caché*. It consists of a steel tube, ten inches long, which ends in a screw and hides within itself a cutting blade, which is thrown out of its bed by the hand of the operator compressing the handles. Two inches from the extremity a shoulder is placed, which prevents its entering the child's head too far, and the blade is fixed upon its pivot by a slot, which renders its removal easy for cleansing. The inventor indicates, amongst the advantages, the facility of introduction, the screw readily catching the scalp and penetrating the skull; and the graduation of the cutting part of the instrument.

IV. Lactation.

The Uterine Douche in the Treatment of Galactorrhœa. By Dr. Abegg. (Monatsschr. f. Geb., Dec. 1860.)

Dr. Abegg, referring to the influence of excitation of the breasts upon the uterus, conjectures that a corresponding influence may be exerted by excitation in the opposite direction—namely, from uterus to breasts. He has applied his idea to the arrest of galactorrhœa. A woman, aged thirty-one, always in good health, was delivered of her first child two years before, and had suckled it until its death when nine weeks old. On the 28th October, 1854, she was again delivered of a weakly child that died on the 1st of December following. She had weaned the child on the 15th November on account of sore nipples. Galactorrhœa continued, in spite of iodine, iron, and compressive bandages, until the middle of January. Menstruation had not returned, but there was profuse leucorrhœa. On the 3rd February the warm douche was applied for 15″, and repeated until the 14th; on the 15th a slight discharge of blood appeared from the uterus, and lasted fourteen days. During this time the secretion of milk gradually disappeared. Menstruation became re-established. Dr. Abegg relates a second case in which the douche seemed equally efficacious. [It deserves to be considered whether the anxiety generally evinced to suppress the milk after the premature loss of the child is rational. There are good physiological reasons for permitting the breasts to continue in functional activity if so disposed, sparing the ovarian function, promoting the involution of the uterus, and postponing impregnation. It is certainly not wise hastily to interfere with the course of Nature in such cases. When necessary to interfere, the uterine douche will probably be a valuable remedy.—Rep.]

MEDICAL INTELLIGENCE.

The late W. Baly, M.D., Physician to the Queen.

Since the issue of our January number the profession has had to mourn the untimely loss of one whose amiable, unobtrusive character, sound judgment, and practical experience endeared him to many true friends. We fully purposed to have given to our readers a detailed sketch of Dr. Baly's life and career; but as an able and accurate one (from the pen, as we have learnt, of his friend and colleague, Mr. Paget) was presented to the public shortly after his death through the medium of the medical weekly press, we shall (and this without apology) content ourselves with merely transcribing one or two such extracts from this short memoir as may best suffice to present to the minds of those who did not enjoy his acquaintance or his friendship a faithful picture of one so eminently and so worthily beloved as a physician and a man.

Passing over the interesting notice of the circumstances of his early years, his active and earnest pupil-life in London, his studies at Paris, at Heidelberg, and at Berlin, where he graduated in 1836, we proceed to view him commencing practice in London as a physician, and engaging himself so sedulously in furnishing to the profession that admirable translation of Müller's 'Physiology,' copiously adorned by his own original observations, to which, in our first article of this number (p. 292), we alluded. We afterwards find him receiving the appointment to visit and report on the Milbank Penitentiary, with its arduous duties and rare opportunities; an appointment which he retained for nigh twenty years, and which furnished material for his well-known paper on the Diseases of Prisons, in the 28th volume of the 'Transactions of the Royal Medico-Chirurgical Society;' for the Gulstonian Lectures on Dysentery in 1847; and for the 'Report on the Cause and Mode of Diffusion of Epidemic Cholera,' at the instigation of the Cholera Committee of the Royal College of Physicians, in 1853. Subsequently, Dr. Baly became Lecturer on Forensic Medicine at St. Bartholomew's Hospital; after that, F.R.C.P. and F.R.S.; then Assistant-Physician, and then Lecturer on Medicine, at St. Bartholomew's Hospital. Finally, in 1859, he was called upon to assume the high and responsible office of Physician Extraordinary to the Queen, and to be in constant attendance on the Royal Family.

"Thus [proceeds Mr. Paget] had Baly risen, till the greatest honours of his profession seemed quite within his reach: more than his ambition might soon be satisfied: he appeared to stand firm in the confidence of his Sovereign, in the high esteem of all his brethren, in the affection of many friends: the pride of sisters who had worked lovingly with him in his laborious days. He was glad in his success, brightened by prosperity; but, from all this happiness, in a moment he was hurled to death;—yet not to death; for they who knew his inmost mind are sure that, in the midst of this life, he was preparing himself for another and far better.

"It will be asked hereafter, what was the secret of Baly's singular success? Truly, there was no secret in it. He had excellent intellectual capacity, and great working power; he was very prudent, gentle, true-hearted, and virtuous; and he daily cultivated and used all his powers, with the single view of doing the best he could in the daily duties of his life. His intellectual strength was not in any one of his mental powers, but in the fair proportions of them all. He had keen senses; very clear observant power; great caution and sobriety in thinking; an excellent ability in analysis. Few could sift facts better than he, or bring them out with more clearness and simplicity; he was eminently clear-headed. And all that he did—his practice and his lectures, his writings and his conversation—represented this character of his mind; all was clear, simple, definite, and orderly.

"An excellent feature in his disposition was a corresponding simplicity and sincerity of heart. He never professed to be what he was not; nay, he never professed himself what he was; he did well, without any self-assertion, and seemed content to do his duty without claiming or expressly seeking for its reward. Doubtless, among the motives that impelled him to a laborious exertion of his powers, ambition had some place; but his ambition was happily tempered by a natural timidity which made him always prudent. Hence, probably, it was that he never committed the faults of mere ambition; he never wasted time or power in striving after things which he was unfit for, or unlikely to attain; he never sought for praise or reputation in pursuits that were alien from his proper work; and when his ambition was gratified, he had the same prudent fear lest he should fall short of the full discharge of the responsibilities which high station and opportunities of doing good brought with them. To the last, he never made light of duty; he never thought it easy to do well; he did not presume that he had yet done his best; he seemed to think that there was in store some far better thing for him to do.

"It will readily be believed that Baly was very popular among his pupils and professional brethren. His popularity, like his success, came to him without being expressly sought. It was the natural result of his honesty, gentleness, and liberality, and it increased with his merits. He tolerated kindly the faults of others; he saw them clearly enough, but he did not resent them; he spent his life without a controversy, and among the crowds that followed him to the grave, there were many who had loved him and had rejoiced in his prosperity, though he was their successful rival.

"These were the plain reasons of Baly's prosperous career; and the best lesson which our profession may read in his life—yet not safely, till they have well studied the solemn lesson of his death—is, to believe in the sufficiency of intellectual and moral excellence for the attainment of great success. He entered the profession by the broad and most frequented road; he started in the crowd, with neither wealth nor costly education; he began life without any advantage of social position, without an influential friend; he had no brilliant genius, no lucky gift of cleverness. Yet his career was a constant success; and, in its course, he gained and learned to use as many of these aids to progress as he desired. He constantly cultivated and adorned his mind, and made friends in every rank of life—literally from the prison to the palace—and when he died, the grief was as wide-spread. It was the grief not only of bereavement but of disappointed hope: for all felt that had Baly been permitted to live long he would have achieved great things for his profession and the world."

Such is the graphic, but unaffected and truthful delineation of a character from the pen of one whose powers of observation, and whose close, long-continued, and affectionate intimacy with its possessor, entitled him, perhaps before all others, to express an opinion. The picture is complete, and we can only in conclusion express sincere thanks to Mr. Paget for supplying to us so touching and so invaluable a memoir of him whose shocking death we so deeply lament.*

Dr. Matthew Baillie.

In noticing a book (in our January number, page 91) which treated mainly of the sayings and doings of our professional brethren of bygone days, we inadvertently, and without correction, gave currency to a statement asserting that Dr. Matthew Baillie, as a young man, was "plucked" at the Royal College of Physicians, under the presidency of Sir Hans Sloane, and that in consequence he had allowed himself in most hasty and unjustifiable conduct. Both these assertions, which occur in Mr. Jeaffreson's work (above alluded to), have been lately fully and satisfactorily contradicted, as well by his friend Dr. Latham, in a letter to the 'Lancet' (March 9th), as by Mr. Baillie (son of the doctor) in the 'Athenæum' (March 23rd).

Mr. Jeaffreson has also acknowledged ('Lancet,' March 16th) that in this matter he had made a blunder, having borrowed the anecdote from the pages of a work styled 'Physic and Physicians,' without attempting to verify it.

Sir Hans Sloane ceased to be President to the College, as we have read in its annals, in 1735, and Dr. Matthew Baillie, as we find from a copy (belonging to Mr. T. A. Stone) of a most interesting autobiographical MS. memoir in the possession of Dr. Baillie's son, was not born until the year 1761.† It is, then, quite evident that the anecdote of a young man named

* We cannot forbear noticing a notice of Dr. Baly in the 'Edinburgh Medical Journal' for March, of great importance to the right understanding of Dr. Baly's character, inasmuch as it is from the pen of one of his oldest friends and fellow-pupils (we believe), Dr. Benjamin Bell, of Edinburgh.

† Another transcript of this autobiographical sketch may be seen in the library of the Royal College of Physicians.

Baillie, afterwards of celebrity in his profession, having been rejected at his college examination under the presidency of Sir Hans Sloane (possibly true of some one else), could not refer to the nephew of John and William Hunter. Again, the supposition that Dr. Matthew Baillie was at any time plucked at the College is completely set aside by reference to the College annals, for, as we have ourselves found in examining those annals (by the kind permission of Dr. Pitman, the registrar), it is clearly stated that in each of the three examinations in the only year (1789, Sir G. Baker being president, and Drs. Turton, Milman, Carmichael Smith, and Allston being censors), in which Baillie went up for examination, he was approved, and subsequently ballotted for and admitted a "candidatus;" the Fellowship following in the year after.

This being the case, we gladly embrace the earliest opportunity of correcting an allegation, which, if relied upon, would tend materially to disparage the character of one whom posterity, equally with his contemporaries, holds in the very highest estimation and respect.

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OF THE

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

END OF VOL. XXVII.

ANNUAL REPORT OF CASES ADMITTED INTO THE MEDICAL WARDS OF ST. GEORGE'S HOSPITAL,

During the Year ending Dec. 31*st*, 1859.

By W. H. Dickinson, M.B. Cantab,

Medical Registrar and Demonstrator of Anatomy at St. George's Hospital; Assistant-Physician at the Hospital for Sick Children.

Cases admitted during the year 1859.						During nine years.	
Nature of Disease.	Total number of admissions.	Total number of deaths.	Per-centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admissions.	Per-centage of mortality.
1. Fevers:							
Continued (typhus and typhoïd)	31	3	9·7	3	2	1157	11·4
Slight	17	...	...	2	...		
Remittent	1	...	...	...	...	...	...
Cholera	3	...	...	...	...	146	48·3
Influenza	...	...	...	...	...	93	...
2. Eruptive Fevers:							
Measles	4	...	...	1	...	34	...
Scarlatina	23	4	17·7	1	...	131	12·1
Small-pox	2	...	...	1	...	20	...
Erysipelas	15	3	20·0	3	1	202	15·3
Chronic erysipelas	4	...	...	...	...		
3. Intermittent.							
Quotidian	8	...	...	2	...	44	...
Tertian	8	...	...	3	...	80	...
Quartan	2	...	...	...	...	9	...
Irregular	1	...	...	...	...	25	4·0
4. Rheumatism:							
Acute	86	4	4·4	26	4	592	4·8
Subacute	71	3	4·2	13	3	889	0·9
Chronic	140	1	0·7	19	1	1191	0·9
5. Gout and rheumatic gout	35	...	...	4	...	310	4·1
6. Poisoning:							
Irritant	3	1	33·3	2	1	17	17·6
Narcotic	2	2	100·	...	...	19	15·7
Syphilitic and gonorrhœal	6	1	16·6	6	1	96	1·0
7. Colica pictonum	15	...	...	1	...	73	...
8. Intestinal worms	1	...	...	...	...	37	5·4
Echinococcus	...	...	...	...	...	2	100·
9. Dropsies:							
Anasarca	47	22	46·6	46	22	898	34·3
Ascites	27	15	57·4	23	15	159	47·8
Hydrothorax	11	11	100·	11	11	...	...
Hydropericardium	3	2	66·6	2	2	...	...
10. Hæmorrhages:							
Epistaxis	3	...	...	1	...	38	10·5
Hæmoptysis	12	2	16·6	10	2	168	23·2
Hæmatemesis	13	...	...	5	...	71	7·0
Hæmaturia	12	5	41·6	12	5	64	18·7
Intestinal	6	1	16·6	6	1	53	18·8
Uterine	8	...	...	...	...	102	2·9
11. Purpura	5	2	40·0	2	1	52	25·0
Scurvy	...	...	...	...	...	10	...
12. Anæmia and debility	57	1	1·7	17	1	544	2·2
13. Chlorosis	18	...	...	6	...	144	0·6
14. Cachæmia	10	8	80·0	10	8	65	47·6
15. Scrofula	4	2	50·0	4	2	20	30·0
16. Tubercular diseases:							
Phthisis	130	52	40·0	72	39	1137	34·7
Tabes mesenterica, &c.	9	7	77·7	6	6	11	72·7
Tubercle in brain	...	...	...	...	...	23	86·9
Hydatids	...	...	...	...	...	14	57·7

Cases admitted during the year 1859.						During nine years.	
Nature of Disease.	Total number of admissions.	Total number of deaths.	Per-centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admissions.	Per-centage of mortality.
17. Malignant growths:							
Encephaloïd	7	7	100·	3	3	101	75·2
Scirrhus	13	3	23·0	3	2	164	22·5
Of bone	1	1	100·	1	1	3	100·
Colloïd	...	...	...	...	...	4	100·
Epithelial	...	...	...	...	...	4	50·
18. Hysteria	39	...	...	12	...	458	0·02
Hypochondriasis	3	...	...	...	...	...	...
19. Chorea	28	...	...	1	...	190	2·1
20. Delirium tremens	17	3	17·5	6	2	144	22·2
21. Tetanus	1	...	...	...	...	4	50·0
22. Brain and cord:							
Encephalitis	6	4	66·6	4	4	83	86·7
Chronic disease of brain	27	6	22·2	5	2	97	28·8
Epilepsy	9	...	...	3	...	208	17·3
Apoplexy	8	4	50·0	3	2	68	66·1
Functional disturbance	10	4	40·0	5	1	287	4·5
Sun-stroke	1	1	100·	...	...	1	100·
Insanity	8	...	...	1	...	?	...
Inflammation of cord	2	2	100·	...	...	20	70·
23. Paralysis:							
Hemiplegia	40	6	15·0	7	3	259	10·
Paraplegia	17	3	17·6	3	...	139	13·6
General paralysis	3	...	...	1	...	10	10·
Local paralysis	5	...	...	...	...	99	5·0
24. Neuralgia:							
Of face and head	4	...	...	...	...	30	...
Sciatica	23	...	...	3	...	133	1·5
Angina pectoris	1	...	...	...	...	5	...
Undescribed	3	...	...	...	...	...	...
25. Heart:							
Pericarditis	11	4	36·3	11	4	141	35·4
Endocarditis	9	1	11·1	9	1	134	11·2
Hypertrophy	26	20	76·9	26	20	211	61·1
Dilatation	10	5	50·0	10	5	135	53·3
Fatty degeneration	3	2	66·6	3	2	...	...
Valvular disease	84	15	17·8	48	13	551	24·3
26. Diseases of bloodvessels:							
Aneurysm	13	6	46·1	7	4	59	49·3
Phlebitis	7	...	...	1	...	51	25·4
Ulceration of aorta	1	1	100·	1	1	...	...
27. Diseases of respiratory organs:							
Laryngitis	4	2	50·0	1	...	67	52·2
Tracheitis	4	4	100·	2	2	19	68·4
Bronchitis	94	19	20·2	38	14	1111	17·7
Pneumonia	31	17	54·8	21	16	435	34·7
Gangrene	1	...	...	...	...	...	...
Pulmonary apoplexy	3	3	100·	3	3	...	...
Vesicular emphysema	9	5	55·5	9	5	126	37·3
Pleurisy	42	8	19·0	19	8	469	31·3
Empyema	1	1	100·	1	1	...	...
Pneumothorax	2	2	100·	2	2	11	81·8
Asthma	1	...	...	1	...	24	...
Hooping cough	1	...	...	...	...	14	28·5
28. Diseases of mouth and pharynx:							
Glossitis	...	...	...	...	...	3	...
Quinsey	6	...	...	1	...	91	1·0
Cynanche pharyngea	16	...	...	...	...	...	...
Relaxation of throat	2	...	...	2	...	...	...
Diphtheria	9	4	44·4	4	3	13	53·8
Mumps	...	...	...	...	...	19	5·2
Enlarged tonsils	4	...	...	1	...	41	2·4
Ulceration	8	1	12·5	2	...	29	55·1
29. Diseases of stomach and œsophagus:							
Dyspepsia	39	...	...	5	...	533	1·3
Ulceration	2	...	...	1	...	23	65·2
Stricture (œsophagus and pylorus)	10	...	...	...	...	24	8·3

Cases admitted during the year 1859.						During nine years.	
Nature of Disease.	Total number of admissions.	Total number of deaths.	Per-centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admissions.	Per-centage of mortality.
30. Diseases of intestinal canal:							
Enteritis	2	...	...	1	...	6	...
Constipation	14	1	7·1	2	1	335	1·1
Obstruction	3	2	66·6	...	...	13	53·0
Diarrhœa	8	...	...	1	...	273	8·4
Dysentery	6	1	16·6	...	...	49	28·5
Ulcerations	11	9	81·8	8	8	39	94·8
Tympanitis	2	...	...	...	...	22	...
31. Diseases of peritoneum:							
Acute peritonitis	10	7	70·0	7	7	181	41·9
Chronic peritonitis	20	6	30·0	12	6	114	39·4
32. Diseases of liver and gall-bladder:							
Congestion	3	...	...	1	...	77	15·5
Cirrhosis	9	5	55·5	4	3	83	73·4
Enlargement	15	7	46·6	4	3	95	28·4
Abscess	3	3	100·	3	3	9	...
Gall-stones	1	...	...	...	...	11	36·3
Jaundice	14	2	14·2	6	2	146	21·9
33. Diseases of pancreas	...	...	...	...	...	...	...
34. Diseases of spleen	6	2	33·3	4	2	38	39·2
35. Diseases of urinary organs:							
Albuminuria	96	37	38·5	43	27	875	42·1
Abscess of kidney and pyelitis	2	1	50·0	...	...	17	41·1
Malignant diseases of kidney	1	1	100·	...	...	...	...
Renal calculus	2	1	50·0	1	1	...	...
Supra-renal disease	2	2	100·	2	2	4	75·0
Cystitis	4	...	...	2	...	48	16·6
Ischuria	3	1	33·3	2	1	7	57·1
Diuresis (without albumen or sugar)	1	...	...	...	...	9	...
36. Diabetes	4	1	25·0	...	...	45	17·7
37. Ovarian disease	18	5	27·2	6	3	140	12·8
38. Diseases of uterus, &c.:							
Amenorrhœa	18	...	...	6	...	162	2·4
Leucorrhœa	2	...	...	...	...	96	...
Menorrhagia	8	...	...	...	...	102	2·9
Displacement	5	...	...	2	...	59	...
Inflammation of uterus or appendages	5	...	...	2	...	39	...
Tumours (non-malignant)	7	...	...	1	...	68	8·8
Polypus	2	...	...	...	...	...	...
Cancer	11	3	27·2	2	2	...	...
Vesico-vaginal fistula	1	...	...	...	...	...	...
Diseases of external organs	5	1	20·0	2	1	27	3·2
39. Diseases of bones and joints	19	6	31·5	14	5	105	25·7
40. Diseases of skin, &c.:							
Erythema	5	...	...	2	...	106	2·8
Urticaria and roseola	1	...	...	...	...	27	7·4
Lichen and prurigo	...	...	...	...	...	28	...
Scaly eruptions	5	...	...	...	...	84	...
Vesicular eruptions	6	...	...	...	...	109	1·8
Pustular eruptions	10	...	...	2	...	47	...
Pompholyx and rupia	...	...	...	...	...	15	6·6
Cellular inflammation	3	1	33·3	2	1	107	28·0
Superficial abscess	2	...	...	...	...		
Diseases of absorbent glands	3	2	66·6	3	2	...	...
41. Diseases of muscles	1	...	...	1	...	5	40·
42. Anomalous and accidental cases	14	...	...	...	...	65	9·2

Remarks.

Passing in brief review the several classes of the annual register, it will be seen that several slight modifications have been effected in the subdivisions of disease. These will be sufficiently apparent in their own places, and need no notice here. It has been deemed advisable to avoid any serious alterations, reserving such for the commencement of the next decennial period. The present table completes the ninth year.

1. *Fever.*—The well-marked cases of typhoïd or typhus have been distinguished from cases of slight fever, which are now placed in a separate class. One case was fatal where the disease came on immediately after acute rheumatism with pericarditis; another was complicated with pneumonia and ulceration of the bowels.

The cases of cholera were rather of severe English than of true Asiatic.

2. *Exanthemata.*—The only complicated case of measles was with syphilis. Of the fatal cases of scarlatina, one terminated as the patient was apparently recovering, with sudden profuse hæmorrhage from the throat. This apparently proceeded from a deep sloughing ulcer on one tonsil, although its source could not be positively demonstrated after death. Another died in epileptic convulsions coming on suddenly five weeks after the outset of the disease. There was no other symptom of renal affection during life, and as there was no post-mortem, the connexion of the symptom with the state of the kidneys was only a matter of conjecture.

In a third case which terminated fatally the cuticle became raised into large blisters, in many places, by the exudation beneath it of opaque purulent matter.

The erysipelas which proved fatal in the case stated as complicated came on when the patient was in a state of extreme exhaustion from parturition and want of food. She had at the same time lobular pneumonia and an early condition of phthisis.

3. *Intermittent Fever.*—The diseases associated with ague were various, and apparently bearing no relation to that disease excepting in one instance of enlarged spleen. In one case there was phthisis, in another albuminuria, in another valvular disease of the heart, and in the last bronchitis.

4. *Rheumatism.*—The morbid conditions associated with acute rheumatism were, except in four instances, concerning the heart. In the 86 cases recorded there was evidence of some affection of that organ in 23.

Pericarditis . . . . in 7
Endocarditis (without pericarditis) in 8
Previous valvular disease . . . in 9
(in one of which pericarditis supervened).

Four cases described as acute rheumatism were fatal indirectly. In one there was very extensive affection of the heart involving both the lining and investing membranes. In one there occurred pyæmia, either as the result of the rheumatic affection, or as an independent disease producing symptoms which were erroneously regarded as rheumatic. In the third, typhoïd fever supervened closely upon the acute rheumatism, while in the last that affection was combined with delirium tremens.

Sub-acute Rheumatism.—In the 3 cases of this disorder stated as fatal, that result was due in one case to phthisis, in another to pyæmia, in the third to disease of the joints.

The complications in the other cases were thus distributed:

Disease of the bones or joints, 4
Valvular disease of the heart, 2

beside erythema nodosum, chronic peritonitis, phthisis, epilepsy, and anæmia, each in a single instance.

Chronic Rheumatism.—In the only case entered under this head which proved fatal, the symptoms were due to malignant disease of the bones. The remaining complications were many and various. Valvular disease of the heart occurred four times; phthisis, thrice; disease of the joints, twice.

5. *Gout.*—In consequence of the cases having been entered without the observance of a sufficiently rigid distinction between chronic gout and rheumatic gout, the registrar has thrown these two forms of disease into one statement as the only means of avoiding error.

The complications were neuralgia, delirium tremens, hemiplegia, and albuminuria.

6. *Irritant Poisoning.*—One of these cases was that of a child one year and nine months old, who was said to have swallowed hydrochloric acid. Diarrhœa was the only observed result. In another, white precipitate was taken without much effect. In the only fatal case, a large number of compound colocynth pills were self-administered. These were followed by diarrhœa, ulceration, and finally perforation of the large intestine.

Narcotic Poisoning.—Both these cases ended fatally. They were both supposed to have been suicidal. In one half a drachm of the essence of bitter almonds had been taken; in the other the poison was presumed to have been morphia or some preparation of opium.

The Divisions 7 and 8 need no comment.

9. *Anasarca.*—This affection occurred independently in one doubtful instance. The symptoms came on after scarlatina, but without the presence of albumen in the urine. The other causes of dropsical effusion into the cellular tissue were thus divided among the remaining 46 cases:

Disease of the kidneys, displayed after death, or indicated by persistently albuminous urine, in	34
Disease of the heart, in	16

(In 9 of which both affections occurred together.)

In 8 there was some affection of the lungs, mostly bronchitis; but this in every instance was associated with some affection of the heart or kidneys.

In 4 the anasarca was attributed to anæmia.

In 1 it occurred in conjunction with ascites, as a consequence of disease of the liver. In every fatal case, either the heart or the kidneys were affected. The deaths were thus distributed:

Anasarca, purely	cardiac,	fatal in	5
„ „	renal,	„	9
Combined		„	8

Ascites.—In the 27 cases the cause was not clearly made out in 5.

Disease of the	liver	was present in	11
„	kidneys	„	8
„	heart	„	5

Disease of the uterine organs	„	3
„ peritoneum	„	2

Diseases of the heart and kidneys were combined much in the same way as with anasarca.

The classes hydrothorax and hydropericardium have been introduced for the first time in the present table; so that dropsical effusion into the pleura or pericardium is now distinguished from pleurisy or pericarditis.

Hydrothorax.—This affection was oftener found after death than expected during life. It was in every instance the result of disease of the heart or kidneys, save in one case where it came on in conjunction with ascites during the last stage of malignant disease of the ovary.

The two cases of *Hydropericardium* which terminated fatally were associated with other varieties of dropsy, as the consequence of disease of the heart or kidneys. In the other example, the state was probably owing to pericarditis, although its origin remained a matter of uncertainty.

10. *Epistaxis* was due in one case to disease of the heart, in the two others its cause was not made out.

Hæmoptysis.—Of the 12 cases of this affection there was unmistakeable evidence of phthisis in 7. In 1 it appeared vicarious of menstruation. The remaining 4 were supposed to depend upon phthisis, but this was without proof.

Hæmatemesis.—Two were supposed to depend upon hepatic disease, one upon ulceration of the stomach, and one upon amenorrhœa. In the remaining instance the cause was a matter of uncertainty.

Hæmaturia.—In 1 of the 12 cases here enumerated, the symptom was supposed to depend upon a calculus in the kidney, all the rest upon disease of the kidney, with persistent albuminuria. All the fatal cases were from the latter cause. It may be as well to mention that no cases have been classed as hæmaturia, unless the blood was in sufficient quantity visibly to discolour the urine. Had those been included where a few blood globules were found by microscopic examination, the number would have been much larger.

11. *Purpura.*—There occurred one fatal case of purpura, where there was no other lesion to account for death. The patient sank in a condition much resembling typhoïd fever. In another, which terminated fatally, the result was due to a complication of disease, the heart, kidneys, liver, and lungs being affected.

12. *Anæmia and Debility.*—In this class have been included, not only cases of simple anæmia, but those of debility, which were not strictly owing to this cause, but which could not be conveniently placed elsewhere; for instance, prostration originating in the nervous system from drinking, &c. The only fatal case was that of a woman who had suffered from starvation after parturition. The immediate cause of death was erysipelas. A more particular notice of her case will be found elsewhere.

13. *Chlorosis.*—This affection has been regarded as a symptom of amenorrhœa, and therefore requires no particular notice in this place.

14. *Cachæmia.*—Here are included only those cases where the blood

was altered by purulent infection, or by a superabundance of white corpuscles as the result of splenic disease. They were thus divided: pyæmia in 7; leucocythæmia in 3. All the former were fatal, and one of the latter.

15. *Scrofula.*—In all the cases thus described the bones or joints were affected. The majority of such cases necessarily come under notice in the surgical wards. Both the fatal cases had dropsy and renal disease.

16. *Phthisis.*—This was associated with bronchitis, pneumonia, or pleurisy, mostly consequent upon the tubercles, in 24 instances. In one case with emphysema, and in two with pneumothorax. In 10 of the cases disease of the kidneys also existed.

Tabes Mesenterica.—This term is used to include all instances of tubercular disease of the peritoneum, whether confined to the mesenteric glands or not. In 4 of the fatal cases peritonitis was present. In another the cause of death was meningitis, the patient being at the same time phthisical.

17. *Scirrhus.*—This disease affected the uterus in all the instances recorded, save two.

Encephaloïd.—This was divided among the liver, the kidneys, the bones, and the intestines, in the order as to frequency in which they are mentioned.

18. *Hysteria.*—Males were the subject of well-marked hysteria in 2 cases. The affection was only noticed in connexion with amenorrhœa in 3 instances.

19. *Chorea.*—These cases were all uncomplicated, except 1, where hysteria co-existed.

20. *Delirium Tremens.*—Of the 17 cases, a woman was the subject of the disease in one. Two of the fatal cases are stated as complicated; in 1 pneumonia was found after death, probably quite secondary in its origin; in the other, the disease came on after the patient had been attacked with acute rheumatism.

21. *Tetanus.*—One well-marked case of this disease in an idiopathic form occurred. It was preceded by some affection of the throat, which was supposed to have been diphtheria. Recovery took place, although the patient obstinately refused all medicine.

22. *Brain and Spinal Cord.*—In each of the 4 fatal cases of encephalitis, tubercular deposit was found in the lungs or bronchial glands. Six cases of chronic disease of the brain proved fatal; a tumour was found in 2, softening in 4. The complications were few, and comparatively unimportant.

In the 4 cases of functional disturbance which are recorded as fatal, death occurred in 3 from renal disease; in the fourth, as was supposed, from starvation.

23. *Paralysis.*—Many cases of hemiplegia and paraplegia (7 of the former, 2 of the latter) have necessarily been enumerated in the preceding class. Those cases of hemiplegia where there was no evidence of softening or tumour, and where there had been no marked apoplectic fit with loss of consciousness, appear only under the present heading.

Cases of paraplegia without evidence of inflammation of the spinal cord, in which, in short, paralysis was the only symptom, are in like manner not recorded elsewhere.

The 3 cases described as general paralysis were, in fact, of paraplegia beginning at the arms, and the symptom was supposed to result in each instance from inflammatory action within the spinal canal.

25. *Heart.*—The relation of the affections of the heart and its membranes to rheumatism has been adverted to already. Pericarditis occurred with acute rheumatism seven times, with pleurisy twice, and twice as the sequel of long-standing hypertrophy and valvular disease. Endocarditis was not recognised, except as the associate of acute rheumatism. Hypertrophy of the muscular structure was noted in 16 cases in conjunction with some other morbid state of the organ, in 12 with albuminuria. Taking the whole number of cases of hypertrophy, dropsy was present in 12. In the instances of dilatation, that symptom was present in 5, thus occurring with about the same frequency in either case.

Two cases of simple fatty degeneration of the heart terminated in sudden death while in the hospital. Both were complicated with phthisis.

Valvular disease was associated with some other alteration of the heart, generally consequent upon it, in 18 cases; with persistent albuminuria in 9; with a notable degree of dropsy in 10; with some pulmonary affection in 8.

26. *Bloodvessels.*—Of the 6 fatal cases of aneurysm, 4 were of the thoracic aorta; 2 of these terminated by bursting, 1 into the pericardium, 1 into the left pleura. Another case was of the innominate, the patient dying apparently of bronchitis. In the remaining case, the precise seat of the disease could not be discovered. There existed extensive hæmorrhage behind the peritoneum, which was supposed to have resulted from the rupture of a small aneurysm connected with one of the smaller branches of the aorta.

In 2 of the cases of aneurysm, phthisis also existed.

A remarkable case was admitted, where the patient had an abdominal tumour, mistaken during life for an enlarged spleen. This proved to be a mass of blood which had escaped from a small ulcerated opening in the abdominal aorta. The adjacent vertebræ were carious.

27. *Lungs, &c.*—Two of the cases of laryngitis were chronic, 2 acute. In both of the latter the operation for laryngotomy was performed, and both terminated fatally. In one of them the larynx was not opened until the patient was *in articulo;* in the other, the patient survived the operation for twenty-four hours, and then sank without any assignable cause.

Of the 4 cases of tracheitis (all of the female sex), 1 was an ordinary case of croup in a child two years and nine months old, which proved fatal after the operation.

All the remaining cases were regarded as diphtheria, and treated with stimulants; but since the changes discovered after death were characteristic of croup, they are necessarily enumerated under this heading.

One occurred in a child two years old. A small patch of lymph was seen upon one tonsil, which led to the diagnosis. After death false membrane was found coating the lower inch of the trachea, and filling up the right bronchus and all its ramifications.

In the next case, the child, aged nine, was admitted with membrane on the fauces, and an ulcer upon each tonsil. She sank without any dyspnœa, although after death false membrane was found lining the air passages from the epiglottis to the secondary ramifications of the bronchial tubes.

The last patient was seventeen years old; she had come from a place where diphtheria had been common (Foulness). On admission, the fauces were covered with grey membrane, respiration being natural. But dyspnœa soon came on, and was not much relieved by tracheotomy. After death, false membrane was seen coating the whole of the air passages from the epiglottis to the secondary branches of the bronchial tubes, besides what existed upon the fauces. There was also a slight degree of pneumonia.

The complications of bronchitis were as follows:

Phthisis occurred in	15 cases
Albuminuria	6 „
Disease of the heart	5 „
Emphysema	4 „
Pleurisy	3 „
Pneumonia	2 „
Ague	2 „
Rheumatism	2 „
Aneurism	2 „

Beside which, scabies, swelled cervical glands, anæmia, purpura, and erysipelas, each occurred in a single instance.

The complications of pneumonia may be thus stated:

Albuminuria was present in	6 cases
Pleurisy	6 „
Phthisis	4 „
Disease of the heart	4 „
Chronic disease of the brain	2 „
Delirium tremens	2 „
Bronchitis	2 „

Beside these, continued fever, pyæmia, erysipelas, diphtheria, croup, aneurism, and peritonitis each occurred in 1 case. Dropsy was present in 4 instances, but was regarded as a symptom of the affection of the heart or kidneys, with which it was in each case associated.

Of the 3 cases of pulmonary apoplexy, 2 were associated with disease of the heart, 1 with albuminuria and hydrothorax.

Pulmonary emphysema was associated with:

Bronchitis in	7 cases
Disease of the heart	2 „
Phthisis	1 „
Albuminuria	1 „

Bronchitis was thus present in all but 2. In 1 of these phthisis was clearly diagnosed, although as the patient left the hospital this was not verified by dissection.

The complications of pleurisy were thus distributed:

Pneumonia in	6 cases
Disease of the heart	5 "
Phthisis	4 "
Bronchitis	3 "
Albuminuria	2 "

Beside which continued fever, chronic disease of the brain, and peritonitis were each observed in a single case.

One case of empyema was admitted; the disease was associated with dilatation of the heart and peritonitis. The pus was circumscribed in the lower part of the pleura, and the disease was mistaken during life for enlargement of the liver.

In both cases of pneumothorax the perforation was the result of phthisis.

The only case of well-marked asthma was associated with bronchitis and emphysema.

28. Quinsey occurred once in conjunction with chronic rheumatism. The other cases were uncomplicated.

Cynanche Pharyngea occurred in company with chronic rheumatism, neuralgia, and chronic enlargement of the tonsils, each in 1 case.

Relaxation of the Throat was accompanied in 1 case by hysteria, in another by marked anæmia.

Diphtheria.—In two supposed instances of this disease the prominent symptom was the appearance of membrane upon the trachea and bronchi. These therefore appear also under the heading of croup. In one of them tracheotomy was performed. In another case much membrane was found upon the larynx; pneumonia, peritonitis, and perforation of the intestines were each observed in conjunction with the disease.

Ulceration of the Throat.—Cases of ulceration from scarlatina are not included in this class. The only complication here observed was the poison of syphilis.

29. *Diseases of the Stomach and Œsophagus.*

Dyspepsia occurred twice with hysteria; once with prolapsus uteri, rheumatism, and disease of the heart.

Ulceration of the Stomach, &c.—In one of these cases there existed, as far as could be ascertained, a communication between the œsophagus and the trachea.

30. *Diseases of the Intestinal Canal.*—Enteritis occurred in a patient suffering from disease of the heart. Of the two fatal cases of obstruction, one was an example of simple stricture of the colon, the other of encephaloïd disease of the same part of the canal.

Where looseness of the bowels was merely a symptom of some other disease it is not classed as diarrhœa in this table. In one case it was due to the patient having swallowed hydrochloric acid.

Ulceration of the Intestines, ascertained chiefly by post-mortem examination. In 6 cases tubercular, with phthisis in every one. It also occurred once with fever, and once in conjunction with diphtheria and hypercatharsis; this case has been referred to before. Peritonitis supervened upon this lesion in two instances.

31. *Diseases of the Peritoneum.*

Acute Peritonitis was found complicated with phthisis in 3 cases. In 1 of these there was tubercular deposit also in the peritoneum; in another the intestines were ulcerated; and in the third, disease of the heart existed. Of the remaining fatal cases, 3 were associated with disease of the heart, 1 with disease of the kidneys. Thus in every fatal case the peritonitis was found to depend upon some other lesion.

Chronic Peritonitis.—Of the 6 fatal cases, 5 were associated with tubercular disease; the details were as follows:

Phthisis, with deposit in peritoneum, in	3	(1 of which intestines ulcerated.)
Deposit in peritoneum or mesenteric glands, without phthisis . . .	1	
Phthisis alone.	1	(with cirrhosis.)

In the remaining fatal case there existed empyema, with dilatation of the heart.

The complications in the cases which recovered were:

Ovarian or uterine disease in	3
Chronic rheumatism	1
Cynanche pharyngea	1

1 case occurred after childbirth, gave rise to ascites, and terminated in spontaneous cure by an opening through the umbilicus. Malignant disease was not observed as giving rise to chronic peritonitis in a single case.

32. *Diseases of the Liver.*

Jaundice.—This symptom appeared as the consequence of malignant disease in 3 cases. In 1 it was due to gall-stones, in 1 to supposed congestion. In another it was associated with paraplegia, although the precise connexion was not made out. In the remaining 8 cases the cause of the condition was not clearly ascertained.

Cirrhosis.—Of the 9 cases of this disorder, ascites was present in 6, hæmatemesis in 2. These being merely symptoms of cirrhosis, are not counted as complications.

Phthisis was associated with cirrhosis in . .	3
Disease of the heart	2
Disease of the kidneys	2

Enlargement.—This in 4 cases was owing to malignant disease; in the remaining 11 to some less definite cause. Disease of the kidneys was associated with the condition in 3 (in 1 of which there was also phthisis and disease of the spleen).

In 5 cases ascites was present, while hæmatemesis was not observed once.

Abscess.—In each case this was associated with pyæmia. Once only this was traced to its source—a wound in the finger.

34. *Spleen.*—In one of the fatal cases this organ was found to be hard and brawny; in the other it contained fibrinous deposits.

35. *Albuminuria.*—Dropsy and functional disturbance of the brain

have been regarded as symptoms of the renal disease, and therefore have not been counted in the table as complications. In the whole number of cases dropsical effusion took place in 34. The functions of the brain were disturbed, apparently as a consequence of the renal disorder, in 4 instances. Beside these, the complications were thus distributed:

Disease of the heart	occurred in	15	cases.
Phthisis	"	9	"
Other disease of lungs, mostly bronchitis	"	11	"
Disease of liver	"	4	"
Hemiplegia	"	2	"
Apoplexy	"	1	"
General paralysis	"	1	"

One of the cases of supra-renal disease was recognised as such during life. The discoloration of the skin was like the faintest tinge of jaundice; the conjunctivæ remained quite white. The disease was tubercular, the lungs and the mesenteric glands were affected to an insignificant degree. In the other case there was dropsy, depending upon disease of the heart and kidneys. The supra-renal disease was not recognised during life, although the face and the upper portions of the thighs were of a brownish colour.

37. The distinction between ovarian dropsy and ovarian tumour has not been carried out in this table, in consequence of the coexistence of both fluid and solid matter in these forms of disease.

In the cases given, fluid was greatly predominant in 4.

Of the 5 fatal cases of ovarian disease, 4 were malignant. The other was an example of a simple cyst complicated with phthisis. The patient sank after tapping. In conjunction with these forms of disease, peritonitis occurred in 3 cases. Beside this, crural phlebitis, malignant deposit in the crural and mesenteric glands, disease of the liver and of the kidneys, each occurred in a single instance.

39. *Bones and Joints.*—These structures were diseased in consequence of rheumatism in 6 cases. The affection was of a scrofulous character in 4 cases, in one of which phthisis was also present. In one instance the bones were the seat of malignant deposit.

40. *Skin.*—The only fatal case of cellular inflammation was in conjunction with pyæmia. In both the fatal cases stated as disease of the absorbent glands, those structures were the seat of malignant deposit. In one of them the ovaries were similarly affected; in the other the disease was confined to the glands and cellular tissue.

41. *Muscles.*—In the single instance of disease of the voluntary muscles, the pectoralis major was the seat of severe and constant pain; there was a trace of albumen in the urine.

42. Anomalous, &c.

The anomalous and accidental cases include 2 of concealed pregnancy; 1 of severe and long-continued vomiting during pregnancy; 5 of abdominal swelling of doubtful character; 3 of pain in the same region, also of uncertain nature; 1 of consolidation of the lung, of undetermined character; 1 of palpitation and dyspnœa without discoverable disease; and 1 of discharge from the ear.