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PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *Illustrations of the Nueva Quinologia of Pavon.* With Twenty-seven Coloured Plates of Plants and their Dissections, by W. Fitch, F.L.S.; Three Coloured Plates, containing Forty-four Delineations of Microscopic Sections, by Tuffen West, F.L.S.; and Observations on the Barks described, by John Eliot Howard, F.L.S., &c.—1862. Imperial folio.
2. *Travels in Peru and India while superintending the Collection of Chincona Plants and Seeds in South America, and their Introduction into India.* By Clements R. Markham. Maps and Illustrations.—1862. 8vo.
3. *Report on the Expedition to procure Seeds and Plants of the Cinchona succirubra, or Red Bark Tree.* By Richard Spruce. Map. —1861. pp. 111.
4. *Notes on the Medicinal Cinchona Barks of New Granada,* by H. Karsten; *and of the Cinchona Trees of Huanuco (in Peru),* by E. Poeppig. (Translated from the German.) Pamphlet.—1861. 8vo, pp. 75.
5. *Report on the Expedition to procure Seeds of Cinchona Condaminea from the Sierra de Cajanuma, near Loxa, in Ecuador.* By Robert Cross. Pamphlet, with Appendix.—1861. 8vo, pp. 44.
6. *The Cultivation of the Quina Tree in Java, to the end of* 1859. By Franz Junghuhn and J. E. De Vry. (Translated from the Dutch.) Pamphlet. 8vo, pp. 75.
7. *Gardener's Chronicle,* 1862 and 1863. (Letters of Mr. Cross.)

There is much of almost romantic interest in the history of the Cinchona barks, from the time of the original discovery of their useful properties in the middle of the seventeenth century to the present date. As in so many similar instances, the early history is mythical, and not to be depended upon. One story sets forth that the discovery resulted from the observation, made by the natives of Peru, that the lions, or rather pumas, cured themselves of intermittent fever by gnawing the bark of the quina tree. Humboldt expresses doubts as to whether the natives had anything to do with the matter, seeing that the Indians dwelling in the valleys near Loxa, where intermittent fevers are rife, have an aversion to the quina bark. Poeppig, Spruce, and other recent travellers, give a like testimony. Mr. Spruce further tells us, that the *cascarilleros*, or bark collectors, and the inhabitants of Ecuador in general, have no idea of the use that is made of the bark in foreign countries, their notion being that a chocolate-coloured dye is obtained from it, and explains the repugnance of the natives to the use of the bark medicinally on the following grounds:

"The inhabitants of South America, although few of them have heard of Dr. Cullen, have a theory which refers all diseases to the influence of either heat or cold, and, by what seems to them a simple process of reasoning, their remedies to agents of the opposite complexion; thus, if an ailment have been brought on by 'calor,' it must be cured with 'frescos,' but if by 'frio,' with 'calidos.' Confounding cause with effect, they suppose all fever to proceed from 'calor.' Now, they consider the 'cascarilla' a terribly strong 'calido,' and justly; so, by their theory, which is the reverse of Hahnemann's, its use would only aggravate the symptoms of fever. Even at Guayaquil there is such a general disinclination to the use of quinine, that when physicians there have occasion to prescribe it, they indicate it by the conventional term 'alcaloide véjetal,' which all the apothecaries understand to mean sulphate of quinine, while the patient is kept in happy ignorance that he is taking that deadly substance."

So ignorant are the bark collectors even, that Mr. Spruce says they did not know whether the trees produced fruit or not.

Joseph de Jussieu mentions the fact of a Jesuit having been cured of fever by Peruvian bark as early as 1600. We owe the name Cinchona, however, or more properly Chincona, to the Countess de Chincon, wife of the Viceroy of Peru. This lady is said to have been cured of an intermittent fever, in 1638, by the use of cinchona bark, and to have introduced the drug into Spain in 1640. Hence the name "Pulvis Comitissæ," a name subsequently changed into Cardinal's or Jesuit's powder, in consequence of the drug having been introduced to the notice of Cardinal Mazarin by Cardinal de Lugo, Procurator-General of the Order of the Jesuits.

Science owes to De la Condamine and Joseph de Jussieu the first description of the cinchona tree. These travellers set out from France to South America in 1735, and from their materials Linnæus, in 1742, established the genus *Cinchona*. Joseph de Jussieu, after fifteen years' labour in Peru, was robbed of his collections by his servant, under the not wholly false impression that they contained valuable treasures. This termination to the toil of so many years served to deprive the unhappy botanist of reason, so that he ultimately returned to France, shattered alike in mind and body.

In 1760, the Spanish botanist, Mutis—then in New Granada—was commissioned by the Spanish Government to investigate the cinchona barks; and in 1778, Ruiz and Pavon, two well-known Spanish botanists, set out for South America. They were scarcely more fortunate, as regards their collections, than De Jussieu had been. Once their specimens were destroyed by shipwreck on the coast of Portugal, while a second accumulation was destroyed by fire in 1785. After the return of Ruiz and Pavon to Spain, their pupil Tafalla continued to investigate the Peruvian cinchonas. The results at which he arrived are given in the 'Flora Peruviana,' 1798–1802; in the 'Quinologia' of Ruiz, published at Madrid in 1792; and in a supplement to the latter work, issued in 1801.

The Bolivian barks were first studied by Haenke, a German traveller, who in 1785 traversed a district subsequently visited by Dr. Weddell.

Poeppig, Professor of Botany at Leipsig, travelled in Peru and Chili during the years 1827–1832, and added much to our knowledge of the grey barks of Huanuco.

Dr. Weddell, an eminent French botanist, paid two visits to Bolivia at the instance of the Government of Louis Philippe. An account of his travels is given in his 'Voyage dans le Nord de Bolivie,' Paris, 1853. His splendid monograph on the genus Cinchona, entitled 'Histoire Naturelle des Quinquinas,' Paris, 1849, was until recently the most valuable work of reference on the subject on which it treats. The information it contains respecting the Bolivian barks, and the mode of collecting them, together with the numerous beautiful illustrations of the various species of the genus, will always render this work a storehouse for those desiring a knowledge of this important genus. To this author we owe the first accurate description of the *Cinchona Calisaya*, one, if not the most valuable of the species.

Dr. Karsten's researches were conducted principally in New Granada, and an account of them is given in a splendidly illustrated work, called 'Floræ Columbiæ Specimina Selecta,' Berlin, 1858; and also in a pamphlet published in 1858, and translated into English at the instance of Mr. Markham. It is to be regretted that the translation of this pamphlet, whose title is given at the head of the present notice, was not more carefully performed.

The distinguished quinologist, Mr. Howard, after having, at the suggestion of Dr. Pereira, availed himself of Pavon's specimens of barks in the British Museum, and of other sources of information open to him in this country,* caused inquiries to be made in Spain, and in 1858 was enabled to purchase fifty-four of Pavon's specimens of barks, with an unpublished manuscript, descriptive of several species of cinchona discovered by Pavon and others. Subsequently, having learnt that the original specimens described in the manuscript were still in existence at the Royal Museum of Madrid, Mr. Howard commissioned the well-known botanical artist, Fitch, to go to Madrid and make drawings from these specimens. These drawings, together with some supplied by Mr. Howard, and three showing the microscopical structure of the barks, with other botanical details from the pencil of Mr. Tuffen West, constitute the magnificent volume with which Mr.

* Examination of Pavon's Collection of Peruvian Barks contained in the British Museum. London, 1853, and Appendix 1855. Pharmaceutical Journal, 1854, 1856.

Howard has enriched pharmacological and botanical science. There may be differences of opinion as to the specific value of some of the forms described by Mr. Howard, and as to the importance of the characters he relies upon for their discrimination; but there can be but one feeling of admiration for the superbly illustrated work, which from genuine love of science, and at great cost to himself, he has placed before the public. A reference to Pereira, Royle, and others of our text-books, will give the reader further details as to the works published on the subject of cinchona, and of which Bergen, who wrote as far back as 1826, enumerates no less than 637! The information therein contained has become more or less obsolete, owing to the more accurate information contained in the works of Weddell, Howard, Karsten, and others, laid under contribution in the compilation of the present notice. Thus much, in brief, for the history of the plants.

We can do no more than allude to the disputes which arose soon after the introduction of the drug into Europe as to its efficacy, as it may now safely be assumed that the balance of evidence is decidedly in favour of the utility of these barks, and of the principles derived from them; although it must be confessed that no mean array of evidence might be cited to show that they are valueless. In the last century the debates on the subject ran fast and furious, and were not seldom tinctured by polemical acrimony and Protestant antipathy to the Jesuits. More recently, we find Majendie asserting, as the result of experimental observation, the inertness of the drug. These discrepancies arose partly from the confusion of the several kinds of bark trees by the botanists—a confusion accidental in some cases, but in others brought about by the jealousies of rival botanists. Commercial greed also operated in concealing the places where the trees grew, and the ports whence the bark was exported. In the course of this notice we hope to be able to show how other discrepancies may have arisen, and how likely they are again to be encountered, if the sources of fallacy be not sedulously hunted out and prevented.

The species of *Cinchona* are very unequally distributed in the forests on the eastern side of the Andes, from the extreme north of South America, in latitude 11° N., where they occur on the mountains of Santa Martha, to the forests of Bolivia, as far S. as 19° S. Speaking generally, the trees grow at an elevation of from 4000 to 8000 feet above the level of the sea.

The geological formations on which they are found are mica slate, gneiss, clay slates, and lower silurian rocks, covered, however, in most cases, by several inches of rich vegetable soil. The climate of the districts inhabited by the cinchonas is by no means an agreeable one, as witnessed by Dr. Karsten, who describes the rainy season as lasting for nine months in the year, during which time

"A steady rain is only interrupted during the day by short gleams of sunshine interchanging with clouds and mist, whilst in that part of the year which answers to our winter, cold nights in which the temperature of the air descends to freezing-point, are followed by days in which the rays of the sun, piercing here and there through the thick clouds, raise the temperature to 77° F., whilst the leaves are kept almost constantly bedewed by the continual mists."

The temperature of this vast region, as stated by Mr. Markham, ranges (to speak generally) from 60° to 80° F., thus corresponding pretty closely with the isotherms laid down by Dove for these regions. It is in the highest degree necessary to pay attention to these external conditions, for, to say the least, they are of equal importance with the selection of the proper species for medicinal uses. The medicinal properties of many plants vary extremely in the same species under different conditions, and even when no appreciable difference in those conditions can be traced. Identity in medicinal properties by no means corresponds with identity in structural characteristics in all cases, though it does so in many. Digitalis grown in the Himalayas is said to be nearly if not quite inert. On the other hand, hemp grown in this country is almost entirely devoid of the peculiar resinous principles so abundantly found in the same species growing in the hot sunny plains of India. But we need not go so far in search of instances of this kind. *Œnanthe crocata* and *Cicuta virosa*, tolerably common plants in the south of England, where they have a well-established evil repute as poisonous plants, are both harmless on the other side of the Tweed, according to Dr. Christison. We have also the testimony of Dr. Karsten upon this point, with regard to the variability in medicinal virtues of *Cinchona lancifolia* in New Granada, and that of Mr. Spruce in the case of *C. Condaminea*, the qualities of whose bark vary accordingly as the tree has grown on the sides of the mountains most exposed to the rays of the evening or morning sun. Vegetable physiology is not at present competent to account for such peculiarities in a satisfactory manner, but it is at least necessary to bear them in mind in the experiments now being carried on, as the kind of cinchona-tree that is proved to yield quinine and valuable alkaloïds in Peru may by no means *necessarily* yield the same products when grown in other climes.

Although physiology affords us but little help in these questions, morphology and minute anatomy do give a little information of a valuable kind, but upon which too much stress must not be laid. Students confining themselves to the elaboration of one subject, are too apt to attach a greater degree of importance to appearances than that presented to the mind of a more widely experienced observer; and thus, although we are far from depreciating the observations of the quinologists and the relations they assert to exist between the minute structure of the barks and the proportion of valuable ingredients which they contain, we deem it to be essential that these observations should be contrasted with, and, as a meteorologist would say, corrected by, the general results obtained by vegetable anatomists in other plants. The anatomical structure of the cinchona barks does not differ in any important degree from that of exogenous trees in general. There is, on the exterior, an epidermis only seen on the younger twigs, as it is soon pushed off by the growth of the underlying corky layers (*epiphlœum*), which vary in thickness and colour in different species. Beneath this cellular corky layer is another series of cells, constituting the *mesophlœum*, or cellular envelope, whose constituent cells are of a different pattern from those of the outer corky layers. This middle bark is the region in which the alkaloïds especially abound; in it, also, are many cells filled with a coloured resinous juice, and separating it from the liber or inner bark (*endophlœum*) are a few laticiferous ducts. The inner bark is distinguished by the presence, in greater or less numbers, of long wood cells—"bast cells"—traversing a matrix of cellular tissue, into which also the medullary or pith rays of the wood are prolonged. Within the liber is a layer of growing tissue, called the *cambium*, developing into bark on the outside, and into the true woody layers on the inner side. With these latter we have nothing to do at present. Although these bark layers are originally distinct, yet in process of time they become more or less confused, and the limits between them obliterated.

It is more especially the middle bark or cellular envelope that is valuable in a medicinal point of view, and in proportion to its thickness, as a general rule, is the value of the bark. It has long been known that the barks yielding most quinine have what is termed a short fibrous fracture, owing to the preponderance of cellular tissue over the woody tissue of the liber. It has also been remarked that those barks which are richest in quinine have their liber-cells most blocked up by woody deposits, although the alkaloïds exist chiefly in the other portions of the bark. Moreover, the early disappearance of a set of slender thin-walled vessels, called sap-vessels, is a characteristic of barks that are rich in organic bases. As a rule, subject to exception in the red bark, the best kinds of bark are the produce of trees grown in elevated situations, and in a comparatively cold atmosphere. In such localities the quantity of woody tissue is comparatively slight in proportion to the cellular tissue, in which the alkaloïds are stored up. Our limits do not permit us to enter at greater length into this part of our subject; the illustrations of Mr. Tuffen West in Mr. Howard's volume, and those in Weddell's 'Histoire Nat. des Quinquinas,' will be of more service to the inquirer than pages of description. We must not, however, omit to note a curious circumstance mentioned by Mr. Howard, viz., the formation of crystals in the bark, outside the cells. These crystals Mr. Howard shows, by chemical analysis, to be really crystalline alkaloïds, and not mere raphides, such as abound in the cells and juices of most plants. The crystals in question are supposed to be post-mortem depositions from the sap. The alkaloïds are supposed by Dr. de Vry, a Dutch chemist, associated with Junghuhn in the experimental cultivation of the cinchonas in Java, to be produced by a reaction between the ammonia contained in the bark and the cincho-tannic acid. Mr. Howard, moreover, has shown that the bark of the trunk contains the largest quantity of alkaloïds; hence the flat table pieces or large quills should be preferred to small quills, the produce of young branches, in which the astringent principles especially prevail. The rind of the root is also, to a great extent, destitute of alkaloïds; hence Mr. Howard points out the improvidence of stripping the roots, and thereby not only getting a useless product, but also endangering the life of the tree, and thus preventing the formation of suckers, by means of which the felled tree might ultimately be replaced.

Karsten and other observers have shown that certain more obvious structural peculiarities than any that have just been mentioned may be employed to distinguish the valuable from the inert kinds. "It may certainly be assumed," says Dr. Karsten, "that the *Cinchonæ* with the capsule opening from the base upward and crowned by the calyx, and having moreover a corolla of delicate texture, with bearded edges and generally unindented seed-lobes, give a bark which may be considered pharmaco-dynamically as anti-periodic," while other nearly allied species, but differing in the foregoing particulars, possess only astringent and tonic properties. Dr. Karsten even goes further than this, for he says that the proper average quantity of the organic bases in the bark of the different genuine *Cinchonæ* can be nearly estimated from their form. "The short oval or elliptic capsules crowned by the calyx, the perforated or not perforated seed-lobe border, as well as the little pits (scrobicules) which appear in the axillary vein, near the midrib of the comparatively small leather-like leaves, are signs of a regularly larger quantity of alkaloïd in the plant so characterized, while the large-leaved, unpitted kinds, as well as those with long lance-shaped capsules, or with porous or perforated seed-lobe borders, as in *C. purpurea*, &c., show a small quantity or a total absence of quinine and cinchonine." Unfortunately this theory, however applicable in many cases, and to those that fell under Dr. Karsten's own observation, is falsified in other cases, and notoriously so in the case of the most highly esteemed species, *C. succirubra*. That the relations between anatomical conformation and chemical constitution should be fully worked out, is most desirable, as many of the discrepancies before alluded to may be accounted for by the wrong kind of bark having been employed.

In spite of Mr. Howard's statement, that the difference in the structure of the true and false barks is evident to the naked eye, we think that the ordinary medical practitioner, with his scanty leisure and multifarious duties, will do well to leave the discrimination and selection of barks to the professed pharmacist.

Of the numerous species described by botanists as belonging to this genus, only a few have been proven to possess valuable properties, while others have been shown to be entirely inert. Before giving the list of valuable species, we feel constrained to say a word or two about the nomenclature, already sufficiently involved, and likely from injudicious changes to become more so. First, as to the name of the genus *Chincona*, or *Cinchona*. Linnæus, erroneously no doubt, wrote the latter, which has been up to this time universally adopted by botanists, pharmacists, and medical men. Mr. Markham's suggestion to alter established usage is therefore objectionable: the change, however, is so slight, that we apprehend no practical inconvenience will arise, whichever way the name be spelt. As to the species, the case is different: these have been named and renamed, according to the views of particular authors, till confusion has become worse confounded. Fortunately for our present purpose, one only of the medicinal species to be hereafter mentioned suffers under this horrible malady, caused by conflicting synonyms. Let us explain: in the works of Weddell, Howard, and other recent writers, the species yielding the various kinds of crown bark, has been called *Cinchona Condaminea;* its varieties have been named—1. *Uritusinga*, from the name of the place where it grows; 2. *Chahuarguera* (!) from two native words signifying a resemblance to garments made of the fibre of the American aloe or agave; 3. *crispa;* and 4. *lancifolia*, besides others that need no mention here.

Now Dr. Hooker points out that the original name applied by Linnæus and adopted by many other writers was *C. officinalis*, a name changed by Humboldt and Bonpland, without sufficient reason, to *C. Condaminea*. Dr. Hooker very justly remarks, that "When once the law of priority is departed from without perfectly good cause, the door is opened to endless future change and consequent confusion." Hence then, in future, we are to employ Linnæus' original and unobjectionable name *C. officinalis*. The variety—1. *Uritusinga*, having been the original one detected by La Condamine, is to be called *C. officinalis*, var. *Condaminea;* the variety 2. *Chahuarguera*, having been figured by Bonpland, is to be called *C. officinalis*, var. *Bonplandiana*, a change for the better so far as facility of pronunciation is concerned; vars. 3 and 4 remain as they were. We should not have trespassed so long on our readers' attention, had we not thought it more than likely that this recent change of names, however desirable it may be, may sorely puzzle those not well versed in botanical lore. Adopting then the correct nomenclature, we have the following medicinal species:—

Cinchona Calisaya furnishes yellow bark.
" *nitida* }
" *micrantha* } furnish grey barks.
" *Peruviana* }
Cinchona officinalis.
" var. *lancifolia* furnishes Carthagena bark.
" " *Condaminea* }
" " *crispa* } furnish crown barks.
" " *Bonplandiana* }
Cinchona succirubra furnishes red bark.

Geographically speaking, the kinds may be thus grouped:—

1. The Calisaya, or yellow bark region of Bolivia and Southern Peru.
2. The grey bark region of Huanuco and northern Peru.
3. The Carthagena bark region of New Granada.
4. The red bark region of Ecuador.
5. The crown bark region of Loxa.

Cinchona Calisaya, and a small shrubby variety of it, called *C. Calisaya*, var. *Josephiana*, so named in honour of Joseph de Jussieu, yield the much-valued yellow or Calisaya bark. These trees inhabit the forests of Bolivia and Caravaya in South Peru, and were originally discovered in 1785 by the German botanist Haenke, accompanied by a Spanish naval officer. "Formerly," says Dr. Weddell, "they were everywhere found around the inhabited parts of the region, while at present, to find a tolerably-sized tree, it is necessary to make several days' journey into the heart of the forests." Further north, although the soil and climate continue much the same, the valuable species disappear until in 10° S. latitude, in the forests of Huanuco (the district traversed by Ruiz and Pavon), the species yielding the Huanuco or Lima barks of commerce are met with.* The epithet grey is applied to these barks from the appearance presented by the lichens growing on them; when wetted, they have an opalescent appearance. They are esteemed in commerce as yielding cinchonine.

Again, going northwards, is an interspace inhabited by many inert *Cinchonæ*, and beyond this district are the forests of Loxa, in Ecuador, yielding the various kinds of crown bark, the produce of *C. officinalis* and its varieties. These barks are rich in cinchonidin, and hence the cure of the Countess de Chincon must be attributed to that substance. For upwards of a century these were the only barks known. Carthagena bark is yielded by another variety, *C. officinalis*, var. *lancifolia*, and also furnishes a large amount of quinidin. The tree is a native of the forests of New Granada, where it was originally discovered by Mutis.

On the eastern side of Chimborazo in Ecuador, are situated the forests supplying the red bark, cascarilla roja, the produce of *Cinchona succirubra*, and now considered as valuable, or more so, than the yellow bark.† To Pavôn, Dr. Klotsch, and especially to Mr. Spruce, a botanical traveller, we are especially indebted for our knowledge of this important species. It is described as being the most handsome tree of the forests in which it is found, and seems to require a higher temperature than that which is congenial to the other kinds.

The method of collecting the red bark, as described by Mr. Cross, seems to be nearly the same as that noticed by Dr. Weddell, in the case of the Bolivian barks. The red bark is exported from Guayaquil, and contains from three to four per cent. of alkaloïds. Its introduction into England resulted from the capture of a Spanish ship with a cargo of this bark by an English frigate in 1779, and Dr. Saunders, of Guy's Hospital, was the first to ascertain its value. Unmerited discredit was afterwards thrown upon it, from the use of fictitious kinds in place of the true red bark. North of Chimborazo, in the elevated plains of Riobamba and Quito, the *Cinchonæ* disappear, but in the mountain ranges further to the north, from about lat. 2° N. to the Santa Martha mountains in 11° N. they are again met with.

There are thus many spots in the regions alluded to, as well as in Mexico, &c., where the conditions seem favourable for the growth of cinchonas, but where, nevertheless, they are not to be found, owing, as Mr. Markham suggests, to breaks in the chain of mountains, and the occurrence of low lands in the intervals.

The utter disregard which the Spanish and South American Governments have shown in past years to the maintenance of the cinchona forests, and the reckless destruction of these valuable trees, which they took no steps to prevent, have caused a well-grounded apprehension lest the supply of quinine may shortly fail. Representations to this

* One of the species, *C. micrantha*, is also found in Bolivia.

† The maximum quantity of alkaloïd in the red bark is about 5 per cent. Mr. Howard mentions one exceptional case in which he obtained 8½ per cent. From the other species, 3 per cent. of alkaloïds is considered a good yield. The quantity varies very much even in the same variety.

effect were made by M. de Jussieu and others more than a century since, but still the same recklessness prevailed, accompanied by carelessness in the due selection of the proper kind of bark, and by obstructive and vexatious fiscal regulations. The recent testimony of Dr. Weddell as to the course pursued in Bolivia, and that in Peru by Mr. Markham, as well as in Ecuador by Mr. Spruce, is all to the same effect. It is a fact by no means devoid of significance in this particular, that in the recent International Exhibition, not one of the South American Governments contributed specimens of the barks grown within their territories—a void in part supplied by Mr. Howard, who exhibited living plants of some of the most valuable kinds, as well as a fine collection of commercial specimens. Our Dutch neighbours also sent illustrations of the success of the experimental culture of cinchona barks, now being carried on by them in Java. No wonder, then, that attempts have been made at various times to transplant the cinchona plants to other regions. The truth of Mr. Spruce's proposition, that "whatever vegetable substance is needful to man, he must ultimately cultivate the plant producing it," will not be disputed. The earliest attempt to export living cinchona plants was that of La Condamine, who in 1745 tried to convey some young plants down the Amazon to Cayenne, from whence he intended to transmit them to Paris—a plan that was frustrated by a wave, which dashing over his vessel, swept off the box containing the plants.

To the late distinguished Professor of Materia Medica in King's College, Dr. Royle, much credit is due for his persevering attempts to induce the Indian Government to take up the subject, and at various times seeds and plants were, under his auspices, conveyed to India, where, however, they speedily perished. The Dutch Government in 1852 were scarcely more fortunate in their efforts to introduce the plants into Java. They despatched a botanist to Peru to procure seeds and plants, which were safely landed in Java, where, however, the cultivation failed in regard to the better sorts, while, on the other hand, they had the mortification of propagating in abundance a worthless species, *C. Pahudiana*. Many plants have since been raised from seeds presented by Drs. Weddell and Karsten, and with a liberality that contrasts most favourably with the former practices of the Dutch Government in similar matters, presents and exchanges of the useful sorts have been made between that Government and our own.

In 1859, Mr. Markham was commissioned to undertake the collection and transmission of cinchona plants and seeds from South America to India. The narrative of his journey in both countries is very agreeably told in his recently-published work, which also contains a digest of the history of the cinchonas, and an account of the experiment now being made in India to cultivate them. Mr. Markham, in conjunction with Mr. Weir, explored the Caravaya region where the *C. Calisaya* is found, while to Mr. Spruce, who was assisted by Dr. Taylor and Mr. Cross, the duty of collecting plants and seeds of *C. officinalis* and *C. succirubra* was entrusted. Mr. Pritchett undertook similar duties in Huanuco. Mr. Markham reached Islay, in Peru, in March, 1861, and succeeded in reaching Sandia, on the eastern slopes of the Andes.

He had collected some two hundred plants, when a Government order arrived to prevent the removal of plants or seeds. This difficulty was evaded by sending Mr. Weir to Crucero, where, as anticipated, he was confronted, to no purpose, by the authorities, while Mr. Markham himself took flight with the plants over the frozen Cordilleras, and succeeded in reaching Islay on June 1st. Here again the Customs officers interfered, and rendered necessary a journey to Lima, in order to obtain permission to ship the plants. Meanwhile an attempt was made to bribe the men in charge of the cases, and to induce them to destroy the plants by pouring on them boiling water through holes bored in the cases. The plants were ultimately safely landed in England; a few were left at Kew, and the remainder despatched to India. During their passage down the Red Sea, the plants almost all died from the excessive heat; the few survivors experienced a like fate during a delay that occurred at Bombay. This result was the more annoying, as the circuitous route *viâ* Southampton might readily have been avoided by sending the plants direct to India, for at the very time of their embarkation from Peru, H.M.S. *Vixen* was lying idle off Callao. Mr. Pritchett's collections of young plants also died *en route*, but numerous seeds gathered by him were sown at Ootacamund, and are, we understand, growing well.

The proceedings of Mr. Spruce, Dr. Taylor, and Mr. Cross, the gardener associated with them in the search for and collection of seeds and young plants of the red bark, are well told in the report of Mr. Spruce, as well as in some communications of that gentleman in the 'Journal of the Linnean Society,' and in some letters of Mr. Cross in the 'Gardener's Chronicle,' 1862. On receiving his commission, Mr. Spruce at once set to work in the face of many difficulties caused by the necessity of consulting the owners of the property on which the trees grew, by the unsettled state of the country owing to a civil war, whereby communication between the interior and Guayaquil was shut off. Pending the arrival of Mr. Cross, Mr. Spruce proceeded to the forests, and made copious notes on the vegetation and climate of the district. He had the mortification, moreover, while watching the ripening of the fruit of the cinchona trees, to see the capsules attacked both by fungi and maggots; and as if these depredators were not sufficient to depress the spirits of the naturalist, the inhabitants began to pluck the remaining seed-vessels before they were ripe, in which proceeding, however, they were checked by the address of Mr. Spruce. After a time, and in spite of vexatious restrictions exercised by the soldiers, Mr. Cross was enabled to join Mr. Spruce in July, 1860, and at once proceeded to take cuttings and layers from the old stools. These operations were carried on in the face of two appalling obstacles,—to wit, a scarcity of water and an invasion of caterpillars; and in August the seeds ripened, and were collected. Experience had taught the collectors to gather the capsules at daybreak, when they are closed, and so prevent the loss accruing from the scattering of the seeds from the seed-vessels, which open during the day and close at night.

Communication with Guayaquil was resumed in September, when Mr. Spruce at once proceeded to that port, and despatched the seeds to Kew and Jamaica. Mr. Cross remained behind in charge of the cuttings and seedlings, and at length, in the early part of December, 1861, the young plants were taken up most carefully, packed in cylindrical baskets open at both ends, the leaves and stems enveloped in dry moss, while the roots were protected by a layer of the same material kept damp. Each basket so packed contained about fifty plants, and was conveyed by a mule. During the descent of the steep forest slopes, one of the mules got entangled in the branches of a tree thickly hung with climbing plants, which becoming fixed to the baskets, caused the destruction of the latter during the struggles of the animal to extricate itself. Another mule shortly afterwards fell, and rolled over, crushing the baskets and their contents. It would be tedious to detail the many precautions taken, and the hair's-breadth 'scapes of the poor plants, which were ultimately placed in fifteen Ward's cases made thoroughly water-tight: a necessary proceeding, when it is remembered that during the voyage the cases might be standing for several hours—perhaps days—in five or six inches of salt water. The cases were eventually placed on a raft, and floated down the river from Ventanas to Guayaquil, where they arrived on the 27th December.

At this place the cases and their contents attracted the attention of the merchants; and the authorities have since caused the enactment of a decree, forbidding the exportation of seeds or plants under very heavy penalties. By the time this decree was enacted, however, the cases were in the Straits of Babel Mandeb. Passing from one steamer to another, thence *viâ* the Panama railroad to Aspinwall, where the plants were again shipped, they were transported to the Island of St. Thomas. During this part of their journey the deck on which the cases stood was continually flooded by salt water. At St. Thomas the plants were re-shipped and conveyed to Southampton, where they arrived on February 13th, 1862, having encountered gales of cold wind in the channel, where the thermometer often fell as low as 30° F. The plants were at once forwarded to Kew, where some of them were retained and their places supplied by cuttings of other species grown in that establishment. On the 27th of February, the plants were again shipped at Southampton for Alexandria, having had a partial drenching of salt water *en route*. From Alexandria to Cairo and Suez, and thence down the Red Sea, where, in spite of the predictions of the fearful, the plants received no injury, but were safely landed at Bombay. During the journey from the last-mentioned place to Calicut, the plants were exposed to great risks, owing apparently to the perversity of the captain of the steamer, in spite of which, however, they reached Calicut in safety. They were now conveyed by canoe up the river, and when further progress was arrested by the shallowness of the water, they were transferred to the backs of the coolies, and reached their final destination at Ootacamund on the 8th of April, only nineteen of their number having been lost during the voyage from Peru to India.

Mr. Cross, after thus successfully carrying out his anxious and arduous undertaking, paid a second visit to South America, reaching Guayaquil in Sept., 1861. He proceeded to Loxa, and in spite of the decree before mentioned, and now in full force, he managed to collect and despatch to India one hundred thousand seeds of *C. officinalis*.

Several sites have been proposed in various parts of India, as fitting for the culture of the cinchonas. Of these, the neighbourhood of Ootacamund, in the Neilgherry Hills, seems the most to resemble in elevation and climate the Peruvian habitat of the trees. The Ootacamund Gardens are, moreover, under the charge of Mr. M'Ivor, whose practical knowledge of his profession, and of the requirements of the cinchonas in particular, render him peculiarly fitted to superintend the experiment of the introduction and cultivation of these trees in India. Ootacamund may therefore be considered as at present the head-quarters of the cinchonas in India; and to show what success has, up to this time, attended the experiment, we may mention that up to January, 1863, thirty-five thousand plants of the best kind were permanently placed out, while no less than one hundred and seventeen thousand seven hundred were in preparation, under Mr. M'Ivor's care. This gentleman's method of culture is described in an interesting report, dated July, 1862, and printed in the second part of Dr. Seemann's recently established 'Journal of Botany.' The Dutch experimenters in Java appear to have copied nature too slavishly, and to have grown the plants under the dense shade of the forest, where, in the consequent "struggle for existence," the weakest succumbs. Mr. M'Ivor follows the "selective" principle, and gives his plants all the assistance and protection that theory and practice can afford. At present there are two principal plantations—one for the Loxa barks, which require a lofty situation, and another, at a lower elevation, for the red bark and calisaya-trees. Seven hundred and forty-five acres of land are in preparation for this experiment, and it is the intention of Government to plant 150 acres annually for at least ten years.

In Ceylon, too, Mr. Thwaites, a distinguished botanist, reports that he has raised nearly a thousand plants of *C. officinalis* from seed, and that numerous plants of the equally valuable species *C. succirubra*, are doing well.

In the neighbourhood of Darjeeling, in the Sikkim Himalaya, the cultivation is being carried on with energy, the plants having been derived from the nursery at Ootacamund, the Botanic Garden at Calcutta, and from Java. It is worth notice, that the plants from the latter locality, though exposed to more casualties on their journey, have proved to be far healthier than the others, and the loss among them has been, according to Dr. Anderson, the Superintendent of the Calcutta Garden, much less. Plantations have also been established in the north-western provinces, in Travancore in the Punjaub, in Assam, and other parts of India.

Our West Indian Colonies, although they have been supplied with seeds and plants, do not appear to have taken the matter up with the spirit and energy of the Indian Government. In Jamaica and Trinidad, however, the culture has been begun.

At Kew a depôt is maintained of seedlings and young plants, so that the prospects of the experiments, carried out in the face of so many difficulties, may be described as most favourable, the more so as the chemists assure us of the presence of the alkaloïds in the plants grown in Java and India. Dr. de Vry, in the report drawn up by himself and Mr. Junghuhn, enters into details on this subject; and Dr. Anderson gives strong presumptive evidence of the presence of the alkaloïds in the leaves even of *C. succirubra*, grown in India. A strong infusion of the leaves was administered to some patients suffering from intermittent fever, and with apparently excellent effect. Should this observation be confirmed, the leaves would serve as a substitute for the bark, till the trees arrive of a size to allow them to yield a supply of bark.

Mr. Howard forcibly urges upon the profession the desirability of employing the salts of cinchonine as a substitute for quinine, as the same amount of febrifugal power may be obtained from the former as from the latter, and at one-fourth of the cost. It is assuredly of great importance that the accuracy of this statement be tested in practice.

Time, patience, and Government aid are needed for the success of the experiment now being carried on in India; Government aid, because few, if any, private individuals could or would undertake the necessary sacrifice of time and money.* Ten years must at least intervene before the trees can yield any profitable return on the outlay expended on them, unless, indeed, the leaves can be utilized. Is it not possible that by the time our Indian possessions are enabled to supply themselves and us with quinine, chemists may have discovered the means of producing this and the allied alkaloïds artificially, or may have supplied us with an efficient substitute? Meanwhile, we think that all concerned in the laborious undertaking whose progress we have attempted to record, have fair grounds for exultation in the practical results of their labours; nor must we overlook the immense services rendered by a member of our own profession, in the safe transmission of so many young plants from South America to England, and thence again to the hot regions of the Red Sea and India. Without the Wardian case such success would have been unattainable.

Review II.

On Diseases of the Chest, including Diseases of the Heart and Great Vessels; their Pathology, Physical Diagnosis, Symptoms, and Treatment. By Henry William Fuller, M.D. Cantab., &c. &c.—*London*, 1862. 8vo, pp. 703.

In this treatise, the extent of which is indicated by its comprehensive title-page, we have not simply a compilation, but one with no less

* Since the above was written, Mr. Markham, in a paper read at the Society of Arts, March 27th, 1863, mentions that 35,000 plants have already been ordered by companies and private planters. The reader is referred to that paper for further details on this subject.

pretension than that of a complete work on all thoracic disease, embodying the results of the author's experience and observation; at the same time that he has availed himself of the labours of his predecessors, and admits that he has profited largely by their investigations. Without here entering into the disputed points brought under our notice, we may say that our readers will find the grounds fully stated for any difference of opinion which Dr. Fuller has seen reason to express. It would carry us beyond the limits of available space were we to attempt a critical examination of any of these subjects of controversy; we shall, therefore, content ourselves with stating a few of the prominent features of the treatise, which may appear to us to present originality either in themselves, or in their mode of handling by the author.

In the first place, we would draw attention to our author's mode of putting his conclusions before the reader in a tabulated form, which certainly has the great advantage of clearness and conciseness; and, considering that the work is addressed to the student of medicine as much as to the practitioner, this plan has much to recommend it.

One aim of the author having been to simplify the nomenclature of sounds heard in the chest, we may notice certain remarks to be found with reference to the terms *râles* and *rhonchus,* the latter being known as "dry" sounds, as compared with the former, which are appropriately termed "moist" sounds. Although the former are undoubtedly connected with a narrowing of the channels through which the air passes, yet it does not, as Dr. Fuller observes, accurately state the fact to refer the sound to the total absence of secretion, since this very narrowing is often caused by the presence in an air-tube of viscid, tenacious mucus.

"If, for the sake of convenience," Dr. Fuller remarks, "the terms 'râle' and 'rhonchus' are to be retained, the former should be restricted to sounds of bubbling, the latter to those of vibration. A distinction would thus be drawn between sounds which, though not necessarily indicative of a very different condition of the pulmonary tissue, yet take their origin in a different mechanism."

In the author's opinion, it would simplify our phraseology if the word "sounds" were substituted for "râles" and "rhonchus."

In the following table, Dr. Fuller has included all varieties of sounds having practical significance, arranged under the two preceding divisions:

Sounds of vibration	Low-pitched or grave-toned	Snoring	Sonorous rhonchi.
		Cooing	
		Buzzing	
		Grunting	
		Creaking	
	High-pitched or shrill	Whistling . . .	Sibilant rhonchi.
		Piping	
		Hissing	

Sounds of bubbling	Crepitating	Minute, dry, rapidly evolved, crackling. . . .	Crepitant râle, dry crepitation, dry crackling.
	Bubbling	Fine or small, and tolerably even sized—bubbling.	Sub-crepitant, or muco-crepitant râle, humid crepitation, or humid crackling.
		Coarse and irregular sized—bubbling	Mucous or sub-mucous râle.
		Large and hollow sounding—gurgling	Cavernous râle.
	Clicking	Dry.	
		Moist.	

When expounding the laws which govern the emission of sounds by the chest under percussion, Dr. Fuller proceeds to explain the cause of certain exceptions to those laws, concerning which he urges that no satisfactory explanation has hitherto been advanced. These exceptions are found in those instances of the emission of sound when the lung is supposed to present the conditions contributing to dulness on percussion, such as the occurrence of a clear high-pitched sound, of a somewhat metallic character, over lung-tissue more or less solidified; and the existence of abnormal resonance in the infra-clavicular regions, the pleural cavity being three parts full of fluid, and the lung thereby partially compressed. The precise character of the sound is apt to vary from day to day, and does not always admit of accurate definition; but it is described by the author as "an abnormally clear but shallow resonance." After having reviewed the various hypotheses that have been put forth to account for the fact referred to, Dr. Fuller adds:

"From a careful consideration of the various circumstances under which this singular phenomenon is met with, I have been led to believe that it arises from the presence of air pent up in lung-tissue, in the immediate vicinity of consolidated tissue—a condition which prevents the diffusion of the vibrations excited by percussion, and leads to the concentration and intensification of the resonance. Nothing can be more certain than that in many cases of pneumonia, especially when accompanied with some amount of capillary bronchitis, the gorged and distended condition of the capillary vessels, and the effusion existing in the terminal bronchi, block up the air-passage from the air-cells, and thus retain in them air in a state of greater or less elastic tension. The post-mortem examinations of persons who have died of pneumonia show this to be a condition of very frequent occurrence. The lung cannot be compressed by any moderate degree of force; little or no air can be squeezed out of it; but no sooner is its tissue cut by the scalpel, than there issues forth a sanious frothy fluid, or, in other words, a fluid largely mixed with air. The portions of the lung in which this exists are those immediately above the point to which hepatization has extended, and below that to which the air has free access. They are those, in short, where crepitation or fine bubbling sounds occur, and are precisely those over which this clear-toned resonance is met with. On several occasions I have traced this resonance shifting its position higher and higher in the chest, as the inflammation has spread upwards, whilst at the same time dulness on percussion has also extended upwards, and has occupied the parts immediately below it, which previously had been the seat of this clear-toned resonance. In instances of tubercular deposit, the conditions essential to the retention of air in the lung-tissue are of less frequent occurrence, and abnormal resonance over condensed tubercular lung is met with less frequently; but it does occur in certain instances; and it is easy to conceive how some of the smaller bronchi may become occluded either by tenacious secretion, or by the pressure of tuberculous matter; and that the obstruction thus created may, for a time at least, prevent the escape of air from that portion of the lung to which these bronchi lead. And so again in pleurisy, with an abundant secretion into the pleura. Anything, in short, which serves to occlude a bronchus, while as yet the lung beyond the point of obstruction remains even partially distended with air, will tend to produce this abnormal resonance." (p. 59.)

Dr. Fuller supports his pathological inferences by experiments upon sheep's lungs injected and inflated to varying degrees, and eliciting sounds in accordance with the preceding observations. We consider the views of the author, upon these exceptional sounds, to be deserving of attention, worked out with much care, most probably conveying the correct explanation of the phenomenon, and affording an instance of exact induction.

Among the adventitious sounds produced within the chest by the act of breathing, and included in the table already quoted, among the sounds of "bubbling"—the *râles*—we find two varieties of the "clicking" sound —viz., dry and moist. Dr. Fuller has convinced himself that it is due to the sudden and forcible passage of air through a small bronchus, the sides of which, at one or more points, have been brought close together by external pressure, or have been agglutinated, as it were, by tenacious mucous secretion. Thus its common cause is the presence of tubercle pressing here and there upon the walls of smaller bronchi, and not only rendering them impervious, but exciting slight local irritation, with the consequent secretion of viscid tenacious mucus. Dry clicking, Dr. Fuller points out, originates in connexion with a very small quantity of viscid secretion, and is met with almost exclusively during inspiration; whilst moist clicking is connected with a somewhat larger quantity of fluid, and though most distinct and constant during inspiration, occurs not infrequently during expiration. In our opinion the explanation thus offered appears sufficiently to clear away the obscurity attending the production of a sound which has been regarded as indicative of the early stage of tubercular softening, but which, taken alone, does not warrant so grave a conclusion. Its persistence and subsequent conversion into other varieties of bubbling, leaves, according to the author, no doubt as to the existence of tubercle.

In the chapter on the resonance of the voice in health and disease, we find many points on which we could dwell with satisfaction, but we restrict ourselves to the author's remarks on ægophony.

With Skoda, Davies, and other experimentalists, Dr. Fuller has found Laennec's explanation of this sound to be insufficient:

"Although they have confirmed to the fullest extent the frequent coexistence of pleuritic effusion and ægophonic resonance—nay, more, though they have proved that in a pure intensely developed and persistent form ægophonic resonance is never met with without the presence of fluid in the pleura, yet they have also shown that effusion may exist without this peculiar vocal resonance; and conversely, that ægophonic resonance may and does occur in pneumonic or tuberculous consolidation without any fluid in the pleura." (p. 106.)

The conditions laid down by Dr. Fuller as essential to the production of ægophony, are—1st, a condition of lungs calculated to give rise to bronchophonic resonance; 2ndly, the existence of some agency able to impart to that resonance a tremulous, bleating character. The author believes this to be found in the vibration of partly solidified lung against the costal pleura. This condition Dr. Fuller compares with the schoolboy's trick of speaking upon thin paper placed over the teeth of a comb, or speaking as Punch and Judy showmen do with a disc of metal or ivory placed between the lips and teeth.

In discussing the practical question of paracentesis of the chest, Dr. Fuller dwells upon the importance of employing the grooved needle for exploration before introducing a trochar. The supposed dangers of the admission of air into the pleural cavity, in cases in which this operation is admissible, are disposed of by the author as groundless, where pus has been proved to be present by the use of the grooved needle. Indeed, the little reason for alarm on the score of the entry of air into the cavity is seen from the plan known as the system of "drainage," in which two openings are made in such wise that the pus flows drop by drop, and its collection in the pleura prevented.* All the points raised in the consideration of this operation have been fully and ably treated by Dr. Fuller.

Dr. Fuller prefers the claim of originality in the treatment of hooping-cough. Having had regard in the first place to the catarrhal stage of the disease, Dr. Fuller directs his aim to arresting its spasmodic stage. In this, so far, there is neither novelty nor originality; but the following treatment has not, we believe, been previously advised:

"As soon as the whoop declares itself, a draught is given every three or four hours, containing half a grain or a grain of sulphate of zinc, and a sixth of a grain of extract of belladonna to two drachms of syrup, in from two to six of water, and an additional grain of sulphate of zinc, and an additional sixth of a grain of belladonna are added to each dose daily, or every alternate day, until the quantity taken daily amounts to from six grains to a drachm of zinc, and from two to six grains of the extract of belladonna, according to the age of the patient. To children under a twelvemonth old I have never administered more than ten grains of zinc and two grains of belladonna daily, which were given in doses of a grain and a quarter of the zinc, and a quarter of a grain of belladonna every three hours; whilst for children of eight or ten years of age I frequently prescribe half a drachm or two scruples of the zinc, and six grains of belladonna. If the dose be gradually and cautiously increased, the medicine will not occasion sickness; and as it neither heats nor excites the patient, it may be given as soon as the true nature of the complaint is ascertained. Its administration, however, need not preclude the exhibition of other remedies; and if there is feverish heat of the skin and persistent quickness of breathing, indicating inflammation of the lung, or if the bronchial flux is great and oppresses the breathing, it is always prudent to have recourse to auxiliary measures." (p. 336.)

That belladonna is a drug which the system will tolerate in much larger doses than has usually been supposed possible, has been made

* Regarding this "drainage" we would refer the reader to p. 261 of our number for January last.

known to the profession through the medium of a paper by our author in the forty-third volume of the Royal Medico-Chirurgical 'Transactions.' The precautions to be observed in its administration being that it shall be given daily in divided doses, and that these shall be gradually increased, a sudden large augmentation being speedily followed by its characteristic signs of poisoning.

Some of our readers, who may have been led to adopt extreme views on the debated questions of depletion or stimulations might possibly be disposed at once to close Dr. Fuller's book, on finding that he goes the length of recommending venesection under some circumstances in the treatment of pneumonia and of hæmoptysis, a practice which we understand has, under these circumstances, been long enforced at the hospital to which our author is attached. The study of the limitations with which the author has fenced around his recommendation affords proof that Dr. Fuller exercises a judicious discretion in the selection of his remedies. Under the head of hæmoptysis he writes:—

"Small and repeated blood-lettings, from six to ten ounces, according to the constitution of the individual, appear to answer better than a single full bleeding, and venesection from the arm has proved more serviceable in my hands than the loss of blood by cupping; the blood is drawn more quickly, and the effect on the pulse is more decided. But whether venesection from the arm or cupping be employed, dry cupping ought never to be neglected. In most instances it will supersede the necessity for blood-letting, and is at all times a valuable adjunct." (p. 267.)

The following remarks may be borne in mind at the present time, when we are instructed by a certain class of practitioners that brandy alone is the sovereign remedy for pneumonia:—

"When it is remembered that physicians living at the same time, and observing the disease under the same atmospheric and other conditions, have arrived at precisely opposite conclusions from the same numerical method of calculation, it needs not an elaborate argument to prove how little reliance is to be placed on the results. Common-sense points out that what is serviceable in one case will prove mischievous in another, under different conditions of age, sex, constitution, and the like; and experience not only endorses this view, but proves that under certain circumstances blood-letting is a palliative of extreme value." (p. 239.)

In his own practice the author states, however, that he has only had recourse to blood-letting three times within the last four years.

The chapter on pulmonary consumption occupies one hundred pages. We cannot omit to notice more particularly Dr. Fuller's views on one point which has undergone a great change within the last few years. It has been in accordance with popular prejudice and the opinions of the medical profession in past days, that the first and most essential of all hygienic measures for consumptive patients, has been warmth. So far has this been carried that where banishment to a warm climate has been found impracticable, the sufferer has often been immured in hot rooms from which every breath of pure cold air has been jealously excluded by double windows, and other contrivances of a like nature. It has, however, gradually come to be forced upon the notice of medical men that members of consumptive families, as well as other people, enjoy better health and greater vigour in the cold, bracing weather of winter than in the relaxing, enervating warmth of summer months. Dr. Fuller observes,—

"Of all countries in the world, there are none so exempt from the ravages of consumption as those which are included in the isothermal lines of 30° and 40° mean annual temperature. In St. Petersburgh and Moscow, with a mean annual temperature of about 38° Fahr.; in Canada and the northern districts of North America; in Iceland and in the Faroe Islands, and in the northern parts of Norway, Sweden, and Lapland, the disease is comparatively rare, whilst in France, Italy, and along the shores of the Mediterranean, in Malta and Madeira, and other localities to which consumptive invalids are usually consigned, the ratio of mortality among the natives from phthisis equals, and in many instances exceeds, that which obtains in many parts of our own country." (p. 417.)

These facts have been confirmed and impressed upon the minds of practitioners, by restoration of health and vigour by removal to a colder climate. "Some of the most remarkable recoveries from consumption," Dr. Fuller states, "which have come within my own cognisance have been in the persons of those whose occupations or necessities have driven them to the cold regions of the north." Dr. Fuller guards himself against the inference that warmth is prejudicial to, or not required by consumptive patients indiscriminately. On the contrary, the author very distinctly states, "Whereas many persons are benefited by warmth, others are equally benefited by cold, and that the opposite opinion, which is commonly enforced in practice, is constantly leading to lamentable results." (p. 418.)

The protection of the surface of the body is strictly enjoined by the author, while the tonic influence of a cool climate is enforced. We must, however, refer our readers to the pages of Dr. Fuller's work for exact information as to his views on this very important topic. We must of course add that the same views have been held by others, although they have too often been lost sight of in the prevalent preference for residence in warm climates. Had it not been for the peculiar form in which a distinguished surgeon propounded his opinions some years ago, he would in all probability have gained a wider hearing when he so energetically declared the efficacy of cold in phthisis. The late Dr. Hull, of Norwich, in a small treatise on phthisis, endeavoured to enforce a similar doctrine, fortifying himself with the opinions of Sydenham, whose "palmarium" remedy was out-door exercise on horseback.

The following summary of the author's conclusions respecting the sounds of the heart in health are given at p. 482:

"1st. That both sides of the heart contribute to the production of the two sounds.

"2ndly. That the synchroneity of the causes which gave rise to the first and second sounds respectively on the two sides of the heart, lead to a blending of the sounds generated on the two sides, so that two sounds only are heard accompanying the heart's action instead of four.

"3rdly. That the first sound as heard on the chest walls is produced wholly by the sudden tension of the auriculo-ventricular, but is modified under certain conditions by the violent contraction of the muscular structure of the heart.

"4thly. That the second sound is referable to the sudden tension of the semilunar valves.

"5thly. That the differences of character observable between the first and second sound are due to differences in the structure, positions, and attachments of the auriculo-ventricular and semilunar valves respectively, the former being deep-seated and connected with tendons and muscles which, equally with themselves, are thrown into vibration by the tension to which they are subjected, whereas the latter are free from muscular and tendinous attachment, and have not the thick muscular walls of the heart interposed between them and the ear of the observer."

The cause here assigned for the first sound is the same as that originally given by Dr. Billing. It will be observed that no part in the production of the first sound has been here allowed to the passage of the blood over the surface of the ventricle, large arteries, and valves. These structures, Dr. Fuller urges, are so exquisitely smooth, that it is improbable that in health there would be any appreciable sound from the collision of the globules of the blood. This position, Dr. Fuller argues, is strengthened by the admission that no sound is excited by the rush of blood and the collision of its particles during the systole of the auricles, when the blood comes in contact with the irregular surface of the walls of the ventricles.

The chapter on pericarditis contains an admirable *résumé* of all that has been written thereon worth reading, with the incorporation of the author's large experience in this disease. Having made the treatment of acute rheumatism his particular study, Dr. Fuller is well able to write the natural history and treatment of this formidable malady, the origin and root of so many subsequent forms of cardiac derangement. Under both heads we can confidently refer our readers to this section of the book before us.

We have had occasion to allude to the investigations of Dr. Fuller on the action of belladonna; we would now direct the attention of our readers to the author's opinions on the action of digitalis in diseases of the heart. The opinion has been universally entertained that this drug exercises a depressing influence upon the heart, to the extent of even paralysing its fibres, and causing sudden death by this action; therefore, that it is a medicinal agent utterly inadmissible in cases of dilatation of the heart's cavities. It is indeed surprising how tenaciously this view has been adhered to, in spite of opposing facts; thus, the late Dr. Pereira, while recording the salutary effects of large doses, has with evident mistrust and hesitation reported the fact that he had himself ventured to give it in one or two instances, to the extent of two or three drachms, at the same time expressing his surprise that he had not done great harm thereby, and confessed that he had not ventured on a repetition of the experiment. Had Dr. Pereira pursued his observations, there can be little doubt but that it would not have been reserved for the present time to show the real action of this medicine. The numerous cases that have lately been made public have incontestably proved that the action of digitalis is in many instances at least the reverse of depressing, and free from the dangers that have commonly been attributed to it.

The chapter on aneurysm of the aorta forms quite an essay on that malady, and would have merited attention had it even been separate;

and indeed it may be doubted whether, commercially considered, it might not have been wise to have divided the work into two or more independent volumes. Taken, moreover, as it is presented to us, it may perhaps further be affirmed that this would have been a gain to the reader, since the first part comprises the elementary topics of the principles of physical diagnosis, and their application to the investigation of diseases of the chest—topics which must of necessity be subsequently more or less iterated in the application of these to the study of the several forms of thoracic disease. Although, therefore, we think that both student and practitioner would have had cause to rejoice in some measure of abridgment or condensation, we can safely commend this work to their attention, as conveying a vast amount of scientific information, and as furnishing a full and safe practical guide in treatment.

Review III.

1. *Contributions to Practical Medicine.* By James Begbie, M.D., F.R.S.E., &c.—*Edinburgh,* 1862. pp. 318.
2. *Clinical Medicine: Observations recorded at the Bedside, with Commentaries.* By W. T. Gairdner, M.D.—*Edinburgh,* 1862. pp. 741.

All who regard the progress of medicine with an intelligent and friendly eye, must have witnessed with sincere gratification the prodigious advance that has taken place in recent times in microscopical anatomy and in physiological chemistry. That the inquiries into these subjects which have been so ardently pursued, are steps in the right direction we most willingly admit. But we cannot help thinking that the zeal with which these fascinating subjects have been prosecuted, has led us too much to regard them as an end, and in some measure to lose sight of the great truth, that in our character of physicians the ultimate object which we should never lose sight of in any of our investigations, ought to be the extension and improvement of the resources of medicine as a practical art, which has in view the relief of human suffering and the prolongation of human life. It is scarcely possible to deny that some years ago this tendency was discoverable in certain departments of medical literature to such an extent, that it seemed as if medicine were cultivated much more with reference to its biological relations than for any practical results. The evils of such views were neither few nor small. Nor were they confined altogether to the ranks of the profession; they had an influence on the general public anything but favourable to the credit and position of medical men. How this came to pass it might, we think, neither be useless to inquire, nor difficult to explain. At present, however, we content ourselves with merely adverting to the subject, and congratulating ourselves that a more sedate and healthy tone of medical investigation is again asserting itself—a fact, of which the appearance of such works as Dr. Begbie's and Dr. Gairdner's affords most agreeable and satisfactory evidence.

The names of the two authors whose works stand at the head of this Article, are a sufficient guarantee that we do not intend, in these few remarks, to recommend either a blind adherence to antiquated opinions, which the rigorous scrutiny of modern investigation may have proved to be erroneous, or the practice of an empirical routine, unsupported by any stable or accredited principles. Both these writers are men of too high standing and too extensive information in the history of our profession and in the recent progress of scientific inquiries, for any one to suppose that what they write tends to exemplify or encourage such notions. Dr. Begbie is a physician whose large experience ranges over a long series of years, and whose great sagacity and judgment give the highest value to the observations which, in his present volume, he has given to the profession. Dr. Gairdner is a man of high accomplishments, of extensive information, and has deservedly a high reputation for independent and philosophical habits of thought. No retrogressive tendencies need be feared in the writings of two such men as these. What we admire in the writings of both, especially Dr. Gairdner, is the sobriety and caution of their views and deductions. Perfectly familiar, as both of them evidently are, with all the facts that tend to support the humoral pathology now so much in favour, they do not lay a greater stress upon these facts than they are legitimately capable of bearing. They do not represent the knowledge of the numerous chemical actions and transformations that go on in the animal economy, as affording either a solution of the great mystery of life, or a complete account of the whole phenomena of disease. Full importance is assigned to them as a series of sequences of great value and interest in certain morbid and certain restorative processes. But they are treated as nothing more, nor are they exalted beyond their just value, to the exclusion of what Lord Bacon calls those "middle principles" which lie between the domain of established but isolated facts, and those higher generalizations after which the human mind ever aspires, but which in many branches of science, as well as our own, are still dimly seen or vaguely guessed at, if not altogether unrevealed.

Having thus shortly characterized the two interesting works which we have classed together, we proceed to give such an account of them individually as our space enables us to offer, beginning with that of Dr. Begbie.

The Papers which form the present volume, the author informs us, have all appeared in the periodicals of the day, and are now collected and reprinted with such additions or corrections as subsequent experience has suggested to him. The first paper treats of gout and the gouty diathesis, and will richly repay a very careful perusal. Most, if not all, practitioners are probably familiar with the symptoms of a fit of regular gout, and also, we hope, with the minute and graphic description given of it by Sydenham. But even in this its most perfect and typical form, it ought, according to modern investigation, to be regarded as the local manifestation of a certain depraved or vitiated state of the constitution, called by medical men a gouty diathesis, which, we need hardly say, may produce numerous and multiform derangements both of function and structure in every organ and texture of the frame, complicating and modifying almost every diseased process to which these organs and tissues are liable, even though the person suffering these has never experienced a regular fit of the gout. Dr. Begbie tells us it is not his design to delineate the gouty diathesis, and this, considering the importance and difficulty of the subject, and the qualifications he has shown for the task, we deeply regret. With a candour and uprightness all the more praiseworthy that they are not always met with, even in quarters where we might expect to find them, Dr. Begbie has narrated his unsuccessful, as well as his fortunate cases. They are told clearly, succinctly, and without the omission of any particulars necessary for their full comprehension. But after reading them very attentively, we feel how desirable it would have been if the author had described the various signs or symptoms by which he has been able to distinguish that constitutional taint which, wherever it exists, is capable of so modifying local inflammation that it ceases to be amenable to those laws which regulate its ordinary progress. We do not find much to guide us in this respect, except that the connexion which Dr. Begbie has shown to exist betwixt the gouty tendency and inflammation of any organ whatever, imposes on us the necessity of rigorous inquiry, to ascertain whether the sufferer himself, or any of his relatives, have ever shown symptoms of gout, an inquiry all the more needful if the disease prove obstinately untractable under the use of remedies usually successful.

The cases detailed by Dr. Begbie, we think, establish the modifying influence of gout over diseases of the nervous centres and of the nerves issuing from them—of the circulating system—of the organs of sense —of the digestive apparatus—and of the synovial, mucous, and serous membranes. We cannot condense these cases, but the following are the chief conclusions the author deduces from them, conclusions which it will be seen coincide with those arrived at by other workers in the same field.

Gout is a constitutional disease, sometimes hereditary, sometimes acquired, dependent on the circulation of a *materies morbi* produced by mal-assimilation. The gouty diathesis is intimately related to the existence of uric acid and its compounds in the system. There are various stages of this diathesis differing in duration and in type, depending, Dr. Begbie thinks, on a poison circulating in the blood. That the primary effects of this are sometimes slow disturbance of functions, chiefly those of the digestive organs and the nervous centres, sometimes active disease, in certain tissues especially, the synovial, serous, and mucous membranes. That the occurrence of active gout appears to be useful for a time by eliminating the morbid material from the system. That the contaminated blood eventually weakens the tone of the heart and bloodvessels, producing dilatation and attenuation of their cavities and textures, thus leading to congestion and obstruction, and all their consequences, and finally to structural disease from earthy deposit in the circulating apparatus, thus connecting gout with cerebral disease on the one hand, and severe and fatal cardiac affections on the other. While we willingly acknowledge the cautious and moderate way in which the author states his views respecting the order of succession in which the phenomena present themselves; still we must admit that much remains to be done before we can be said to have arrived at a knowledge of the *primum mobile* of the disease. We have not space to compare the old Cullenian pathology with modern views, but we cannot help being struck with the absence of reference to the nervous system in Dr. Begbie's attempts to indicate the causation of the disease. We can do no more than throw out this suggestion on the subject, and with a brief account of Dr. Begbie's remedial views, we take leave of this instructive and interesting paper. Perfect temperance, active exercise, and care, especially, of the functions of the skin, is looked upon by our author, as by others, as the basis of preventive treatment. While during the fit, after clearing the *primæ viæ*, colchicum wine and the alkaline diuretics, especially the salts of potash, the former in small doses perseveringly used, are what he chiefly confides in. The same remedies he advises in all internal inflammations, indeed in almost all diseased conditions of any organ, where an association of the disease with a gouty tendency can be traced or suspected, either from the history of the patient, or from the anomalous character and unwonted obstinacy of the symptoms under ordinary treatment. This we take to be the main practical lesson of the paper; remembering, however, that the depurants which the author advises must often be combined with such means as each special class of cases may require. He also places great reliance on cod liver oil as a means of combating every form of gouty cachexia, and improving, as far as they can be improved, the nodosities which are some of the most distressing of gouty sequelæ.

Following the paper on gout, there are two on the connexion between rheumatism and chorea, and of the first-named disease with erythema nodosum; both are interesting, the latter especially; but we cannot now analyze them. We think Dr. Begbie successful in both cases in tracing the association which he seeks to establish, especially in the case of rheumatism and erythema nodosum, a striking instance of which lately came under our own observation, in a healthy woman of fifty, worn out by anxiety and long watching. There is a marked discrepancy between the treatment of rheumatism spoken of by Dr. Begbie, with strong approval, at p. 87, which regards venesection with calomel and opium as the chief therapeutic agents, and the depuratory mode of management by the salts of potash, which he represents at p. 93 as that which he ordinarily employs, owing, we presume, to his adoption of the modern humoral pathology. At another part of the volume he seems still to have recourse occasionally to bloodletting as at least an auxiliary. We could have wished for a little more definite information from one so well qualified to give it, as to his practical views. Certainly there are not many cases of rheumatism more successful than the first case recorded by Dr. Begbie in the Paper on Rheumatism and Chorea, where the treatment was strictly antiphlogistic, and the patient was convalescent in little more than a week. There is a remark in Dr. Begbie's book to the effect that the plausibility of the views of the Solidists led us long to shut our eyes to the state of the fluids, especially the blood. It is most true and just, and serious error was the result. But the human mind is prone to extremes. And human progress, in everything but the exact sciences, resembles more a series of oscillations in which the tendency to reach a certain terminus may be clearly observed, than a steady and unfaltering advance. Let us beware, then, that a too exclusive devotion to animal chemistry, or any one subject, does not lead us astray in the opposite direction. The history of medicine proves that the reverse of error is not always or necessarily truth.

The paper on anæmia is so good that we can only commend it to careful study. In the next, on the oxalic diathesis, we do not find much to detain us. That on fatty degeneration of the heart, consists mostly of a narrative of the cases of Dr. Chalmers and Dr. Abercrombie; and that on Diphtheria is so recent, and we believe so well known and highly estimated, that we may safely pass it over. The papers on Stramonium and Arsenic deserve great consideration, not so much on account of what they contain, as from their bringing emphatically before us the testimony of a very competent and trustworthy observer to the value of strictly remedial medication in certain cases at least. The tendency of medical thought has recently been to make general hygienic considerations every thing, and remedies, properly so-called, nothing, or next to nothing. We have therefore great pleasure in listening to Dr. Begbie's exposition of opinions very opposite to this view—itself a reaction from previous exaggeration—so far as regards the use of arsenic and the extract of datura stramonium. The latter given in doses of one-third of a grain three times a day, Dr. Begbie regardsas a most valuable narcotic and antispasmodic, superior to Dr. Hunt's favourite remedy, belladonna, in many cases of neuralgia. In the former, his confidence is very great, in chorea, in skin diseases, in obscure diseases of the uterine system, and wherever a rheumatic taint may be traced or suspected, given very steadily till slight inflammation of the eyelids, and a very thin, silvery film on the tongue, shows the physiological action of the drug, and then in smaller doses cautiously continued for a long time.

Dr. Begbie is a strenuous humoralist, and we do not quarrel with him on this score; but we beg to remark that, even when principles well ascertained are applied to subjects where we do not know *the whole* of the case, the reasoning becomes hypothetical, because what is assumed to make it applicable *is* hypothesis. What mischief has resulted to our science and our art from the adoption of mere hypotheses, and the more subtle form of hypothetical reasoning we have alluded to, we apprehend few men in the profession know better than Dr. Begbie himself.

The work of Dr. Gairdner, which we now proceed to notice, is characterized by different qualities from those displayed in the volume of Dr. Begbie. And without making any invidious comparison, we have no scruple in saying, it is not inferior either in respect of interest or of intrinsic merit. Throughout, it bears the marks of careful observation and scrupulous fidelity, as well as such a readiness and skill in illustration as must place the author very high in the rank of a clinical teacher. But what is perhaps most commendable in the book, is the homage it everywhere pays to the supremacy of truth—truth in matters of fact, and an unswerving desire to ascertain the truth also in matters of speculation. A quality this, inestimably important to the philosophical character of a writer, as well as to his moral status, and which, without being obtrusively or ostentatiously displayed, is unmistakably exhibited in every statement which Dr. Gairdner puts forth, and in every deduction he draws from his facts. We must not spend time, however, in preliminary dissertation, but, so far as we are able, proceed to give an account of the book, or at least of such portions of it as we can select for observation or analysis.

The first papers which we shall notice are those on pneumonia, at the commencement of the volume; and we shall do little more than abridge the summary Dr. Gairdner gives of the principles which regulate the treatment of this disease. Blood-letting is a remedy of great power in cases adapted for its use—that is, in its very early stages —in vigorous constitutions, and when the attending fever is of an inflammatory type, and the symptoms urgent; without attention to these conditions, the practice is sure to pass into a dangerous routine. Hence the extreme difficulty of obtaining comparative statistics respecting the treatment of the disease, for if selected cases only are bled, they cannot be set against unselected cases, or against cases selected for their adaptation to an opposite mode of treatment, and without selection, the application of depleting measures is vicious *ab initiô* as a basis of reasoning, and morally wrong. The statistics of Louis and Grisolle, however, show the power of bleeding to modify the progress and abridge the duration of the disease *in cases adapted for its use.* Active remedies in pneumonia must be regulated by symptoms, not by physical signs, for the modern idea of the malady includes many cases not recognised as such in former times, and differing essentially in the pathological state of the constitution, from the pneumonia of Cullen, characterized by high synovial fever and urgent pain and dyspnœa—a class of symptoms now very rarely witnessed, and still, when seen, Dr. Gairdner thinks, indicating antiphlogistic remedies. Lastly, we are reminded of the necessity in all cases of bearing in mind the efforts of the tendency to a spontaneous recovery. We ourselves concur entirely in these views, and we believe they will commend themselves to the good sense and sober thinking of the experienced members of our profession.

The papers on pneumonia are followed by two on alcoholic stimulants, which are remarkable for their good sense and moderation. We agree in Dr. Gairdner's view, that with every feeling of respect for the motives of the advocates of total abstinence, the principle on which they rest is a fallacy; and all conduct resting on a fallacious principle is necessarily insecure. Moreover, we are strongly inclined to believe that a large portion of the reputed good ascribed to their exertions has resulted from the dissemination among the thoughtful and intelligent of the working classes, who are not themselves abstainers, of information relating to the evils, moral and economical, that flow from excess.

There is not much to detain us in a short paper on influenza, and we then come to a series on typhus, enteric, and scarlet fevers. In regard to the typhus and enteric fevers, Dr. Gairdner's chief object is to establish the truth of Dr. Jenner's views as to the perfect distinctness of the two diseases—an object which he has prosecuted with great acuteness and industry. And if Dr. Jenner's field of observation has been more extensive, the comparatively narrow bounds within which our author's investigations have been restricted afford perhaps a more favourable opportunity for an exhaustive inquiry into all the circumstances connected with the origin and consequences of separate groups of the two diseases, than can be accomplished in the much wider area over which Dr. Jenner's observations extend. Dr. Gairdner has not limited himself to the consideration of the cases that have been brought under his notice in his hospital practice. He seems to have sought out and examined carefully and candidly all the cases of reputed fever he could ascertain to exist in Edinburgh, and traced laboriously their origin and their results; and the upshot of his inquiries is that in no case has he been able to trace enteric fever to the contagion of typhus or the reverse. Groups of the former disease and successive cases of the other, he has found to exist at the same time in different localities, but in no instance was there ever any substitution or interchange of the two diseases among the respective groups of each. Such a statement from so trustworthy an authority is doubtless important. But an objector might remark that, after all, Dr. Gairdner's evidence is little more than negative, by which it is not very easy to prove a positive opinion; and that from the very interesting and satisfactory diminution of fever in Edinburgh during the last twelve years—a decrease all the more gratifying that we cannot avoid associating it with the better feeding, better clothing, and increased comfort of the poorer classes. Dr. Gairdner's numbers are small enough to warrant a very determinate conclusion. We must observe, too, that Dr. Gairdner founds his diagnosis exclusively on the appearance of the two eruptions of typhus and typhoïd fevers, giving up all idea of distinguishing them by their symptoms. Referring to our own experience, we think we have seen cases absolutely typical, so far as symptoms go, of enteric fever which have presented before their termination all the characteristic appearances of malignant typhus; and we state this the more confidently that a precisely similar case was mentioned to us a very short time ago by a highly-accomplished physician of great experience both in hospital and general practice. However painful, then, it may be to have established opinions unsettled, we can scarce regret that the question of the identity or non-identity of typhus and enteric fever has been reopened by Dr. Henry Kennedy, of Dublin. One of the most valuable and interesting features of Dr. Gairdner's work is its verisimilitude. Reading it is like listening to the commentaries of an able and skilful teacher, on cases that we actually see and watch. But the close application of his remarks to cases immediately under his superintendence, and his obvious reluctance to swell his book by general dissertations, sometimes makes us regret that he had not entered a little more fully into the general history and treatment of the diseases he considers. The epidemic scarlatina which Dr. Gairdner describes has evidently been mild. Still scarlatina is one of the most formidable diseases which occur in this island. His remarks on the treatment are extremely judicious, but there is prodigious diversity in the appearances it presents and the course that it runs; and the practitioner, while he avoids the "*nimia* diligentia" of Sydenham, cannot too carefully watch the progress of the disease, or be too guarded in his prognosis, till every symptom of illness is conclusively at an end. Dr. Gairdner cautions his pupils against being too much alarmed by the occurrence of delirium in this disease, because the course of the fever being short, this symptom will probably subside when the crisis of the fever is over. Speaking generally, he is right; unquestionably so, if the delirium is mild or only partial, and not always persistent or complete. But there is a delirium of a turbulent character, and accompanied with excessive restlessness, which we deem most serious. When it occurs, as we have generally seen it do, on the fourth or fifth day—mostly the fourth—when the little patient picks, or rather tears, at the lips and tongue, or moves the hands before the eyes with a kind of spinning motion, we have repeatedly, even in cases that previously seemed tolerably mild, seen it prove fatal in less than forty-eight hours. We would suggest to those of our brethren who have numerous opportunities of post-mortem examination, to look for any evidences that may exist of congestion or inflammatory exudation in the inferior surfaces of the brain, in the striated bodies, or optic thalami. Where the pulse will bear it, which often, however, is not the case, we have sometimes seen benefit from very cautious leeching at the back of the neck.

A very good paper on cholera (malignant) follows the one we have been adverting to. It presents an excellent summary of what may be considered as established facts, especially as regards the Edinburgh epidemics, in the history of that terrible disease, without entering, however, into the vexed question of its contagious or non-contagious nature. We need not dwell on the pathological statements of this paper. The inferences deducible from them are almost entirely of a negative character. But to us, as to Dr. Gairdner, it appears pretty well made out that the characteristic symptoms of the collapse stages of the disease are the result of the enormous choleraic evacuations from the bowels, by which the serum, with the salts it contains, as well as albumen, are rapidly drained off from the blood. A great deal no doubt remains behind this, supposing it true, respecting the primary effects as well as the nature of the poison which produces the disease. But if this be the order of consecution so far, at least something is gained. And we are able partially to account for the reported success, at least comparatively speaking, as stated in Mr. Ross's tables, of a treatment which seems to depend on the restoration of the abstracted fluid by the administration of cold water or of ice, and by maintaining the body throughout in contact with fluid media or aqueous vapour—a treatment which seems, in addition, germane to the feelings of the patient.

The paper on delirium tremens is very sensible, and probably expresses the sentiments that have been growing among the more thoughtful of the profession for a long time—a total disbelief in the absolute and specific efficacy of opium and alcoholic stimulants in this disease, and a disposition, in certain cases at least, to trust to the gradual elimination of the poison by the efforts of nature, aided by such appliances as the particular symptoms of each individual case may suggest. If the statements brought forward regarding the treatment of delirium tremens by digitalis be corroborated by further experience, and if the physiological effects of the medicine are not exhibited when it is given in half-ounce doses, but are produced, even during the course of delirium tremens, when the quantity is diminished, as has been stated, it is a fact which, to say the least of it, opens up some very extraordinary topics of speculation.

We now draw our remarks on Dr. Gairdner to a close, but we cannot do so without characterizing, though we have not space to analyse, his various papers on diseases of the chest and their physical diagnosis. Highly as we estimate those we have so imperfectly gone over, we do not know but what those in the latter part of the volume are more valuable still. Certainly they are in no respect inferior, to say the very least, displaying throughout a calm, sagacious, and unprejudiced spirit of investigation, and a truly judicial temper in estimating the value of signs and the efficacy of remedies. There is much ingenuity and acuteness in the remarks which he makes on the cases of pneumothorax with regard especially to its diagnostic signs; while the whole discussion of pleuritic effusions is extremely temperate, rational, and fair, especially with respect to the evacuation of pleuritic effusions by puncture of the thoracic parietes. Dr. Gairdner does not appear to be a zealous advocate of this operation in acute pleurisy, while he by no means peremptorily forbids it in all cases, stating, however, that only twice in eleven years of extensive practice has he felt called upon to perform it. In the operation he has always employed the exhausting syringe introduced by Dr. Bowditch, of Boston, U.S., since he first became acquainted with that instrument.

The chapter on cardiac murmurs is an exceedingly interesting and useful one, more complete and exhaustive than most of the other papers in the volume. We regret the less our inability to give an account of it, because it will well repay attentive perusal, and indeed cannot be read with the full profit that may be derived from it otherwise than *in extenso*, and with the aid of the accompanying diagrams. Notwithstanding all that has been written on the subject, it is a valuable addition to the literature of auscultation.* It is, however, the clear exposition that is given of the relation that subsists between

* To this subject, as treated by Dr. Gairdner, we alluded at p. 259 of the 59th number of our Review.

physical signs and the symptoms of disease, and the just estimate that is shown of their comparative value in the cases detailed, and the accompanying remarks, which constitute, in our judgment, the peculiar value of the whole series of papers on chest diseases, as well as that on cardiac murmurs. It cannot be doubted that in practice serious mischief has resulted from a too exclusive regard to mere physical signs, without a corresponding attention to symptoms.

The sounds heard in the chest are merely acoustic phenomena, resulting from the condition of the organs it contains, and partly of the parietes that surround them. The relation that subsists between these is so far from being altogether definite, that it often requires much thought and careful study, as Dr. Gairdner clearly shows, to decipher their true meaning and import, even with reference to the organs we are examining. Further, signs give us no information as to the pathological state of the constitution, from which chiefly our indications of treatment must be drawn. Symptoms, as is truly said by Dr. John Brown, are the mother-tongue of Nature—signs no more than a foreign language.

We take leave of Dr. Gairdner and also of Dr. Begbie, by expressing our sincere hope that we may soon meet either or both in the field of medical literature, in the cultivation of which they are so highly qualified to distinguish themselves.

REVIEW IV.

Lecture on Milk; on the Composition of Cheese, and on Practical Mistakes in Cheese-making; Cheese Experiments; on Poisonous Cheese. By Dr. AUGUSTUS VOELCKER. (Reprinted from Vols. xxii. and xxiii. of the 'Journal of the Royal Agricultural Society of England.')

IT is very interesting to see the methods of modern science applied to the illustration and improvement of some of the oldest of the arts. Dr. Augustus Voelcker's papers on the products of the dairy are a happy example of this. The limits to which we are restricted allow us to notice only some of the results of his practical inquiries. *In limine*, however, we have no hesitation in stating that each of the papers of which we have prefixed the title is deserving of being carefully read and studied, and not only by the farmer but also by the medical man; by the one for his own profit, by the other for the benefit of his patients—milk, butter, and cheese being acknowledged as three of the most important articles of food, and each being subject to adulteration, so as to injuriously affect the health, and the last mentioned (cheese) even to a poisonous degree.

In treating of milk, Dr. Voelcker, after describing its ordinary qualities and composition, gives some instructive particulars of the various circumstances by which these qualities are altered or modified, and of the marked differences as to composition of the milk of different animals.

In the instance of cow's milk, of all its ingredients, he finds the proportion of casein to be the least variable; that of the butter most. Of four specimens of new milk of which he gives an analysis, the butter in the richest was so much as 7·62 per cent.; in the poorest, no more than 1·99 per cent.; whilst the casein in the former was 3·31 per cent.; in the latter, 1·99 per cent. All four were specimens of genuine milk, not produced any way abnormally, and owed the difference which they exhibited, as he believes, to the richer pasturage in the one instance, and the poorer in the other.

The table which he gives, showing the composition of human milk, and of the milk of some other animals, is valuable, and especially practically for reference as to the regulating of the dilution of cow's milk for use in infancy, and the strengthening, if necessary, of ass's milk, if dietetically prescribed.

	Cow's.	Human.	Ass's.
Water	87·02	88·94	91·65
Butter	3·13	2·67	·11
Casein	4·48	3·92	1·82
Milk-sugar	4·77	4·33	6·08
Mineral matters (ash)	·60	·14	·34
	100·00	100·00	100·00

We omit the columns relating to the milk of the goat, the ewe, and the dog, all of them richer in animal matter; and especially that of the dog, which is introduced as an example of the milk of the carnivora. To the greater richness of the milk of the last he attributes the difficulty of bringing up a puppy by hand.

Regarding the influence of food on milk, he notices the interesting fact that there is no necessary connexion between the one and the other as to quality or quantity, inasmuch as if food be given in excess, it may be expended not in producing an increase of milk, but of fat or flesh, with even a diminution of milk; and further, if the excess be of oil-cake—that abounding in liquid fat—the butter yielded may partake of its nature, be unduly soft, and difficult of separation in the churning. In the use of lactometers, he points out a very necessary precaution, founded on the difference of the specific gravity of cream and milk; the former, as he finds, varying from 1·012 to 1·019; the latter from 1·030 to 1·032. Now, if the cream be skimmed from the milk, the specific gravity rising five degrees of the instrument, allows of the addition of 10 per cent. of water to reduce it to its original specific weight. It is thus he supposes that the adulteration of milk in large towns is commonly effected. To detect it, he very properly recommends the use of two kinds of lactometers—one for the ascertaining of the specific gravity, the other for measuring the volume of cream which rises to the surface in a certain time: it is a graduated tube. In its use he suggests a caution—that it is not applicable to milk that has been much agitated by a long railway journey. Another caution he gives, and a very important one—that milk, to keep, should be well cooled before it is sent away, and should be kept cool to keep long.

As regards cheese, the author makes two preliminary remarks: the first is, as respects the intimate connexion which he has observed to "exist on the one hand between good cheese and cleanliness, order, general intelligence, and desire to excel; and, on the other hand, between bad cheese, slovenliness, ignorance and practical conceit." The second is that, even in the best-managed dairies, cheese-making is practised entirely as an empirical art, admitting, it is allowed by the best judges, of great improvement.

Our English cheeses he describes as of three kinds, according to the manner in which they are made. One, such as Stilton, from milk to which cream has been added; another, such as the best Cheshire and double Gloucester, from whole milk—that is, new milk from which no cream has been taken; the third, such as the ordinary Cheshire and Gloucester, from skim-milk—that is, milk from which more or less cream has been taken for the making of butter. Though the richness of a cheese mainly depends on the proportion of butter it contains, he shows that it does not entirely so depend, but in part on the skill employed in its manufacture. This is well demonstrated by his analyses of different kinds of cheese, and by reference to their market value.

As a good and instructive example of the results of scientific research, we give his analysis of skim-milk cheese, and his remarks thereon:

"Milk," he says, "varies so much in quality that in one dairy a better and richer cheese can be made from milk which has been skimmed than in another where only the evening milk is skimmed and added to the whole new morning's milk.

"The following analyses clearly bring out this important fact:

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
Water	27·68	39·43	38·39	43·47	45·39
Butter	30·80	27·08	23·21	15·89	9·97
Casein	35·12	30·37	28·37	28·93	33·12
Milk-sugar, lactic acid, and extractive matters	1·46	·22	6·80	6·47	6·39
Mineral matters (ash)	4·94	2·90	3·23	4·84	5·13
	100·00	100·00	100·00	100·00	100·00

"No. 1," he remarks, "though made from skim-milk, is as rich in butter as good Cheshire cheese. It was rather more than six months old before it was analysed, when its quality was pronounced by several good judges to be excellent—superior, indeed, to most of the Gloucester cheese which I have ever tasted.

"No. 2 and No. 3, though not quite equal to No. 1, after keeping for six months turned out a very good cheese indeed.

"No. 4, it will be seen, contained only 16 per cent. of butter in round numbers, and nearly 44 per cent. of water. If such cheese can be sold at 7*d.* per lb., and butter at 1*s.* to 1*s.* 4*d.* per lb., I can well understand that it must pay a farmer to make nothing but skim-milk cheese, and to convert all the cream into butter.

"No. 5 was made of milk skimmed at least three times, and sold on the farm where it was made, to the labourers at 3*d.* per lb. Such cheese cannot be kept for any great length of time, for it soon gets so hard and horn-like that a pickaxe must be used to break it into pieces."

Into the details given by the author of the defects existing more or less in the methods in use at present practised in cheese-making, we cannot enter; and indeed, in the discussion, he says he must confine himself to notice the chief of errors.

The first great fault he points out is the one rising out of ignorance of ferments—those subtle agents which may be introduced into the milk by dirty fingers in milking, or by neglect of scrupulous cleansing of the vessels in use, or from proximity of an offensive pigsty or drain; milk, it is worthy of remark, being, from its unstable composition, probably a more delicate test of noxious agencies than any living organism. He well describes how a good dairy-woman from the very beginning unconsciously carries on a steady and constant battle with these remarkable ferments, straining the milk before admitting it into the cheese-tub, and no sooner has the cheese left the tub than she begins to pour in scalding water, and to scrub it and make it as clean as possible. He relates an instance of a cesspool close at hand baffling every attempt to make good cheese.

Passing over his remarks on the practical faults committed during the making of cheese, we shall notice briefly his observations on rennet and whey, which are original and important.

He rejects the assumed theory that casein is held in solution by a small quantity of alkali, and proves experimentally that rennet coagulates milk even when the whey that separates is neutral or has an alkaline reaction. How rennet takes effect he does not attempt to explain; at present, he thinks its action one *sui generis*, and as yet known only by its effects—in this respect, we may add, like so many others. The modifying influence of temperature he finds to be very remarkable—one of about 120° Fah. favouring it most; and beyond that, the contrary, 135° and upwards altogether arresting its coagulating influence. Whey, he finds, always contains a residuum of albuminous matter, a substance not affected by rennet, as also a portion of butter, besides the sugar of milk, its chief ingredient. He holds it to be in consequence very nourishing, and deserving, as an article of food, of more attention than is commonly paid to it. Hitherto its albuminous constituent has been mostly, if not entirely ignored. In perfectly clear whey he has found the albumen equal in quantity to about one-third of the casein separable by rennet. He says, "mixed with a little barley-meal, it constitutes the best food that can be given to pigs, for it fattens rapidly, and produces the most delicately flavoured bacon."

The following are some of the practical mistakes pointed out by him, which are made in keeping cheese. We quote them as worthy of being remembered by the lovers of good cheese:

"1. Cheese is deteriorated in quality when it is placed in damp or badly ventilated rooms.

"2. Cheese newly made is spoiled by not turning it frequently enough.

"3. Cheese does not ripen properly, and therefore remains deficient in flavour, if the temperature of the cheese-room is too low. . . . Any temperature under 60° is unfavourable.

"4. Cheese is also spoiled if the temperature of the cheese-room is too high—as high as 75°.

"5. Lastly, cheese is sometimes spoiled if the temperature of the cheese-room varies too much at different times."

On the subject of poisonous cheese, Dr. Voelcker communicates some curious particulars. He describes two kinds—one kind owing its noxious qualities to the nefarious practice of adding sulphate of zinc or sulphate of copper to the cheese; the sulphate of zinc, to impart to "fresh cheese the peculiar biting taste of old;" the sulphate of copper, to prevent what is called "heaving." These are salts which are easily detected by the analytical chemist. The other kind of poisonous cheese, it would appear, owes its noxious quality to a "something" generated in the cheese, at present not admitting of detection by chemical means, and of a nature analogous to the poison occasionally engendered in German sausage. Instances are given by the author of persons suffering severely from partaking of such cheese, and even of death from its effects. After remarking, "we are as yet as far as ever from knowing the composition of this virulent poison," he adds, "this, however, we know, that it is developed when the curd of milk is kept too long exposed to the air before it is salted; or kept in damp, badly ventilated places; or when too much whey is left. In fact, all the circumstances which tend to produce acid and to generate free fatty acids, are apt to produce this particular poison. In old cheese, it is true, we have similar fatty acids, but they are here united with ammonia, and in this combination harmless. What is more strange, poisonous cheese of this character, when kept until it becomes quite decayed, loses its poisonous properties and becomes wholesome."

We are glad to learn that the information, thus varied and important, contained in these papers, is merely the firstfruits of the author's researches, and that the inquiry is continued in progress in the Royal Agricultural College, Cirencester, of which Dr. Voelcker has the direction. What he has already accomplished, has done much, we think, to remove the opprobrium "that hitherto," as he states, "scarcely anything directly bearing on dairy practice has been done by scientific men;" and looking forward to his further results, we have confidence that these will be as enlightening, and will remove the opprobrium entirely.

Review V.

1. *Lectures on Syphilitic and Vaccino-Syphilitic Inoculations: their Prevention, Diagnosis, and Treatment.* Illustrated by Coloured Plates. By Henry Lee, F.R.C.S. Second Edition.—*London,* 1863. pp. 335.

2. *Sifilide trasmessa per Mezzo della Vaccinazione in Rivolta presso Acqui.* Per Giacinto Pacchiotti, Professore nella R. Universita di Torino, &c.

Syphilis transmitted by Vaccination in Rivolta. By G. Pacchiotti, Professor at the University of Turin.

It would be superfluous to lay before the readers of this Journal an extended account of Mr. Henry Lee's researches on syphilis, the result of his observations having been submitted to the profession mainly through the medium of our pages. It will suffice, therefore, that we announce the second edition of an independent work embodying the author's former and later experience, and the confirmation of his views by later or contemporary observers. In addition to a full statement of these, our readers will find in the work before us a complete account and close scrutiny of the history of the vaccino-syphilitic inoculations at Rivolta, which were brought under the notice of the profession in England, in the pages of the 'Lancet,' in 1861. The statements there given, it was felt by many, were so far from satisfactory as to be incredible, in the face of the great fact that among the millions who since the date of Jenner's discovery had received the protection of vaccination, no such lamentable occurrence had been known. It is true that Mr. Lee had collected in his paper published in the 'Medical and Chirurgical Transactions, 1861,' the opinions of eight medical men who believed in the possibility of the transmission of syphilis by vaccination; but these expressions of belief were loosely stated, and totally devoid of even a semblance of proof. These opinions, therefore, were worth nothing against the almost universal experience to the contrary. Mr. Lee objects to this general opinion that syphilis cannot be communicated by vaccination, that vaccinated children are not watched long enough to ensure that no such results have followed within the period required for the incubation of constitutional syphilis. Against this objection, however, it must be urged that although a large number of children are not seen again by the public vaccinators after the eighth day, yet that there remains a vast number of children, vaccinated by private practitioners, who not only continue under observation by the medical man for more than the required period of incubation, but continue personally known to their medical attendants for many years of their lives; and yet such accidents have not been met with. It was not, therefore, without having assigned good reasons for his scepticism, that Mr. Kesteven* urged the probability of some grave fallacy in the narration of the alleged transmission of syphilis by vaccination at

* Lancet, November, 1861.

Rivolta, and suggested the possibility of the introduction of syphilitic virus *simultaneously with, and not merely through the means of vaccination.* The occurrence deservedly attracted considerable attention at the time, and the cases were formally discussed at a meeting of the Officers of Health of the Metropolis, on the reading of a paper by Dr. Ballard. No satisfactory explanation of the Rivolta cases was, however, adduced, and the question remained unsolved until the publication of the report on these cases by Dr. Pacchiotti.

This report has been studied by Mr. Lee, who bringing to its study a more profound insight into syphilitic disease than is possessed by most men, has adopted the conclusions of Dr. Pacchiotti. These, in short, are:—That the disease observed in the children at Rivolta was syphilis, transmitted through vaccination; that mothers were infected with syphilis by nursing the children—proving that the mothers were previously healthy, and therefore that the disease in the children was not hereditary syphilis; that in two instances the disease was communicated to a brother and sister by kissing; that the disease was also communicated in a natural way to husbands; that re-vaccination subsequently practised on the children produced no result; that the vaccino-syphilis had its origin in one vaccinifer who had two or three months before vaccination contracted syphilis from the breast of a syphilitic woman, who at the same time infected another child; that the syphilis was transmitted by him to the rest by means of the said vaccination.

The profession in England is greatly indebted to Mr. Lee for the pains he has bestowed upon the elucidation of this alarming history. It should not be forgotten that he has accompanied his conclusions by an earnest warning of caution and care in the selection of vaccinifers for the communication of vaccine disease. It is clearly shown that syphilis was introduced "simultaneously with, and not merely through the medium of vaccination," as shown by the fact that the vaccination had proved successful, while the sad story that has followed might with due care have been prevented.

We would earnestly commend to our readers these chapters of Mr. Lee's work, as they not only furnish an example of painstaking close inquiries by the Italian Commission, but they supply a picture of the natural history of syphilis, happily not often to be met with. Mr. Lee has given us a sketch of what syphilis was when first known in Europe, and a comparison of this with the outbreak of the disease at Rivolta, the whole forming a most valuable contribution to pathology, medical jurisprudence, and the history of medical science.

Review VI.

Mémoires de Médecine et Chirurgie pratiques. Par le Dr. Prosper Hullin.—*Paris*, 1862.

Practical Memoirs on Medicine and Surgery. By Dr. P. Hullin.—*Paris.* pp. 527.

The thirteen chapters of which this volume is composed contain descriptions of instruments invented by the author, notes of cases, and numerous observations made during the past forty years in a laborious country practice. The fact that some of them were written twenty or thirty years ago, and are now for the first time published in form of a book, detracts something from their value; but they offer a remarkable instance of the aid that a "country practitioner" can render to medical science by taking notes of his cases and reflecting on them carefully.

Chapter I. *On a New Instrument and a New Method employed for removing Polypi from the Uterus, Vagina, and Rectum.*—The principle on which the ligature is passed round the pedicle of a large polypus is essentially the same as that recommended by Gooch. The thread is carried up on two long and flattened needles with rounded ends, instead of being run through the double canula; but the mode of tightening the thread and constricting the polypus is original. A long metal stem, broad at its base and tapering gradually towards its upper end, at right angles to which a ring projects, is pushed up in the vagina to the pedicle of the polypus, the lower ends of the needles being slipped through the ring; the needles are next withdrawn through the ring so as to bring down with them the ends of the ligature; these are fastened to a pin which runs across the base of the stem, and is turned round by a handle. The ligature is thus tightened at will and the polypus constricted, the movement being regulated by a rack and pinion.

This seems to be the most ingenious plan that we have seen for applying the ligature; but inasmuch as all the best authorities recommend excision even in the case of the largest polypi, it will probably not find much favour in this country. Besides, if removal by constriction is preferred, the écraseur with the wire noose, as employed by M. Maisonneuve in Paris, or the galvano-caustic sling connected with Middeldorpf's apparatus, the good effects and rapid action of which we have often witnessed in the wards of Prof. Braun, of Vienna, must be preferred to the above tedious method.

Chapter II. *On some Instruments and Plans for Extracting a Large Child from a Small Pelvis.*—The first instrument, called the *cephalapagotome*, is used, as its name implies, for perforating and extracting the head. The stem is of metal, about a foot long, mounted on a strong ebony handle, and ending in a large spear-head. It is in two pieces; an outer, cylindrical, serves as a sheath for an inner solid piece, which moves freely up and down inside it, and passes right through the middle of the handle, into which the outer cylinder is fixed, to be connected with a small ivory knob that protrudes from the bottom of the instrument. A disc or plate encircles the stem completely at its upper end, and can be slid up and down at will with the inner moveable piece, with which it is connected by a screw that passes through a slit in the outer cylinder. The spear-head is flattened and sharpened at the point and sides; on either side of its base is fixed by a moveable hinge an extra barb or wing connected with two strings that pass down the handle, one of which pulls the barb close to the side of the stem, where it is fixed by the disc, when the spear is to be used for thrusting and cutting; the other jerks it out at right angles to the stem, so that it can act as a barb when extraction is necessary. The disc serves a twofold purpose; it lodges the ends of the barbs in two little holes or depressions on its upper concave surface, and when the spear has been thrust into the head of the child, it is pushed firmly up against the scalp, where it fixes the instrument and prevents it from slipping out and injuring the soft parts of the mother.

The other instrument, or *multiple forceps*, has for its base the common forceps of Levret, made thicker and stronger than usual. The inner or concave surface of the fenestrated blade is pierced with six holes, into which can be screwed as many small hooks directed inwards and curved downwards, so as to fix firmly the head of the dead fœtus. In order to protect the soft parts of the mother, a wooden guard is made to fit accurately the inner concave surface of the blade, and is pierced with six holes, in which are received and hidden the six hooks; this can be slipped from the blade and withdrawn as soon as the latter is applied to the head of the fœtus. It is further made to act as a *cephalotribe* by the addition of two plates with the same shape and curve as the blades, but not fenestrated: they have also their inner surfaces roughened, and are fixed by screws to the concavity of the blades. Pressure is effected by a long screw which passes through both handles at the most prominent part of their curve, and can approximate them forcibly. Several experiments made on the dead subject are related to show how and when these instruments are applicable, but, unfortunately, nothing is said of their employment in midwifery.

The cephalapagotome combines in itself the perforator and crotchet. If, however, the pelvis be very narrow, or the head very large, it is merely used as a perforator and to fix the head, which is then crushed by the cephalotribe, and extracted by the hooked forceps. He points out its special use after decapitation has been resorted to, where the head is left in the uterus, in a case of very narrow pelvis: an instrument to fix the head for the cephalotribe is here essential. He concludes that, in any case in which the diameters of the pelvis are not less than $5\frac{1}{2}$ centimètres in the conjugate and 11 in the transversal, the woman can be delivered by his instruments, and saved from the Cæsarean section.

CHAPTER III. *Practical and Phrenological Remarks on a severe Case of Puerperal Convulsions.*—A primipara, in the seventh month of pregnancy, is seized with convulsions, which last three days and four nights, but are followed by recovery. The treatment adopted here, and recommended by him in all such cases, consisted of general and local bloodletting; dilatation of the os, and extraction of the child as quickly as possible; the employment of tepid baths, water at a lower temperature than that of the bath being poured on the head. Himself a believer in phrenology, he argues that the doctrine of Gall and Spurzheim is confirmed by the fact that the woman recovered her faculties one by one and gradually, not at once and simultaneously.

CHAPTER IV.—A case is related of *compound comminuted fracture of the tibia and fibula,* which at first resisted all treatment, and showed signs of becoming gangrenous, but was healed by the following plan: The limb, previously bandaged, was laid on a pillow, the sides of which were adapted to it by bands passed beneath the pillow, drawn up, and tightened above. After a time the leg was further supported by what he calls a "corset"—a band stiffened with whalebone, that encircled the leg incompletely, so as to leave the wound exposed for dressing.

CHAPTER V. *On Vaccination and the Vaccine Lymph; Re-vaccination; a New Form of Lancet; how to Preserve the Lymph.*—This, by far the most interesting chapter, is based on a large practical experience of forty years, and observation of more than 20,000 cases. He confirms in a remarkable manner the views that have been so prominently brought forward in later times—that re-vaccination and repeated renewal of the vaccine lymph are most necessary. For brevity's sake, the results of his experience will be given in the form of a series of propositions.

Of Vaccination.—1. That the day on which the lymph is to be taken cannot positively be specified, as the progress made by the vesicle varies in different individuals; but that, as a rule, the poison is most active after the fifth and before the end of the eighth day, while the vesicle is still small, is becoming umbilicated, and contains a small quantity of white viscid lymph. 2. That vaccination exerts a favourable influence on certain diseases, shortening their duration and hastening the healing process—e.g., pertussis; certain eruptions, especially prurigo (Biett was in the habit of inoculating those suffering from prurigo formicans, with the best results); intermittent fever; tubercle (he has observed in the district of Mortagne, the field in which he has laboured, that in proportion as variola has been driven out by the spread of vaccination, so have the number of deaths from tubercle diminished). 3. That on the same individual the vesicles are not all developed *pari passu,* but some later than others. 4. That the lymph of the false vesicle—Trousseau's vaccinoïd or modified vesicle—which is developed in a soil not fitted for the reception of the virus, will reproduce a true vesicle when conveyed to a fruitful soil. 5. That the absence of a good scar is not proof positive that a child has not been vaccinated properly. 6. That Antier's assertion "that the fœtus in utero can be vaccinated through the mother," does not as a rule hold good. In nine out of twelve children on whom he made the experiment, vaccination succeeded perfectly when performed on the child itself a few weeks after birth. Nevertheless he advises that pregnant women be vaccinated during an epidemic of variola, the operation being of course repeated on the child after birth. 7. That during the development or just after the completion of the vaccine vesicle, an eruption of supernumerary pustules, in every respect resembling that produced by vaccination, may appear about the body; that these are analogous to the pustules produced by a second vaccination, and are advantageous to the individual. 8. That vaccination is a more efficient protection against variola than the actual virus of variola. He has often seen vaccination succeed in those deeply pitted with the small-pox. 9. That an individual, even though properly vaccinated, is not secure from variola on the fifth day of the development of the vesicle. He has seen a child attacked by variola confluens on this very day.

Of the Vaccine Lymph.—10. That to ensure perfect success in the operation, the lymph should be previously renewed or regenerated. 11. That the vaccine matter fresh from the cow has different degrees of activity in different years, affecting the constitution at one time more, at another less, just as variola is now confluent, now discrete. Thus, fresh matter used in 1836 produced a more powerful reaction—inflammatory fever, painful swelling of the axillary glands, &c., than that employed in 1844; so great was the difference, that the former virus, after being weakened by eight years' transmission from individual to individual, produced just the same effect in 1844 as the fresh matter taken from the cow in that year. Again, in 1853, he took a number of children, and vaccinated the left arm with lymph that had been transmitted continuously from 1836, the right arm with fresh vaccine matter of 1853; here the difference was most striking—the rapid progress, the large size of the vesicle, the wide areola, the intense inflammation and swelling of the axillary glands, and the large, deep, honeycomb scar on the right arm, contrasted remarkably with the feeble action and small superficial scar on the left. 12. That, in conclusion, fresh lymph is much more actively transmitted than old.

Of Re-vaccination.—13. That it is necessary, that it ought to be obligatory, and that it be performed every fifth or sixth year. 14. That the longer the interval that has elapsed from the time of the first vaccination, the greater the liability to contagion. Re-vaccination, performed during three epidemics of variola, succeeded in one-fourth of those between thirty and fifty years of age, but only in one-eighth of those between fifteen and thirty. 15. That the vesicle of a second vaccination is larger, more inflamed, and produces more reaction than that of the first. *The lancet* was devised to regulate exactly the depth of the incision, which should be made vertically, not horizontally. It consists of a somewhat flattened cylindrical sheath which conceals a lancet-blade; this blade is moved up and down within the sheath, and can be protruded at will, just as in the sliding pencil or penknife. Its lower end is connected, inside the sheath, with a screw turned by a button which projects from the bottom of the instrument, and which can fix the blade when its point is sufficiently protruded.

Preservation of Lymph.—The vesicle being punctured at the time above specified, the lymph is received into a fine capillary glass tube, the ends of which, as soon as it is filled, are sealed by being held in the flame of a candle. The capillary tubes, thus charged, are placed in a test-tube which is filled with oil, corked, and preserved in cold water. In this way lymph may be kept perfect for more than a year; he has found it quite good even after twenty-six months. Cold of 20° below zero C., freezes the lymph, and renders it inactive; cold of 12° below zero C., has no ill effect upon it. Heat of 50°–55° C., has the same hurtful effect as the extreme cold. Hence care will be necessary in preserving it in countries where the extremes of temperature are great.

Statistics, kept by his father from 1801 to 1835, show that in the small district of Mortagne, during the first seventeen years, 1570 were vaccinated, 298 had small-pox, of whom 113 died. During the last eighteen years, from 1818 to 1835, 4087 were vaccinated, 98 had variola, of whom 28 died. The difference between the year 1801 (in which 10 were vaccinated, 30 had variola, and 15 died) and the year 1835 (in which 452 were vaccinated, no cases of variola and no deaths occurred) is very striking. He continues these statistics from 1835 to 1851, during which time he saw only 22 cases of variola; of these 4, who had not been vaccinated, died.

CHAPTER VI. *On an Epidemic of Dysentery at Mortagne in* 1849, *and the Analogies of this Epidemic with the Cholera which prevailed at the same time in the Environs.*—The epidemic bursting over the little town in the intensely hot weather of July, continued its ravages till the beginning of November, in spite of a great change in the temperature in the month of September. During the later period of its stay, it seemed to confine itself more exclusively to those houses in which cases had already occurred, which, coupled with the fact that it persisted in spite of atmospheric changes, leads him to a belief in its contagion. Three different stages were remarked in the course of the disease—the first characterized by rigor, fever, and abdominal pain; the second by increase of pain, frequent stools with tenesmus, and scanty urine; the third by choleraic symptoms, or tendency to collapse.

Treatment.—Bloodletting, more often local than general, was first practised; an emetic of ipecacuanha was then administered; mild purgatives were occasionally employed; but he lays greatest stress on the value of opium, recommending the acetate of morphine where pain is the prominent symptom, and some preparation of opium when the evacuations are very abundant. No mention is made of mercury. Wine was freely administered in the severer cases. In the third or choleraic stage, when the stools became very frequent, and there was tendency to collapse, he found combinations of rhatany with opium most advantageous. He remarks on the fact that cholera, which prevailed all round, spared the town at this time, and questions how far it was excluded by dysentery. Lastly, he states that the aphthæ appearing in the mouth, and hiccough, are not necessarily signs of approaching death, as is supposed, for these symptoms appeared in many of his patients who recovered. Of 104 cases he only lost 5.

CHAPTER VII. *On an Epidemic of Croup that prevailed at Mortagne from April* 10*th till November* 28*th*, 1858.—The epidemic (apparently one of diphtheria rather than genuine croup) broke out in the month of April, which was unusually hot and dry, although March had been cold and windy, and the winter very severe. In general, the false membrane starting from the tonsils and pharynx showed a great tendency to spread to the larynx and trachea, but in a few cases was limited to the larynx and trachea. He made fair trial of caustics, but condemns their use. Leeches, followed by emetics and purgatives, with inhalation of soothing vapours by means of his apparatus, seem to have succeeded best. Of 188 patients, 119 were girls, of whom 21 died; 69 were boys, of whom 9 died. No trial was made of tracheotomy.

CHAPTER VIII. *Report on the Epidemics of Cholera and Sweating Sickness* (*Suette*) *which prevailed at Champignol, Arconville, and Bergère, during the Summer and Autumn of* 1854.—In comparing the epidemic of 1832 with that of 1854, he notices the much greater severity of the cramps, cardialgia, and collapse, in the former. He concludes that cholera is contagious, though not virulently so; also that it may attack the same individual twice. Of the two epidemics which he witnessed, the first was characterized by rapid collapse, and was treated most successfully by morphine administered every half hour or hour, the dose being increased according to the exigencies of the case, and by hot applications to the body. He was in the habit of applying repeated sinapisms to all parts of the body; the cramps were best relieved by friction. In the second epidemic there was less tendency to collapse, but violent reaction often set in, with head symptoms. In some cases typhoïd fever appeared as a complication, in others dysentery. The morphine proved less successful in this than in the previous epidemic. In the first epidemic he treated 17 cases of genuine cholera, 55 of cholerine, 109 of sweating sickness, and only lost 4 patients. In the second he treated 48 cases of cholera, 68 of cholerine, 20 of sweating sickness, and lost 23. The sweating sickness bore some resemblance to the cholera, being attended in many cases with colics, cramps, vomiting, and diarrhœa, but always terminated with profuse sweating, and was followed by great debility, so that convalescence was established even more slowly than in cholera. It raged at the same time as the cholera, and in the same houses, though those who had the one seemed to be protected against the other. It yielded best to tannate of quinine in full doses.

CHAPTER IX. *Observations on the Therapeutical Effects of Tannate of Quinine.*—That being antiperiodic, tonic, and astringent, it is not only as serviceable as the sulphate in intermittent fevers and periodic neuralgia, but that it also acts most beneficially in malignant forms of dysentery and in complicated cases of typhoïd fever, as well as in rheumatic fever.

CHAPTER X. *An Apparatus for the Inhalation of Aqueous Vapours.*—This resembles in most particulars the common inhalers in use, but has these special advantages—that the mouthpiece is in form of a large mask, which fits the face accurately, and receiving the vapour by the pipe of the apparatus, is furnished with a valvular opening through which the expired air and vapour pass—that the vapour can be forced at will through the conducting pipe by letting air into the apparatus. A stopcock that fits accurately the opening by which the water is poured into the apparatus, is hollow, and has at its side a hole, through which the air passes into the vessel when the cock is turned round in such a way that its hole corresponds exactly to a hole in the socket of the apparatus into which it fits. There is thus no need of forcible inspiration, as the vapour is presented to the mouth and nose in the mask, and can be easily inhaled.

CHAPTER XI. *On the Advantage of establishing Rooms in which the Dead can be placed before Interment.*—Some place of the kind should be built in all capitals, to prevent the possibility of being *buried alive.* Well-authenticated cases of such an accident are not wanting. He relates two cases with a feeling that would do credit to Edgar Poe. The first was that of a certain Mdlle. du Tréan, born of a noble house, from whose tomb the sacristan heard groans and sighs issuing on the morning after her burial. ("*Nous tenons cela des gens dignes de foi.*") The second, still more horrible, concerned the famous Abbé Prévot, who, while meditating in the forest of Chantilly, fell down suddenly in a fit of apoplexy; he was carried home, and the doctor was summoned; but the tragedy did not end here, for this gentleman, who seems to have been a more enthusiastic pathologist than judicious practitioner, proceeded at once to the autopsy—"*regrettable légèreté, ignorance coupable!*" The incision from the chin to the pubes is made, when, *horribile dictu*, "*le malheureux tressaille, il pousse un cri.*"

The largest of these "*salles d'attente mortuaires*" that we have seen is in Vienna. The dead are laid in this for twenty-four hours before interment, and round the wrist of each is fastened a cord that communicates with a bell over the bed. A porter sits day and night in an adjoining room. On asking him whether he had ever been called up, we received the answer: "Never—not even during the time of the cholera."

CHAPTER XII. *Question of Fees to which Practitioners are entitled.*—He proposes that the number of practitioners in each district be proportioned to the number of the inhabitants, and that a fixed income be assured to them; the money to be raised by means of a new tax, called the health-tax (*l'impôt de la santé*). That the inhabitants of every district and town be divided into four classes: poor people, who are exempt from the tax; people of moderate income, who shall pay one franc; people of good means, who shall pay two francs; rich people, who shall pay three francs. He draws a blissful picture of the country under such a system, when there shall be an end to actions for recovery, and to all bickering among practitioners.

CHAPTER XIII.—A case is related of a French soldier who passed a piece of pencil into the urethra to relieve spasmodic stricture, from which he was suffering after an attack of gonorrhœa. The pencil slipped into the bladder, and after causing several months of misery, was at length expelled in the following way:—He was ordered to retain his urine as long as possible, and then, leaning forward with his arms resting on a table and his legs separated, to strain with all his force and expel his urine. The pencil was in this way forced through the urethra and fell out, to the great satisfaction of the sufferer.

Review VII.

Les Médecins au Temps de Molière—Mœurs, Institutions, Doctrines. Par Maurice Raynaud, Docteur en Médecine, Docteur ès Lettres. —*Paris*, 1862. 8vo.

The Physicians of the Time of Molière; their Manners, Institutions, Doctrines, &c. By Dr. Maurice Raynaud.

They say in France that there are certain people who affect to execrate the medical profession, in order to make believe that they have read Molière.

It has certainly been a generally accepted belief that the author of 'Le Malade imaginaire' and 'L'Amour Médecin' was at bitter feud with the Paris Faculty of Medicine, and that his sharp sallies of wit against the absurdities once common, but now rare, among professors of the art of healing, sprang from motives of personal hostility to his medical contemporaries. Better knowledge of the time and circumstances has long convinced all who have examined the matter closely, that such was not the case, and we deem the publication of M. Raynaud's interesting book a favourable opportunity of ventilating a question of some interest in medical literature.

It must be taken for granted that Molière's vocation was that of a satirist. Son of an upholsterer, and valet to a king who could not but regard with favour so witty and pleasant a servant, the dramatist, while conscious of the inferiority of his position and the superiority of his genius, possessed unusual opportunities of observing men as they are in the artificial region of a court. The king was not above enjoying the fun of seeing his courtiers and nobles pelted with Molière's ridicule, and even condescended occasionally to suggest to him new victims. This was notoriously the case with the Marquis de Soyecourt, for the display of whose oddity and passion for the chase, Molière, at his Majesty's desire, added a scene to 'Les Fâcheux.' Unable, at the very short notice he had received from the king, to get up all the hunting terms necessary for the new scene, the unabashed wit adroitly extracted them from the Marquis himself, who was Grand Veneur.

Called upon to provide amusement for his Royal master, Molière composed the greater number of his plays to set off the splendid fêtes which Louis delighted in giving at Versailles, Fontainbleau, and elsewhere. The introduction of a character well known to all the audience gave piquancy to these plays. Authors cannot take liberties of this kind without incurring peril, and Molière, notwithstanding the august protection he enjoyed, occasionally suffered for his impudence. The Duc de Feuillade, being generally pointed at as the original of Molière's "Marquis de Tarte à la Crême," avenged himself in a sorry way. Meeting the poet in one of the royal apartments, he saluted him amicably; as Molière bowed in return, the Duke seized him by the head, and violently rubbed his fair effeminate face against the metal buttons of the ducal coat, exclaiming, "Tarte à la crême, Molière! Tarte à la crême!"

A writer who did not fear to incense dukes and marquises; who braved the wrath of the theological world by his 'Tartuffe;' who incurred the anger of the women by 'Les Précieuses ridicules,' 'L'Ecole des Femmes,' and 'Les Femmes savantes,' was not the man to pause in the pursuit of medical men whenever they offered him fair game. Absurdities certainly were not wanting in the profession. Dogmatism in medical science was rampant. The scholastic glosses upon Hippocrates and Galen had accumulated into a vast mass of abstractions, which still held sway among the faculty. The four elements—earth, air, fire, and water; the dry, the damp, the hot, and the cold; the four humours, the nine temperaments—were manipulated in syllogisms with a vain, profitless subtlety that resulted in mere word-puzzles, more like a complicated algebraical problem than a serious inquiry into matters of life and death.

Molière was no indifferent observer of this state of things. For the greater part of his life he was a martyr to disease. The striking portrait of him hung in the gallery at Dulwich, is expressive of physical pain. He was subject to a convulsive cough, or hiccough, from which nothing but the strictest diet could keep him free. Being a self-indulgent man (a characteristic also expressed in the portrait), and living in the midst of a luxurious Court, the restraint imposed upon his appetite is said to have irritated him against the art which was called curative yet failed to cure him. Higher reasons, doubtless, co-operated in urging him to the onslaught he made on the prevailing system. His keen intellect, which, be it remembered, had been exercised under the tutorship of the learned Gassendi, fully sympathized with men who—like Guy Patin, for instance—led the reaction which had already commenced against scholastic pedantry. The force and perennial freshness of his wit arises in no small measure from the profound insight into the nature and fitness of things which they exhibit. This truism, which applies to all writers of real genius, it is needful to keep before us distinctly when we hear Molière described as a mere writer of farces. M. Raynaud has given a very able, learned, and dispassionate chapter on the medical doctrines of the period which his book embraces. He points out specifically what Gassendi did to break through the spider's web of pedantry with which the schools had obscured the really great merits of the physicians of antiquity; and he then proceeds to indicate how, and in what plays, Molière reflects the teaching of his master in philosophy. As sincere in his hatred of dead formulas, shams, pedantry, and all that belongs to the Tartuffes of science, as any man in our own day, Molière contributed in no inconsiderable degree to the demolition of those *idola* which stood in the way of the new philosophy. His earliest pieces, performed while he was still roaming the provinces—pieces of which little more than the titles remain to us—show that his mind was then directed to the abuses of science falsely so called.

In the 'Three Rival Doctors,' the 'Schoolmaster,' and the 'Doctor in Love,' the same peculiarities of contradiction and inconsistency prevailed which were afterwards elaborated in the character of Metaphrastes in the 'Dépit amoureux,' of Caritides in the 'Facheux,' of Pancrace in the 'Mariage forcé,' and of Vadius in the 'Femmes savantes.' The one, an outrageous talker, proves in an harangue half an hour long, that he knows how to hold his tongue; another worries his interlocutor to death by a discussion on the shape of a hat; all of them are infatuated with their own accumulation of unprofitable knowledge, classified, divided, and subdivided, and are able on every question to quote the opinion of the ancients, whether in Latin, Greek, or Hebrew, yet they show themselves absolutely ignorant of the practical point upon which their opinion is asked.

In dealing with the two vices of the then Faculty in France, pedantry and charlatanism, Molière seems to recognise the honesty that may accompany the former. Thus, in 'Le Malade imaginaire,' Beralde says to the infatuated Argan—

> "Your M. Purgon, for example, has no pretence about him; he is a doctor all over, from the crown of his head to the sole of his foot; a man that believes in his rules more than in all the demonstrations of mathematics, and who thinks a wish to examine them criminal. He sees nothing obscure, doubtful, or difficult in medicine, but with an impetuosity of prejudice, a rigidity of self-confidence, and a brutality of common sense and reason, prescribes his purges and his bleedings right and left without one moment's hesitation. You must not take it ill whatever he may do to you, for he will despatch you with the best intentions in the world, and in killing you he will only do what he has done for his wife and children, and what, if need were, he would do for himself."

Though Purgon is generally taken for the famous and able Dr. Fagon, the last stinging sentence of our quotation reflects an accusation which Guy Patin makes against Guenaut, physician to the king, of having put to death his wife, daughter, nephew, and two sons-in-law with his favourite remedy antimony. The amenities of social intercourse were not much observed in the terrible war between antimony and phlebotomy, by which the profession in the seventeenth century was riven asunder.

The letters of Guy Patin contain passages aimed at his brethren, compared with which the language of Molière is politeness itself. He seems to have been enchanted at the success of the latter, and on hearing of the performances in which the Court physicians were ridiculed, he writes—"Thus people laugh at those who kill mankind with impunity." We remark that Guy Patin only heard of the play.

Etiquette did not then permit grave persons, such as physicians, judges, bishops, to indulge in so frivolous an entertainment as a comedy.

As a pendant to the dramatized pedant Purgon, we may here refer to a good example of Molière's treatment of the species quack in the consultation scene of 'Monsieur de Pourceaugnac,' where the unfortunate hero of the play, being in perfect health, is entrapped into the hands of two practitioners, between whom, once safely seated, he cannot escape until he has heard an exposition of his melancholy condition enough to drive him mad.

In September, 1665, appeared that play which has been called the dramatists' declaration of war against the faculty, 'L'Amour Médecin.' The plot of this comedy is extremely simple. Sganarelle, Molière's type of the bourgeois, is a widower with an only daughter, Lucinda, whom he is very desirous to keep at home, and unmarried, for the sake of his own comfort, not only that he may be spared the cost of a housekeeper, but also that he may escape the necessity of providing his daughter with a dowry. Lucinda has contrived to fall in love with Clitander, and by pining after him she is reduced to a very low condition of melancholy. The father seeks his daughter's confidence, but when he finds himself on the point of discovering a love-affair he craftily flies off at a tangent, declares he can get nothing out of the girl, and that she is an obstinate child, upon whom reasoning is thrown away. Lucinda in vain tries to force her confession upon him; he will not hear her, and spite of the loud reiteration in his ears of the word "husband," he persists in saying she is a bad girl who will not say what is the matter with her. The bold partisan who shouts to him that "'Tis a husband she wants," is Lisette, the servant, who resolves to obtain by intrigue what has not been ceded to persuasion. She easily contrives to alarm Sganarelle by announcing the dangerous illness of his daughter. The most eminent physicians are sent for post haste. Lisette is very sharp in her remarks upon those learned gentlemen, who, as if by way of contrast to the solemnity which kept them from visiting the theatre, are introduced upon the stage dancing and making their reverences to Sganarelle. They are four in number, and after a conversation with the master of the house and Lisette, being left to themselves they sit down, and with a cough all round, commence a consultation; or rather they seem to do so, for their talk is upon the mules they ride, the ground they have covered, and the disputes raging among the profession. Sganarelle comes back and presses for a decision. All four physicians begin to reply at once, and when the confusion of tongues has subsided, one is heard to assert that the patient must be bled immediately, the advantage of which is flatly denied by another, who recommends an emetic. A brief and hot dispute ensues between the two physicians, who are both extremely personal in their allusions to the deaths of former patients by bleeding and by antimony respectively. They each leave Sganarelle with the solemn assurance that the treatment prescribed by his rival will be the death of the girl. The perplexed father thereupon refers to the remaining two doctors, who bewilder him still more by their pedantic talk. This portion of the play was deemed by the faculty of that day the most offensive, being, we fear, for the time, at any rate, very true to nature. Sganarelle is about to try the effect of a quack medicine upon his daughter, when Clitander is introduced by Lisette as a doctor who cures by talismans. The father is hoodwinked, and the lovers adopt, in his presence and with his consent, the talismanic formula of marriage, which turns out to be marriage in sober earnest. So ends the play.

Much of the point and severity of this satirical comedy lay in the counterfeiting of living men, whose peculiarities were closely imitated by the actors, even to the drawl of one and the stutter of another. Guy Patin says that masks indicative of the features of the ridiculed physicians were worn by the actors, but this could hardly have been necessary, and would have been needlessly offensive. As Guy Patin only wrote from hearsay, his authority for the statement is by no means conclusive. Molière was assisted by Boileau in inventing Greek names for his victims, who, be it remembered, though they were attached to the court, were not necessarily the heads of the profession.

It has generally been supposed that the originals of the four doctors here put on the stage were MM. Daquin, Desfougerais, Guenaut, and Esprit. M. Raynaud gives very good reasons for substituting the names of Valot and Brayer for Daquin and Esprit, and thus brings together the very four men who were in attendance upon Cardinal Mazarin in his last moments (1661). An unseemly altercation took place on the occasion, according to Guy Patin, who writes—

"Yesterday, at two o'clock, in the Bois de Vincennes, four doctors—namely, Guenaut, Valot, Brayer, and Beda (Desfougerais), could not agree as to the kind of disease the patient died of. Brayer said the spleen was ruined, Guenaut said it was the liver, Valot maintained it was the lungs, and that there was water on the chest, Desfougerais affirmed there was mesenteric disease. . . . There are four clever men!"

Valot, whom M. Raynaud assumes to be the original of Tomès (the incisor), succeeded Vautier as first Court physician, in 1652, on paying 30,000 crowns to the Cardinal. Louis XIV., who was very methodical, made him keep a "journal of the king's health,"* which was continued by Daquin and Fagon. Who can say whether a page of that professional diary may not often explain the turns of policy pursued by the monarch, and reveal the momentous importance of a fit of indigestion in a king? Nothing was hid from the medical attendants, upon whom Louis relied implicitly. He liked to laugh at them, but he would not for the world have been without them. It was but a pleasant bit of malice that prompted him to say, "Surely we may laugh at the doctors a little, for they make us suffer enough."

A great cry was raised when Valot, not then of the Court, was called in to advise about the illness of the king, who in 1647 was seized with the small-pox. Valot recommended bleeding, to the

* This curious manuscript was published last year by M. Le Roi as a sequel to Dangeau's Mémoires.

horror of his colleagues, but the king recovered. For Anne of Austria, the queen-mother and his early patroness, who died of cancer in the breast, Valot could do little, and he incurred the displeasure of his jealous contemporaries in consequence. He was already long past work when war with the Low Countries broke out, and being resolved to follow the king into Flanders, he succumbed to the fatigues of the campaign and died on the road.

Daquin, the other pretender to the character of Tomès, and who succeeded Valot, was a converted Jew. A man full of tact and adroitness, he endeavoured to make the best of his position by asking for favours on every opportunity, until the king, wearied, it is said, of his pertinacity, suddenly dismissed him, in 1693, for having begged the archbishopric of Tours for his son. A more probable reason of his disgrace was the fall of Madame de Montespan, who had been his great patroness. The ready shrewdness of the man is illustrated by his retort upon the king, when Louis, referring once to an officer whose death was announced, fixed his eyes on Daquin, and said, "That was an old and faithful servant of mine: he had one quality very rare in courtiers—he never asked me for anything." Daquin understood the allusion, and without the slightest discomposure replied: "Dare one ask, sire, what your majesty gave him?" Louis had nothing to say, for the faithful old servant had never received a favour.

Daquin's partiality for the lancet was sufficiently notorious to justify the application to him of the name Tomès, in allusion to his incisive propensities. Another characteristic formalism would apply doubtless to either Valot or Daquin, as when Tomès says that "a man who dies is but a dead man, and a matter of no consequence; but a formality neglected brings a notable damage to the whole body of physicians;" and he relates that, being called to a consultation, "three of *nous autres*, with a country doctor, I stopped the business. The people of the house did what they could, and the disease was progressing rapidly; but I would not give way, and the patient died bravely during our dispute." The "dying bravely" is a touch of genuine humour.

M. Desfougerais, another court physician, appears on the stage as Desfonandrès the manslayer. He was the great partisan of antimony, with which, writes Guy Patin, "he kills more patients than three true men save by the ordinary remedies." His real name was Elié Beda, to which he tacked the designation of nobility when he had attained to wealth and honours. When Molière's play first appeared, Desfougerais must have been about seventy years old. He was distinguished for his courtly manners, which, together with his antimony powders and hatred of bloodletting, procured him a large practice. He had the misfortune to limp, a peculiarity which it is surmised would throw his part in the play upon the actor Béjart, who had a similar infirmity. Desfougerais was reckoned a charlatan by the Faculty, and is styled by Guy Patin, "venerable and detestable quack. . . . But," he adds, after a page of abuse, "he was a worthy man, who never changed his religion but to make his fortune and advance his children." This he said in allusion to Desfougerais' conversion, in 1648, from the Protestant to the Romish faith.

The great champion of antimony, however, and the most celebrated French doctor of the time, was Guenaut, represented by Molière as Macroton, or the drawler, in allusion to his manner of speaking, which was very slow and solemn. If he was, as Guy Patin says, in person like a monkey, and a great dandy, he certainly offered a great temptation to the satirist, while at the same time his high position and great influence made an attack upon him dangerous. To him was addressed the rustic compliment of the carter who stopped the way to the Bois de Vincennes on occasion of the consultation already mentioned: "Let Sir Doctor pass; he will do us the favour to kill the cardinal." He appears to have been also the first physician in Paris that rode to his visits on a horse rather than on a mule. The circumstance which advanced his reputation more than any other was the cure of the young king in 1658. Louis had fallen seriously ill at Mardyck, whence he was transported to Calais. Doctors were summoned from all parts, Guenaut from Paris among the number. A council was held, at which Cardinal Mazarin presided; and as bleeding and other remedies had failed, a dose of antimony was resolved upon. The king recovered, and Guenaut gained great glory. The detractors of antimony were finally overcome, and the Faculty itself a few years later rescinded the decrees which they had fulminated against the hated remedy.

The history of the professional contest which was thus brought to a close forms an interesting chapter in M. Raynaud's book. Guy Patin was very bitter in his denunciations of the emetic, and hated Guenaut intensely as the great partisan of that specific. Patin's love for bleeding was excessive. He practised it upon old men of eighty (eleven times in six days on one occasion), and on children as young as three days. A child of seven he bled thirteen times in a fortnight. He was bled himself seven times for a simple cold, and a medical friend of his underwent the operation sixty-four times for rheumatism. Another member of the profession (M. Labrosse), who died refusing to be bled, is thus gibbeted in Patin's correspondence: "He did us the honour to call us sanguinary pedants, and said he would rather die than be bled. Die he did; the devil will bleed him in the next world, as a rascal and an atheist deserves." So much for not dying according to form, as Molière says.

Bahis is the stage name for M. Esprit, the fourth physician, whose stuttering speech is alluded to in the epithet. He it is who informs Sganarelle that "it is more worthy to die according to the rules than to recover in contradiction to them." M. Brayer, as we have said, is also put forth as the original of Bahis. There is a fifth medical personage, named Filerin, in the comedy, who seriously takes to task the two physicians who had quarrelled, and who is supposed to represent the entire Faculty, and to speak the general opinion in reproaching the offenders for their imprudence in letting the outer world know of their professional differences.

"For centuries," he says, "the world has been infatuated about us. Let us not disabuse the public mind. We are not the only ones who live by the weakness of mankind. There are flatterers, alchemists, and astrologers, who profit by the vanity and ambition of credulous minds. But the greatest weakness men have is their love of life, and that is the source of our gains."

'L'Amour médecin' is perhaps more tainted with personalities than any other of Molière's plays. It is that peculiarity indeed, which we must confess makes it an object of interest to us as offering much material for the history of medicine at that period. It must not, however, be forgotten that among French comedians, properly so called, Molière was almost the first in time as well as first in genius. His precursors were the mountebanks of the fairs and festival days, who employed the histrionic art as a means of vending quack medicines. In the case of these men, no fun was so telling or so appropriate to their purpose as vilification and ill-usage heaped upon the legitimate practitioners of medicine. The dramatist, we may easily imagine, often loitered through scenes of rustic or civic merriment, enjoying with a peculiar zest the broad farcical humour displayed in the booths of the charlatan. And as our own Shakespeare caught many a fruitful idea from the rude representations of his predecessors, and wrought the vulgar material into the noblest poetry, so Molière refined the clownish buffoonery of the quack into refined wit and sarcasm. Thus in bringing doctors so frequently on to his stage he seems but to have followed up the traditions of his dramatic progenitors.

'Monsieur de Pourceaugnac' would seem to be a reminiscence of some scene witnessed at a booth. True it is a satire of higher quality than the common quack's, but its raillery is directed merely against the external absurdities of the profession—the wigs, the Latin, the pompous harangues. 'L'Amour médecin' takes a wider range and strikes at nobler game. But highest of all Molière's pictures of man in relation to medicine is 'Le Malade imaginaire.' This play is not a mere caricature of members of the faculty, but a bitter protest of a dying man against the impotence of medical art.

The year 1673, when this his last comedy appeared, was the last year of Molière's life. The aneurysm which proved fatal to him had made a progress of which he could not be altogether unconscious. In spite of his ardent desire to live he could obtain no cure. Not long before, a literary enemy, Le Boullanger de Chabussay, who knew a good deal of Molière's life, had published a dramatic piece teeming with outrageous personalities, and entitled 'Elomire the Hypochondriac,' in which the poet's name is thinly disguised by an anagram. That the gay, witty, satirical Molière should be hypochondriacal would seem palpably absurd. Yet M. Raynaud points to a passage in Grimarest's eulogistic life of Molière, which would seem to admit that the accusation was just. It is to this effect: "Ten months after his reconciliation with his wife, he gave, on the 10th February, 1673, 'Le Malade imaginaire,' of which he is said to have been the original." If we assume that Argan is the portrait of the author, with what a deep tragic interest is this celebrated comedy invested! A man to whom health was so important for the performance of his task in the royal revels found himself often taken off the boards by sickness, and his strength evidently failing him. He could understand the tortures of a mind like Argan's, longing for life and hating death, desperately clinging at every chance of cure, and blindly confident for a time in those who promised him health. In bitterness of heart, Molière lays bare his secret thoughts to the public gaze. The imaginary nature of his Argan's sickness takes away from the play the painful feeling of reality which would otherwise be felt in the scenes of the sick man's self-inflicted torture. Viewed as the expression of genuine terror, what can be more tragic than Argan's words when about to simulate death, in order to sound his avaricious wife's disposition. He is dominated by the most tyrannical of passions, the fear of death, on which, as Filerin would say, is based his dependence on the physicians. Starting suddenly from his reclining position, "Is there really no danger in counterfeiting death?" One would think that Molière had recognised the force of the insinuation contained in the 'Elomire,' and had resolved to laugh at himself in public. As in the other plays which allude to his domestic troubles, the laughter excited by 'Le Malade imaginaire' may well, to those who remember the author's life, be mingled with tears.

There is something almost awful in the contemplation of that sick man's room, heaped up with drugs, with the doctors moving round their subject like ghosts or like vampires watching their prey, and the greedy hypocrite of a wife counting the gains she is about to inherit. While we think that he who plays the dying man is himself struck with a mortal disease and cannot live long, the comic elements of the piece become grim and lugubrious in the extreme. Subsequent events gave a deeply tragic interest to this last work of the greatest master of the modern comic drama.

We must remind our readers of the plot of "Le Malade imaginaire." Sganarelle, the hypochondriac, has a daughter, Angelique, whom he proposes to give in marriage to Thomas Diafoirus, a medical student, son of a doctor, and nephew to the great practitioner Purgon. With one of the Faculty for a son-in-law, the inveterate patient looks forward to a paradise of bleeding, blistering, and purging. Angelique, however, has given her heart to Cleante, and is most decidedly averse from any match with the pedantic Thomas. Meanwhile Beline, her stepmother, whose hopes lie in the speedy death of the sick man and the possession of his fortune, discourages the notion of marriage, and coaxes her husband into the belief that his daughter ought to be placed in a convent. The intrigue of the play is carried forward by Toinette, the servant, who, when Purgon angrily quits his patient for delaying to obey his orders, assumes the garb of a physician, and pretends to the most extraordinary healing powers. She re-appears in her native character of servant so promptly as to disabuse her master of the suspicion, engendered by the resemblance, that she and the doctor are one. Beralde, the brother of Sganarelle, has meanwhile been reasoning with the latter against his fancy for medicines, and his blind confidence in

Beline, whom he knows to be selfish and cruel. Toinette pretends to uphold her mistress and to justify her master's fond delusions. "To convince him," she continues, "that she is the most virtuous of women, do you, master, fall back in your chair and pretend to be dead, when the mistress comes in M. Beralde will hear her grievous lamentations." Precisely what Beralde and Toinette anticipate takes place. Beline, on learning the joyful news of her husband's death, expresses herself with so much cruel candour as to delight the partisans of Angelique and her lover, and to rouse in Sganarelle no small degree of wrath against his wife. A similar test applied to the daughter draws forth marks of sincere affection, by which the father is moved to consent to her marrying Cleante, on condition that he becomes a doctor. Beralde catches at this idea to persuade Sganarelle himself to become one of the Faculty. This gives the opportunity for introducing the celebrated burlesque of the ceremony of installation, which Molière composed in Macaronic Latin, of an absurdly comical description.

> "Chirurgiani et Apothicari
> Atque totia compania aussi,
> Salus honor et argentum
> Atque bonum appetitum,"

is part of the President's opening address, and a fair specimen of the Latinity of the whole. Three times the baccalaureus Argan makes oath that he will keep the statutes of the Faculty, that he will keep to the old rules, and that he will employ the ancient remedies—

> "Maladus dut il crevare
> Et mori de suo malo."

The first draught of this burlesque was sketched after a merry supper at Madame de la Sablière's, where were assembled the wits of the day, including Molière, Boileau, Lafontaine, and the celebrated Ninon L'Enclos. Molière drew the outline, and the company, including probably a few *esprits forts* of the Faculty, threw in the details, making in all a much longer and more diffuse composition than the one handed down to us. M. Raynaud gives in his first chapter some curious particulars of the original ceremony and of the burlesque, of the type and of the anti-type.

The first representation of the play took place, as we have said, on the 10th of February, 1673, and its many sinister presentiments and passionate denunciations were but shadows of the approaching fate of the author. Molière played Argan. The latter, when urged by Beralde to disabuse himself of his infatuation for doctors by going to see one of Molière's comedies, bursts out into a bitter invective against the profane author. On the day of the fourth performance Molière was more unwell than usual, and his friends entreated him not to go on the stage. "What would you have me do?" he replied, with characteristic generosity; "fifty workpeople depend on my playing for their day's bread. I should never forgive myself if I stayed away." The play proceeded. He uttered the cruel words against himself which we extract :

"*Argan.* That's a pretty impudent fellow, your Molière, with his comedies; and I find it extremely funny to go and play honest men like the doctors!

"*Beralde.* It is not the physicians that he plays, but the absurdities of physic.

"*Argan.* He is a nice fellow to meddle with the management of physic! He is a blackguard and an impudent fellow to make fun of consultations and prescriptions, to attack medical corporations, and to go and put on his stage venerable persons like those gentlemen.

"*Beralde.* Whom but men of divers professions would you have him put there? Princes and kings, who are of as good a family as the doctors, are put on the stage every day.

"*Argan. Par la mort non de diable!* If I were the doctors I would be revenged for his impertinence; and when he falls ill, I would let him die unsuccoured. He might do as he liked and talk as he liked then; I would not prescribe him the slightest little bleeding, the smallest little purge; and I would say to him, Burst, burst; that will teach thee how to play next time with the Faculty.

"*Beralde.* You are very angry with him.

"*Argan.* Ay, he is a foolish fellow; and if the doctors are wise, they will do what I say.

"*Beralde.* He will be wiser than your doctors, for he will seek none of their help.

"*Argan.* So much the worse for him if he does not have recourse to their remedies.

"*Beralde.* He has his reasons for not wishing to take them, and maintains that only hearty, robust people are strong enough to support the disease and the remedies too; for his own part, he has only just force to bear the malady alone."

This grim pleasantry, which occurs in the third scene of Act iii., assumes a ghastly hue when read by the light of what occurred after the conclusion of the act and during the performance of the ceremonial ballet. As Molière, in the character of the Bachelor of Medicine, was taking the oath and pronouncing the word *juro*, he was seized with a fit of coughing. He endeavoured by a forced laugh to conceal the violence of the convulsion from the spectators, and remained in the theatre to the end of the representation. He was then carried to his house in the Rue Richelieu, where he expired soon afterwards, on the 17th February, 1673, suffocated in an attack of pulmonary hæmorrhage. He had literally fulfilled his gloomy prediction, and died without medical assistance.

Viewing him as a champion fighting against the pedantry and obstructiveness of the ancient Faculty, he merits all the glory of having died in the breach. His works undoubtedly exercised an influence which proved beneficial to medical science, in helping her to cast away many of the impediments that hindered her onward course.

We have yet a few words to say with regard to two of Molière's contemporaries who are brought prominently forward by M. Raynaud. We allude to the two physicians Fagon and Mauvillain. The former has been pointed at as the original of Purgon in 'Le Malade imaginaire'—an imputation which we think, with M. Raynaud, is not well-founded. When the play appeared, he was not a man of mark enough to be worth the dramatist's public raillery. His reputation was at its height in the second half of Louis's reign. He was nephew to the celebrated Gay de la Brosse, the founder of the King's Garden; was admitted of the Faculty of Paris in 1664, and appointed Professor of Botany there by Valot. He spent some years in travelling and collecting specimens, and ever after, even when he became first court physician, he gave special attention to the botanical garden of which he was justly regarded as the second founder. When at the summit of his profession he enjoyed almost universal esteem, as much from the gentleness of his manners as from the extent of his learning. His disinterestedness was equal to his modesty, for he would take no money from his patients, and abolished the perquisites that accrued to his office on the appointment to medical professorships in the universities. He owed his rise at court to Madame de Maintenon, with whom he became acquainted during a journey she made to Spain in charge of one of the king's natural sons. Genuine love of talent drew the two together. While Daquin held first place, Fagon was but a humble subordinate; but Madame de Maintenon lost no opportunity of advancing her friend, to the prejudice of the nominee of Madame de Montespan. One evening the king being at Marly had an attack of fever, and was attended by his physicians. About midnight, Daquin perceiving the symptoms favourable, retired, saying he would go to bed. Fagon seeming to follow him, stopped short in the ante-room, and settled for the night in an arm-chair, which, owing to an asthma he suffered from, was his ordinary kind of bed. An hour later the king complained to his valet that the fever was no better. "Sire," was the reply, "M. Daquin has gone to bed, but M. Fagon is there—shall I call him in?" "What will he tell me?" said the king, who dreaded Daquin's hearing of this breach of etiquette. "Sire, he will perhaps tell you something to console you." Fagon entered, felt the royal pulse, administered a warm drink, had his majesty turned over on the other side, and for the first time in his life found himself alone with the king, who did not resist long the charms of his superior understanding and fascinating conversation. Three months after this incident Daquin was dismissed, and Fagon appointed to succeed him.

In reading Molière, a question often recurs as to the source from which he drew so copious a medical vocabulary, and other secrets of the mystery of healing. This problem is resolved by M. Raynaud on the assumption that his medical friends gave him assistance. With three physicians at least he was on terms of intimacy—Bernier, a fellow-student under Gassendi, Liénard, an extravagant Cartesian, who wished to adapt the physical principles of Descartes to the entire system of medicine, and Mauvillain, whose reputation is due to the great fame of his friend.

The introductory pages of 'Tartufe' contain the following lines addressed to the king:—

"SIRE,—A very honest doctor, whose patient I have the honour to be, promises and wishes to undertake before a notary that he will make me live thirty years longer, if I can procure him a favour from your Majesty. As to his promise, I told him I did not ask for so much, and should be satisfied if he would undertake not to kill me. The favour, Sire, is the canonry of your royal chapel of Vincennes, now vacant. Dare I ask this favour of your Majesty the very day of the resurrection of 'Tartufe,' revived by your goodness? By the first favour I am reconciled to the devotees, and by the second I should make my peace with the doctors. For me these are doubtless too many favours at once, but perhaps not too many for your Majesty, and I await with respectful hope the reply to my petition."

The canonry was given, and it is not a little singular in connexion with our present subject that the only royal favour asked by Molière of which there is any record, was on behalf of the son of a medical man. "You have a doctor," said the king one day to Molière; "what does he do for you?" "Sire," replied the wit, "we talk together; he prescribes remedies; I do not take them, and I get well."

Mauvillain enjoyed a fair reputation among the faculty for ability, learning, and engaging manners. Once he incurred considerable professional disgrace with other antimonists by signing certificates favourable to a quack medicine, which crime of *lèse-faculté*, when proved, led to the expulsion of the offending doctors, who possibly had seen in the seller of powders a persecuted chemist. After being purged by a public humiliation and apology, Mauvillain and his friends were restored to the faculty, "but the blot was not wiped away," says Guy Patin.

In conclusion, we heartily recommend M. Raynaud's book, which has already reached a second edition, to the lovers of medical literature, and accept it at the same time as a valuable addition to the literature concerning Molière. A history of the life of that distinguished dramatist is still a desideratum. The late Mr. Prescott, we are informed, had gathered materials for such a work, when his attention was diverted to Spanish subjects by the great success of his 'Ferdinand and Isabella.' He handed over his small Molière library to Mr. Ticknor, of Boston, who, having accumulated further materials, purposed carrying out the views of his friend. Advancing age, however, and the unhappy condition of his country, have combined to check Mr. Ticknor in the prosecution of his design, and quite recently he has abandoned the project altogether, and has given his curious collection of books on the subject to the Boston Athenæum.

REVIEW VIII.

1. *A Manual of the Diseases of India.* By W. J. MOORE, L.R.C.P. Edin., in Medical Charge of the Sanitarium for European Troops on Mount Aboo.—*London*, 1861. pp. 220.

2. *Health in the Tropics; or, Sanitary Art applied to Europeans in India.* By the same.—*London*, 1862. pp. 318.

IT is no small proof of Dr. Moore's mental energy and professional zeal that these two works, both of which, even regarded simply as compilations, must have entailed considerable labour, should have followed one another in such rapid succession. It is still more creditable to him that one of them at least should have been written at an up-country station in India, amidst all the fatigues and worry which the superintendence of an important medical establishment involves, and under the unfavourable influences which a tropical climate, inaccessibility to necessary references, and the inertia of military life, must combine to throw in the way of such an undertaking. But if the operation of one of these causes has, perhaps, rendered the larger and more important of these works less elaborate as a compilation than its author would doubtless have made it had he had at his command those sources of information which are so available to the residents in European, and possibly also to those in some Eastern cities, it has, on the other hand, thrown him more upon the resources of his own personal observation for the illustration of the principles he lays down, and has given to his statements a force and individuality which they would not otherwise have possessed. If there were no other reasons than those to which we have alluded for mitigating the severity of critical censure, we should readily have admitted Dr. Moore's right to appeal to them in favour of a lenient judgment upon any shortcomings which he might have exhibited. But he may invoke still stronger ones. We look upon the man who has the energy as a subordinate to step out of the safe acquiescence in official routine, and the courage to draw attention to the frightful loss both of life and revenue which official blunders and negligence are constantly producing, as worthy of all our admiration and support. And such is very much the position which Dr. Moore is compelled to assume when discussing the hygienic conditions in which the home government has for years placed the British soldier in India. When we are told that the European army in India has hitherto *disappeared*, from disease (taking the average of the Presidencies), about every thirteen years and a half; that the mortality in hospital alone during the last few years has been at the annual rate of 62·45 per 1000; that the annual loss to the state on account of deceased or disabled soldiers alone—putting aside all considerations connected with the expense of the medical establishment, care of sick women and children, and pensions—equals *two millions, or more than one-fifteenth part of the whole revenue of India;* and that the greater portion of this loss is due to *preventible disease*, we think that we are not going very far in asserting that any man who makes an effort to stem this incredible stream of wanton waste of life and treasure is entitled to the lasting gratitude of his country.

If it were not for the beneficial changes which the intelligence of commanding officers here and there has already initiated, and which are gradually producing their effects in the increased comfort and improved health of the troops in the districts in which they have been introduced, one would almost despair of any immediate prospect of ameliorating the condition of the Anglo-Indian soldier. The curse of a jealous and restrictive monopoly, under which India has so long laboured, whose only care was the extraction from the country of the enormous taxation which it levied, and whose profits were so great that it could afford to blink the extravagant cost at which its empire was maintained, seems only to have been exchanged for a pernicious system of double government, in which the authorities at home and those in the country are so occupied by the desire to annoy and neutralize one another, that they have little or no time to devote to the true interests of that mighty empire, or to the conservation of that little handful of men by whom our dominion is maintained in it. Happily, however, there are men like Lord Stanley, Mr. Bright, and Sir Ranald Martin, who possess not only the will but the ability to make themselves heard in the cause of India; and there appears to be every prospect that their labours are tending gradually to introduce a better state of things than has hitherto prevailed in the management of that country. We must not, indeed, forget that India is at present in a transitional condition, and that the progress it has already made in some respects, in the short period that has elapsed since it was formally incorporated with the crown, has been gigantic. But especially do we look for its development, and for the economy of the expenditure by which it must be retained, to the infusion of new blood into all the branches of its executive which the competitive system of appointments is steadily producing. There is ample evidence that in the medical department alone the new system has been followed by the happiest results; not one of the least of which has been the elevation of the character of the medical officer, and the improved status which has accompanied it. It is true that owing to the bad faith and shortsightedness of the authorities a cloud has lately passed over the medical branch of the Indian army, but we have little doubt that it is only a temporary one, and that the claims of the Indian surgeon to fair and reasonable treatment will ere long be fully recognised.

If we must apologize to our readers for this apparent digression from the more immediate examination of Dr. Moore's works which we proposed to ourselves at our outset, it is upon the shoulders of Dr. Moore himself that we must lay the blame of our having so done. We are most unwilling that he should confound us with that class of reviewers who, he asserts, have by their writings so greatly "tended to delay the general progress of sanitary reform." And although he fortifies his charge by a quotation from a contemporary, which from the childishness and absurdity of its character goes some way—so far as an individual instance can do so—to justify it, we cannot but demur on the part of our brethren generally to so sweeping an accusation as being most undeserved. We believe that, as a whole, the "anonymous writers," as Dr. Moore designates them, of the periodical medical press are quite as willing to recognise the claims of sanitary medicine to the rank of a science as "independent authors" like himself are to prefer them. In this Journal we have certainly always upheld strongly its importance both as a "science" and an "art," and we shall ever receive with pleasure and notice with praise the efforts of those who endeavour to disseminate a knowledge of its principles, especially amongst the non-professional public.

It is more particularly in this direction that we think Dr. Moore's work on 'Health in the Tropics' is likely to be of use. Although creditably put together, and containing in a concise and well-digested form a great deal of information that is indispensable to every medical officer who undertakes the care of troops in the East, it treads on ground which has already been to some extent occupied by Sir Ranald Martin and others, whose works must be familiar to all who pass through the admirable course of the Army Medical School. Its chief merit is in its being so well adapted to serve as a handbook of reference for the use of military and naval officers, and other non-medical persons who may be located in India, and whose education has not familiarized them with those elementary scientific truths which lie at the bottom of the sanitary art, and with which every well-informed medical man becomes acquainted in acquiring the ordinary knowledge of his profession. As was very clearly laid down by the late Lord Herbert, the necessities of the military *régime* require that the commanding officer should be supreme in all things, even in matters which strictly belong to the province of the surgeon. It is at best, therefore, as an adviser, that the latter can exercise any influence in the determination of such points as the site of an encampment, the dietary of troops, the arrangements of stations, and numerous others, upon a due regard for which the health of the army must intimately depend. If the commanding officer is wise enough to appreciate the limits of his own knowledge, he will not only consult the medical officer on these subjects, but will also act upon his advice. But if, on the other hand, he is a self-sufficient martinet, he will probably consult no one but himself, and will certainly snub any medical officer who may be indiscreet enough to proffer his advice unasked.

We must, therefore, look eventually to the education of military officers in the general principles of sanitary science as the most effectual agency for arresting that wasteful expenditure of life which has hitherto characterized the history of our army in India. The importance of making an acquaintance with these subjects an indispensable portion of the education of non-medical officers is strongly insisted on by Dr. Moore, and he very properly points to the example of our neighbours across the Channel, who have introduced lectures on hygiène and sanitary matters into the curriculum of the pupils of the Military School of St. Cyr, as worthy of our imitation. In view of this requirement, we know of no work with which the military officer who is destined to serve either in India or in any other part of the tropics could more advantageously provide himself as a key to the numerous sanitary problems with which in his official capacity he will be called upon to deal than the treatise which Dr. Moore has produced.

When the future historian of British India shall undertake the recital of the successive stages of the struggle by which the empire wrested from the Great Mogul has gradually passed under the sovereignty of the British throne, there is one question which he must put to the records of the past before his story will be complete, and that is the cost at which this acquisition has been achieved? How that question will be answered, we have already incidentally indicated. That the bill which England will have paid for this eastern jewel in her crown will be gigantic beyond conception, no one can doubt, but who will venture to sum up the various items of which it is composed, or to set out the relation between the losses and gains which must enter into the account? We will not speak here of the moral and social sacrifices which our dominion in India has entailed upon us, but only of the mere material price at which our possession of it has been bought. Some idea of the cost which we are annually incurring may be formed from the statistics which Dr. Moore adduces in proof of the urgent necessity for improving the conditions under which our Anglo-Indian army is placed. The annual loss by disease in the ranks, taking the mean of the three Presidencies, is estimated at 65·66 per 1000, whilst that of the foot-guards at home (by no means a healthy regiment) is 20·4, and that of the population in the healthy districts of England is 7. If we add to this annual loss of men by death that which occurs from invaliding (29·4 per 1000), we get the enormous total of 95·06 men annually subtracted from each 1000 of our Indian army. The price of this annual loss, stated in money, is, as we have before mentioned, upwards of *two millions sterling.* But this does not by any means entirely express the whole of the loss experienced even in this department by Government. According to Neison and Macpherson, the annual mortality amongst the officers of the Bengal regiments is 24 per 1000, that of officers in the household troops at home being only 9·5 per 1000. Then we have to add the mortality amongst the soldiers' wives, which is on an average 35·4 per 1000 (compared with 11·9 per 1000 for the same persons in England), and their children, which for the whole of India may be stated at 64 per 1000 (compared with 22·3 as the average rate for twenty-four large English towns), and we shall be able to form some conception of the aggregate ravages which disease is annually making amongst our Indian soldiers and their dependents. But there is still another source of loss to the state in the expenditure which is constantly being incurred to improve stations which are naturally unhealthy, and which were originally selected for that purpose, often with a total disregard for all sanitary requirements.

We can hardly be surprised that political necessity should sometimes have dictated the choice of localities for stationing our troops, which from their obviously unhealthy position would never have been selected if there had been any option in the matter; but there is ground for the gravest reflection upon official negligence when we find that newly-constructed cantonments in districts where there is ample room for selecting better spots have been and are being built with as little regard for the soldier's health as in the days of our early occupation of India. Still worse is the fact that even many of the hill sanitaria, to which it must be remembered the invalid soldier is sent to recruit his shattered health, are hardly less pestilential than the ordinary stations on the plains. Thus, Dr. Mackay reports of Ootacamund, in the Neilgherry Hills, that—

"It is intersected by undrained swamps, and that the invalid may shoot snipe in some of them without leaving the compound of his house. In short, the whole station is a strange mixture of snipe-bogs, neglected compounds, and neat flower-gardens. Every convenient bush is made use of to deposit filth under of every description."

And to crown this description of a haunt worthy of Cloacina herself, he adds: "It is argued that the station has been healthy, that such things are better left as they are, and that no injurious consequences can arise in this region." How astonished these "waiters upon Providence" must have been to find the cholera breaking out amongst them, as it did shortly afterwards!

Simla, which is our principal Himalayan sanitarium, from the account given of it by Dr. Clarke, appears to be even worse than Ootacamund, so that we are but little surprised to hear that diarrhœa is so common there as to have obtained the expressive name of "Simla trots," or that Dr. Mackinnon, Deputy Inspector-General of Hospitals, should remark: "Of late years Simla has rather retrograded in public opinion as a sanitarium."

The same story, in a more or less modified form, is told of several other well-known hill sanitaria, until one is almost led to ask whether they were not so named on the *lucus a non lucendo* principle, so little does there appear of sanitary arrangement about any of them. Instead of their elevated situation and cooler atmosphere making conservancy less requisite than it is on the plains, as is the common idea, these conditions are just those which render it more imperative. For, as Dr. Moore correctly remarks, on the plains of India the heat is so intense that the bodies of animals and their excreta, when exposed to the sun, scorch and dry up, their moisture is dissipated, and the first element of putrefaction is thus destroyed. In hill climates, on the contrary, there is just enough warmth to excite decomposition, and to develop all the conditions most favourable to its rapid extension. No wonder, then, that under these circumstances opinions should be much divided as to the value of hill-stations in a sanitary point of view. But, irrespective of any considerations which may be based upon the ill-regulated sanitary arrangements of these stations, there are strong grounds for believing that their value as residences for invalids has been too highly estimated. Evidence from various sources shows that leaving out of the question typhus, cholera, small-pox, "hill diarrhœa," and other affections of a more or less zymotic character, by which these stations are frequently visited, and which are probably in a great degree owing to the neglect of sanitary precautions to which we have just referred, those who resort to them from the plains for the restoration of health are peculiarly liable to hepatitis, and to phthisis and other kinds of pulmonary affection. The sudden change from a torrid to a temperate region, or more correctly speaking, to a region whose climate combines in itself the influence of a vertical sun and a semi-frigid atmosphere, tends to arouse the embers of dormant visceral disease, or to concentrate the cachexia which the malaria of the plains developes upon those organs which are most easily affected by the prejudicial influence of external cold. Hence, it is only under certain favourable conditions that the invalid from the plains can expect to derive any beneficial result from a residence at a hill-station, and Dr. Moore even commits himself to the dictum that—"the longer a European lives on the plains, the less likely is he to receive benefit from the hill climates; for during his residence on the plains he is daily in danger of contracting those diseases, or tendency to diseases, to which sudden removal into a colder atmosphere becomes an obvious and powerful aggravating cause."

The true value of these stations we believe, lies, as Dr. Moore labours to prove, in their utility as periodic retreats for the European *before* he becomes the subject of disease on the plains, and not after. If the English soldier were sent to them for a short time on his first arrival in India, so that he might be gradually inured to the influence of the tropical sun under which he is called to dwell, and if at stated periods whole regiments, or sections of regiments, were regularly transferred to them, not for curative objects, but solely as a prophylactic measure, we have little doubt that the mortality amongst our Anglo-Indian soldiers would be quickly reduced fifty per cent. There may be some difficulties in the way of carrying out this arrangement, especially in the case of cavalry and artillery, but the results to be achieved by such a step are so great that it would be worth while making large sacrifices in mere official routine to accomplish it.

But when hygienic art has done all that it can to prevent disease, and when all the powers of medicine have been enlisted to cure it, there will still remain a notable residuum of mortality and ruined health which must ever attend the residence of the white man in India. Malaria confronts him wherever he locates himself. It reigns supreme over the low-lying districts in which are situated the great centres of commerce and political importance. It even dogs his steps if he retreats to the cooler air of the mountain ranges; it poisons the water he drinks and the air he breathes; and though he may pass apparently unscathed through the ordeal of his prescribed residence in the country, he will too often reach his native shores only to find that he has brought with him the seeds of its fatal influence in some visceral affection which the change to a colder climate calls into active growth. Under such circumstances as these, his existence in India is little else than a constant combat with disease, from which he cannot hope to escape unconquered but by the aid of a good constitution and the observance of the most regular habits of life. The more perfectly he possesses the former, and the more rigidly he adheres to the latter, the better will be his chance of resisting those attacks to which he will inevitably be exposed, and the longer will he be able to protract his residence in the country.

The possibility of his living there at all is, at best, only a question of time. For the Englishman there is no such thing as permanent acclimatization in India.

"Exposure," writes Dr. Moore, "instead of 'hardening' the system, actually has the contrary effect, and *the longer Europeans remain in this country the more they feel the effects of the vertical sun.* Men with a larger amount of strength and vital force than others can bear exposure and the effects of heat longer than those not so gifted; but the deteriorating process, though slow, is nevertheless certain, and if acute dysentery, epidemic cholera, ardent fever, or sunstroke do not some day suddenly destroy, insidious malarious disease, cachexia loci, or splenic leucocythæmia sooner or later results."

These remarks must, however, be understood only as applying to the European who is located in the lower districts of India. There is every reason to believe that on the hillside, and on the elevated plains which are to be found amongst the mountain ranges, life is attended by no more serious risks than it is at home. The indigo planters are acknowledgedly a fine and a healthy set of men, and so far as the experiment has yet been tried there appears to be no cause in the nature of the climate to prevent the permanent establishment of an English colony in these regions, if sufficient inducement could be held out to attract settlers there. The success which has attended the cultivation of tea in the upper districts of India, and the promising prospect which attaches to the still more recent introduction of the cinchona plant,* joined with the advantages which these regions offer for the breeding of sheep and goats, and the cultivation of the vine, will probably ere long attract to them a white population in sufficient numbers to allow the question of their suitability for a permanent English colony to be decided without doubt.

Hitherto the unsettled state of the northern provinces, the not unreasonable shyness of capitalists for embarking in Indian undertakings, and the want of efficient means of communication with the sea-coast, have prevented the hill districts from receiving that attention as a field for colonization which they deserve. Now, however, capital is beginning to flow rapidly into India, and the energy with which plans for the formation of railroads and canals throughout every portion of the empire are being pushed forwards, leads to the hope that a few years will see the slopes of the Himalayas, the Neilgherries, the Shevaroys, the Avarallee, and other ranges, as accessible as Snowdon or the Alps. Not that the attainment even of this result would obviate all the obstacles which stand in the way of rapid colonization of these regions, and especially of the expense of a colonization by European soldiers who have served their full time, which has been more than once mooted. Into this, and other matters connected with the soldier's life in India, in respect to which Dr. Moore expresses views that are marked by good sense, practical experience, and general intelligence, we have not, however, space to enter. Those who may feel an interest in them cannot do better than consult his larger work, which will well repay their perusal.

* See p. 13 of the present number of the Review.

Review IX.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XLV.—*London*, 1862.

The present volume of the 'Medico-Chirurgical Transactions,' besides the usual lists of Officers and Fellows of the Royal Medical and Chirurgical Society, contains twenty-four papers, all of which are of very considerable interest, and of each of which we present a brief abstract :—

I. *Observations on the Discovery of the Original Obstetric Instruments of the Chamberlens.* By Robert Lee, M.D., F.R.S., Obstetric Physician to St. George's Hospital.—The first notice of the invention of new instrumental operations in the practice of midwifery by the Chamberlens is contained, according to Dr. Lee, in a case related by Mauriceau. This case which occurred in Paris in the year 1670, was given up by Mauriceau, and then undertaken by one of the Chamberlens, who happened to be in Paris at the time, but his efforts were unsuccessful, as the patient died in twenty-four hours after the operative interference. Dr. Lee has not succeeded in meeting with a copy of the first edition of Hugh Chamberlen's translation of Mauriceau's work, but Dr. Munk showed him a copy of the second edition, published in 1696, and there is reason to believe that the second is a reprint of the first, which is said by Mauriceau to have appeared in 1672. The preface to the second edition contains the only account of the midwifery forceps ever published by the Chamberlens. The method of delivering women by means of instruments, new at the period, is alluded to by Hugh Chamberlen, as a secret known and practised by his father, his brothers, and himself, and he apologizes for not publishing the secret, as such a proceeding might injure their pecuniary gains ; but he offers, on behalf of his relatives and himself, to assist any woman who may be in need of their services. The secret was the Chamberlen midwifery forceps, but until the year 1818 it was not certainly known what the invention was. In that year, Mr. Carwardine presented to the Medico-Chirurgical Society a very curious and interesting account of the discovery of the Chamberlen forceps, in a notice published in the ninth volume of the 'Medico-Chirurgical Transactions.' It appears that the estate of Woodham Mortimer Hall, near Maldon, in Essex, was purchased by Dr. Peter Chamberlen, before 1683, and continued in his family till about 1715, when it was sold by Hope Chamberlen to a wine merchant, who bequeathed it to the Wine Coopers' Company. A lady who was intimately acquainted with Mr. Carwardine, accidentally discovered in the floor of a closet in this mansion the hinge of a door, which she soon succeeded in opening. There was a considerable space between the floor and the ceiling below, and this vacancy contained several empty boxes, but among these was a curious chest or cabinet, in which were found a collection of old coins, trinkets, gloves, fans, spectacles, and family letters, and also the obstetric instruments. These latter were presented by the family residing at Woodham Mortimer Hall to Mr. Carwardine, and he deposited them with the Medico-Chirurgical Society, thus securing to Chamberlen the posthumous merit of his discovery. The examination of the instruments offered to Mr. Carwardine conclusive proofs of the originality of Chamberlen's invention, and the progress of their construction was distinctly traced in its different stages as it passed through the mind of the inventor. There was first a simple vectis, with an open fenestrum, then there was the idea of uniting two of these instruments by a joint, which makes each blade serve as a fulcrum to the other, instead of making a fulcrum of the soft parts of the mother, and which also supplies a power of drawing the child's head forward. This idea is at first accomplished by a pivot, which being riveted, makes the instrument quite incapable of application. Then he advances so far that he makes a hitch in each vectis for the joint, and fixes a pivot in *one only*, which is to be received into a corresponding hole in the other blade, after they have been applied *separately*. But it is practically difficult, or even sometimes impossible, to lock the joint of the forceps with such accuracy as to bring the pivot and the hole into complete contact, and Chamberlen obviated the inconvenience by producing a lighter and more manageable instrument, which, instead of being united by a pivot, is connected by a tape passed through two holes and wound round the joint, a method combining sufficient accuracy of contact, security, and mobility. Mr. Carwardine is led to believe that Chamberlen was his own artificer, as the workmanship is very rough. Dr. Munk, who has supplied Dr. Lee with much valuable information relating to the Chamberlens, believes that there were two Hugh Chamberlens, both physicians, the elder of whom was the translator of Mauriceau, and was the son of Peter Chamberlen, M.D., who died and was buried at Woodham Mortimer. Dr. Munk considers that Peter Chamberlen was the inventor of the forceps, and that he communicated the invention to his three sons, who survived him, and who all practised midwifery.

II. *On Certain Grave Evils attending Tenotomy, and on a New Mode of Curing Deformities of the Foot.* By Richard Barwell, F.R.C.S., Assistant-Surgeon to the Charing-cross Hospital.—Mr. Barwell thinks that the modern operation of tenotomy in the treatment of deformities of the foot has been much over-rated, and that it often produces lameness which is worse than the original disease. He reviews the experiments on dogs performed by M. Bouvier, and shows that in all the recorded cases some of the subcutaneous structures were permanently injured. Although post-mortem examinations of the human subject are rather rare, Mr. William Adams has collected the results of thirteen autopsies, in six of which the tendo-achillis only was divided; but Mr. Barwell observes that in the seven other cases other tendons were also divided, and in every one of these latter, one or more of the severed tendons was either not united at all or was adherent to the bone or surrounding parts, so as to abolish the action of the muscle. When the tibialis posticus and flexor longus digitorum were divided, they never united so as to be of the slightest use, while the tibialis anticus is more exposed to extinction by non-union than by false union. Mr. Barwell concludes, that such muscles as the tibialis posticus, flexor longus digitorum, and probably also the peronæi, might as well be struck with sudden and irremediable paralysis as be subject to the knife of the tenotomist, and that of some other tendons a nearly similar remark might be made, particularly of those in front of the foot. In one of Mr. Adams's cases, an attempt had been made to divide the posterior tibial tendon; but, although the case was successfully treated, it was subsequently discovered, on a post-mortem examination by Mr. Adams, that the tendon was *not* divided. Hence Mr. Barwell asks whether many of the cases in which the tendon was really divided would not have done as well without such division, and he argues that the tendons in question should never be divided, except in the very rare cases in which no other means will succeed in reducing the deformity. It is therefore necessary to return to mechanical treatment for the elongation of certain muscles. The best forms of instrument hitherto invented act on the principle of confining the foot in a shoe, which, by means of springs, bends or twists the limb in a direction contrary to that of the deformity; but Mr. Barwell considers this instrument objectionable for various reasons, and more especially because it twists the foot only as a whole, without reference to the action of the special tendons and muscles, and to the nature and uses of the bones of the foot. Mr. Barwell then describes a simple mode of treatment devised by himself, and which he illustrates by plates. The apparatus consists of strapping-plaister, a piece of tinned iron, about an inch broad and as long as the patient's leg, a few india-rubber springs, and some other accessory contrivances. The application consists in exercising pressure upon the particular muscle or tendon affected, and thus gradually removing the distortion.

III. *Congenital Malformation of the Eyes in Three Children of One Family.* By THOMAS NUNNELEY, F.R.C.S., Senior Surgeon to the Leeds General Eye and Ear Infirmary.—In this communication Mr. Nunneley describes a peculiar condition which he observed in the eyes of three children—two girls and a boy. In the girls, who were not affected in the same degree, the globes of both eyes were not perfectly round, being flattened in some manner by the recti muscles, and were smaller than natural. The sclerotic coats were very vascular; the irides dull, thin, and tremulous; and the pupils not in the axis of vision, but placed considerably to the nasal side. Both eyes were myopic. In the case of the boy, there was a total absence of the iris in each eye; the ciliary processes were either deficient altogether, or were too small to be seen; the choroïd appeared to be normal, of a dense black colour in ordinary light, and with the ophthalmoscope the vessels were seen to be beautifully developed. The retina and the lens appeared to be natural. Although no visible difference could be detected in the two eyes, the boy saw much better with the left than with the right, and he learned his lessons at school like other boys. The vision was not improved at all by either convex or concave lenses. On the occasion of Mr. Nunneley seeing this boy, some weeks after his first examination, a cataract was found to be completely developed in the right lens.

IV. *Observations on the Division of the Gustatory Nerve, and on Ligature of the Lingual Artery, in the Treatment of Cancer of the Tongue.* By CHARLES A. MOORE, F.R.C.S., Surgeon to the Middlesex Hospital.—Mr. Hilton published, in the 'Guy's Hospital Reports,' in the year 1850, a case of cancer of the tongue, in the treatment of which he had divided the gustatory nerve on the side of the disease. The operation was to a certain extent successful; but no other surgeon appears to have adopted the same proceeding except Mr. Moore, who has practised it in five cases of cancer of the tongue, and has formed a high estimate of its value. The objects of the operation are to destroy the sensibility of the ulcer, so as to enable the patient to take food, to reduce the flow of saliva, and to relieve the pain experienced over the parts supplied by the fifth nerve. Besides the pain in the tongue and the jaw, the cancer often causes acute pain in the parotid region, and in the ear, the temple, and the crown of the head, with all of which parts the fifth nerve sympathizes by means of its branches. It is true that the glosso-pharyngeal and the sympathetic nerves have likewise their share of pain in cancer of the tongue, but no surgical remedy can successfully be applied to either; still very great relief may be obtained by the division of the gustatory nerve between the disease and the brain. A patient on whom this operation has been performed should be relieved of pain in the tongue in front of the fauces, as well as in the jaw, temple, and crown of the head; he should lose the incessant annoyance arising from the increased flow of saliva, should speak more freely and swallow with less difficulty, should sleep better, and be better nourished than before; and, so far as the gustatory nerve is concerned, this alleviation is actually obtained. Mr. Moore divides the nerve far back in the mouth, the guide to its position being the last molar tooth, and, although the nerve cannot be seen in the operation, it may sometimes be felt, and can generally be easily reached. The details of three cases are given, all of which were eventually fatal; but in all a considerable amount of relief was afforded. Mr. Moore considers that of all palliative measures employed in cancer of the tongue, none is to be compared for its efficacy with the division of the sensory nerve. The relief is not temporary, like that afforded by anodynes and local applications, and the influence of the operation extends as far as the area of the fifth reaches, and lasts as long as the nerve continues dissevered. The loss of sensation, in all Mr. Moore's cases, was complete, and one of the patients often found himself involuntarily "chewing" his cancerous tumour between his gums. The ligature of the lingual artery in one of the cases produced a much more decided effect upon the tumour than the division of the nerve, and for a time the whole mass of the disease perceptibly diminished. But the result was not permanent, and after five weeks the tumour began to grow again.

V. *Case of Osteo-Malacia.* By ROBERT BARNES, M.D., F.R.C.P., Physician to the Royal Maternity Charity, Assistant Obstetric Physician to the London Hospital, &c.—The case recorded by Dr. Barnes was that of a woman, forty years old, who had been married ten years, but had never been pregnant. The first characteristic symptom of which she complained was persistent and excruciating pain in the dorsal and lumbar vertebræ, and she walked painfully and with difficulty. Her original height had been five feet eight inches; but when Dr. Barnes first saw her she measured barely four feet eight inches. In addition to suffering great pain, she complained of a variety of dyspeptic symptoms, including sickness and pyrosis. She was ordered to take syrup of the diphosphate of zinc and iron, with good diet and wine, and afterwards she took bismuth, hydrocyanic acid, and opium; but after being under medical treatment for upwards of six months she experienced no marked relief. Dr. Barnes now determined to try the effect of cod-liver oil, as he had recently read a paper by Dr. Breslau, recommending that remedy in osteo-malacia. It was given with a few drops of dilute hydrochloric acid to counteract vomiting, and for two months the patient took this combination with decided advantage and without discomfort; but the pains in the limbs were still very distressing, and she was ordered belladonna and morphia, and afterwards Indian hemp, and under the use of the latter the pains were very much relieved, and she came to the hospital walking, feeling much stronger and better. The whole time that the patient was under observation and treatment was from June, 1860, to November, 1861, and on the last occasion when Dr. Barnes saw her she was still in good health and spirits. She was, however, very much deformed, the deformity chiefly affecting the trunk, but sparing the bones of the head, the clavicles, scapulæ, arms, and legs. Nearly the whole spinal column was distorted, the ribs were compressed, and the pelvis was centripetally contracted on all sides, and it was with difficulty that two fingers could be introduced into the outlet. The condition of the urine has not hitherto been carefully investigated in cases of osteo-malacia, but in the present instance the urine was examined and analysed on several occasions by Dr. Barnes, with the aid of Dr. Letheby. The general results of the analysis showed that this fluid was of ammoniacal odour, very alkaline, containing no albumen, with a deficient amount of urea, but with a great excess of the phosphates and of extractives. The deficiency of urea was supposed by Dr. Letheby to be due to the decomposition of that substance; and Dr. Barnes found, on examining fresh urine from the same patient, that the urea was in large proportion. Dr. Barnes admits that the case is somewhat incomplete, and that although the patient had improved, yet that the disease might re-appear. He assigns an important place in the treatment to cod-liver oil.

VI. *On some Affections of the Cæcal Portion of the Intestines, with Illustrative Cases.* By FREDERICK GEORGE REED, M.D., M.R.C.P.—Dr. Reed gives the history of four cases, the pathology of which he refers to accumulation or lodgment of the intestinal contents in the cæcum and commencement of the colon. In the first case there was great pain felt by the patient over the whole abdomen, and there was a distinctly defined swelling, about the size of a small orange, in the situation of the cæcum. A variety of treatment was adopted, including the use of leeches and the administration of mercury, and was attended with partial amendment; but one day the patient passed something from his bowels, without his personal knowledge and without his feeling any unusual sensation. The substance appeared to be a portion of intestine, ten or twelve inches in length; and on its being examined by the late Mr. Quekett, it was found to consist of portions of the intestinal canal, the parts best marked being the cæcum and the appendix vermiformis. The patient, who was a youth of eighteen, recovered completely after the evacuation of this portion of intestine.

The second case was that of a girl of fourteen, of healthy appearance, who was seized suddenly with pain in the abdomen after a few days' constipation. Aperient medicines and injections brought away some hard and constipated fæces, but without affording any relief to the symptoms, and a distinct fulness was discovered in the right iliac fossa, about three inches in extent, and there was dulness on percussion. Calomel and opium were administered, and leeches were applied over the swelling; and on the eleventh day from the commencement of her illness the patient passed from her bowels a solid mass, which was found on examination to consist of six or seven inches of intestine in a gangrenous state. From this period she began to improve, and many years afterwards she continued perfectly well.

The third case was a fatal one. The patient was a stable-groom, of a very strong and healthy constitution, who one morning ate hastily three or four good-sized apples not fully ripe, afterwards made a hearty breakfast of meat, and subsequently, while grooming a horse, was suddenly seized with intense pain in the right iliac region. There was no general peritoneal tenderness or any local swelling, and the bowels had acted under the use of aperients. He was bled, and calomel and opium were administered, but without relief, and he died on the fourth day after the attack of illness. The post-mortem examination revealed signs of extensive peritonitis, and the cæcum and appendix vermiformis, as well as about four inches of intestine, were invaginated within the ascending colon, and appeared in a partial state of sphacelus.

The fourth case was that of a lady, in whom a swelling, of about the size of the double fist of an ordinary-sized man, was discovered in the right iliac fossa. Leeches, calomel, and opium were employed, and eventually a large quantity of fæcal matter was evacuated; but the swelling appeared externally, and was found to present fluctuation. It finally burst, and discharged ten or twelve ounces of offensive pus and also fæcal matter; but nevertheless the opening closed up, and the patient recovered.

VII. *The Poisonous Effects of Coal-Gas upon the Animal System.* By C. J. B. Aldis, M.D., F.R.C.P., Medical Officer of Health to St. George's, Hanover Square.—VIII. *Additional Experiments on the Poisonous Effects of Coal-Gas upon the Animal System.* By the same.—In these papers Dr. Aldis relates the results of several experiments made upon rats when exposed to the action of different kinds of coal-gas, the varieties of gas being cannel gas, common gas, and foul gas. The common gas contains, after purification, light carburetted hydrogen, hydrogen, carbonic oxide, carbonic acid, oxygen, nitrogen, and the condensable hydrocarbons; the foul gas contains, in addition, ammonia and sulphuretted hydrogen; and cannel gas contains more hydrocarbon vapours than common gas, for which reason it probably destroyed life more quickly than the common gas. In six of the cases the rats were killed by exposure to the gas, and the post-mortem appearances were very similar, the blood being dark-coloured and fluid. In other cases the rats were made insensible by breathing the gas, but they afterwards recovered. In another set of experiments the rats were exposed to gas mixed with varying proportions of atmospheric air. In one of these cases the rat recovered after immersion in the gas for an hour. In three other cases the animals died, and the blood was found after death to be fluid, but of a brighter colour than when gas alone was employed.

IX. *On the Temperature, Urea, Chloride of Sodium, and Urinary Water in Scarlet Fever, and on a Cycle in Disease and Health.* By Sydney Ringer, M.B., Resident Medical Officer to University College Hospital.—The observations on which Mr. Ringer's paper is founded were made on inmates of the Hospital for Sick Children, and they were undertaken to ascertain what relationship exists between the urinary excreta and the temperature in scarlet fever, but they also embrace the determination of the relative quantity of urea, of the chlorides, of the urinary water, of the albumen in the urine, and of the blood in the urine, in the same disease; and Mr. Ringer proposes a theory showing a cycle in disease and a cycle in health, and their relation to each other. It is impossible to give an abstract of Mr. Ringer's elaborate paper, but it appears by his researches that in the large majority of cases the temperature falls on the fifth, tenth, fifteenth, or twentieth day—that is, either on the fifth or a multiple of the fifth day of the disease; and if the temperature should remain high on the fifth, tenth, or fifteenth day, a continuation of the fever for another five days may be expected. As to the urea, it is shown that there is no increase in the amount of this substance during the fever, and that subsequently to the fever there is a great diminution of urea. The chlorides were never absent from the urine that was analysed, but the quantity was always much diminished during the fever days, increased during the three or four days following the fever, and restored to their normal quantity. The albumen appears in the urine at two different periods—first, during the fever; and secondly, about the third week. From the cases given by Mr. Ringer, it appears that no number of days can be defined as marking the elevation or decline of the temperature, but that a cycle of five days is the most common of all.

X. *On Pulse-Breath.* By C. Radclyffe Hall, M.D., F.R.C.P., Physician to the Hospital for Consumption, Torquay.—Dr. Radclyffe Hall, by the term "pulse-breath," signifies an audible pulsation communicated to the breath as it issues from the mouth, by each beat of the heart. The sound is that of a gentle gushing of the breath, synchronous with each pulsation of the heart, and the degree of audibility varies in different cases and in the same case under varying circumstances. Dr. Hall has heard it so loud, that he could count the pulse by it at a distance of fifteen feet, and on the other hand it has been so subdued, that it was requisite to listen close to the patient's face for its detection. He heard it distinctly in two cases of phthisis in an advanced stage, and he considers that the mechanism of the production of the phenomenon is easily explained. A phthisical cavity old enough to possess rather dense walls, and tolerably dry, by being emptied of its customary contents, and not immediately separated from the heart by permeable lung-tissue, vibrates with each beat of the heart, and at each vibration throws the air in the cavity, trachea, larynx, and mouth into a sonorous pulsation. When the cavity is more or less filled with liquid, it no longer vibrates, and as this is the habitual state of a cavity which has not collapsed, the phenomenon of "pulse-breath" is a rare occurrence. But Dr. Hall observed the same phenomenon in a totally different case—namely, one of cardiac disease with enlarged liver, pulmonic congestion, and general anasarca. In the latter instance the explanation is more difficult than when a phthisical cavity exists, but the sound is supposed to be due to the impulse of the heart conveyed through the bloodvessels to the air-cells and passages. "Pulse-breath" has been hitherto undescribed by authors.

XI. *On Brass-founders' Ague.* By Edward Headlam Greenhow, M.D., F.R.C.P., Consulting Physician to the Western General Dispensary, and Assistant-Physician to the Middlesex Hospital.—Dr. Greenhow visited some brass-founders' shops in Birmingham in 1858, and he learned that this class of workmen were liable to suffer from a well-defined form of ailment, known among themselves by the name of ague, and to which he therefore applied the term of "brass-founders' ague." Since that time Dr. Greenhow has found that the complaint prevails in other towns where brass-founding operations are pursued, and he has discovered some notices of this form of complaint in the writings of one British and several foreign authors. The symptoms consist in a sense of weariness or nervousness, a feeling of tightness in the chest, followed by shivering, sometimes succeeded by an indistinct hot stage, but invariably by a very definite stage of profuse sweating. The cause of this malady appears to be the inhalation of the fumes of deflagrating zinc, one of the constituents of brass; and the correctness of this opinion is proved from the fact that persons who work with other metals do not suffer from the same symptoms, while, on the other hand, brass-founders suffer in almost exact proportion to their liability to inhale these fumes. Brass-founders are not all equally exposed to the cause of this disease, for those who use but little zinc in their castings, or cast but rarely, altogether escape, and those who work in large, airy, well-ventilated workshops, suffer less than such as work in smaller and ill-ventilated places. Those, also, who cover their mouth and nostrils with a handkerchief while they are casting, avoid the inhalation of the fumes of the zinc, and suffer much less than those who neglect this precaution.

XII. *On the Connexion between a Local Affection of the Lymphatic System and Chylous Urine, with Remarks on the Pathology of the Disease.* By H. V. CARTER, M.D., Professor of Anatomy and Physiology, Bombay Medical College.—Dr. Carter relates the history of three cases, in which there was derangement of the lymphatic system, leading to local accumulations of chyle, its occasional discharge from the cutaneous surface, or its appearance in the urine. These phenomena sometimes occur together, sometimes separately. In one of the cases there was a small pimple on the surface of the thigh near Poupart's ligament, and from this point a milky fluid was poured out, and sometimes so copiously, that in the course of the day a pint was obtained. The fluid, when collected, resembled rich milk—its colour was yellowish or bluish-white, but it soon acquired a rather pink tint. It had a faint odour and a slightly alkaline reaction, and on standing in a test-tube, it separated into clot and serum. The microscopic examination revealed the presence of groups of minute granules, red blood-corpuscles, oil-globules of various sizes, granular corpuscles, and a few masses of granules. Instances of the more ordinary forms of chylous urine are said by Dr. Carter to be not uncommon in India. The explanation of the phenomenon of chylous urine is difficult, but Dr. Carter supposes that the distension of the delicate lymphatics and lacteals in the lumbar region is at length followed by exudation of their contents at one or more points; or when rupture takes place, a fistulous orifice remains, giving occasional free exit to the chyle or lymph, or an abnormal reservoir may be formed which periodically discharges its contents into the pelvis of the kidney, ureter, or bladder.

XIII. *On a Case of Chylous Urine.* By A. T. H. WATERS, M.R.C.P., Physician to the Liverpool Northern Hospital.—The case related by Dr. Waters was that of a young seaman, a native of Bermuda, who was admitted into the Liverpool Northern Hospital in the first instance for retention of urine. It was soon discovered that the cause of the retention was a coagulation of the urine within the bladder, and it was also discovered that this fluid was of a milky colour and appearance. When first passed, it was white, with rather a pink tinge, resembling new milk in appearance and somewhat in smell, and perfectly free from urinous odour. After it had been passed for a short time, it coagulated into a tremulous mass, exactly resembling *blanc-*

mange. When examined by the microscope, it was found to contain blood, pus, and mucus-corpuscles, with a large number of small fat-globules. On comparing the chemical composition of the chylous urine with that of healthy urine, it was found that in the chylous urine there was a large proportion of water, fat, albumen, and mucus, and a deficiency of urea, extractive matters, and alkaline and earthy salts. Dr. Waters considers that the principal pathological feature of this disease is a relaxed condition of the capillaries of the kidneys and that, as a consequence of this condition, the albumen, the fibrine, the fat- and the blood-corpuscles, are filtered from the bloodvessels, and make their appearance in the urine. The treatment of the case here related was successful, the chief therapeutical agent employed being gallic acid; and Dr. Waters believes that this medicine has the power of controlling the affection. It should not, however, be administered in small quantities, but must be given in large and gradually-increasing doses. In the case of Dr. Waters' patient, the persistent use of the vapour-bath also appeared to have a decidedly beneficial effect.

XIV. *Observations on the Tactile Sensibility of the Hand.* By Edward Ballard, M.D., M.R.C.P., Medical Officer of Health for Islington.—In this paper Dr. Ballard shows, by observations on his own hand, the relative sensibility of its different parts. The phenomena being subjective, the great difficulty to be surmounted is the acquisition of a standard by which they may be measured. The standard must be entrusted to the memory, and it must be definite and decided; and as no part of the body conveys to the intelligence more precise impressions than the hand does, so there is no part with which tactile impressions made elsewhere can be so readily compared. In Dr. Ballard's investigations, he employed the method suggested many years ago by Weber, using an ordinary pair of compasses, tipped with cork points, and after trying other materials, he returned to the cork, as affording the purest kind of tactile impression. The plan adopted to represent the spots of the surface experimented upon was to mark the intended spots with a cross, and having done this, Dr. Ballard had photographs taken so as to represent precisely the locality of each observation. Certain numbers were then chosen as indicating the highest, the lowest, and the mean sensibility of the entire hand, and the intermediate numbers indicated the different degrees of sensibility in different parts. Dr. Ballard gives the results of his researches in a most elaborate report, illustrated by diagrams and copious tables. He confirms the general truths that the sensibility of the hand increases gradually from the wrist to the extremity; that the palmar surface is more sensitive than the dorsal; that the tips of the fingers are the most sensitive parts of the fingers, &c.; but he compares the sensitiveness of one part of the hand with another, and of one finger with another, and of each part of each finger, with an amount of precision which has hitherto been unattained.

XV. *On the Influence of Paralysis, Disease of the Joints, Disease of the Epiphysial Lines, Excision of the Knee, Rickets, &c., upon the Growth of the Bones.* By GEORGE MURRAY HUMPHRY, M.D., F.R.S., Surgeon to Addenbrooke's Hospital, Cambridge, Lecturer on Anatomy and Surgery.—Dr. Humphry has brought together the details of a number of cases showing the influence of the various affections here enumerated upon the growth of bones, and he proves that the bones usually decrease in size under such circumstances, only one instance being given of increase of growth from disease. The question of the growth of a limb after excision of the knee is one of much importance with reference to the propriety of performing this operation in young subjects; and Dr. Humphry has collected a list of eighteen cases in which it had been performed. In the first eight on the list, the growth of the limb operated on maintained its proper rate as compared with that of the other limb; but in two of these cases the operation was performed at the age of seventeen, when the growth of the body may have been nearly completed. In the remaining ten cases the growth of the limb was more or less arrested. Dr. Humphry supports his conclusions by accurate measurements of the bones affected. The effect of rickets is usually to arrest the growth of the bones, and the exceptions to this rule are few. The deficiency is most marked in the limbs, the upper extremities being little more than three-quarters of the proper length, but it is still more apparent in the lower limbs, which are less than three-quarters of their proper length; and the deficiency is greater in the femur than in any other part of the skeleton. Dr. Humphry gives a tabular view of sixty-one cases, collected from different museums, showing the measurement of the bones in cases of rickets.

XVI. *Analysis of* 230 *Cases of Lithotomy.* By THOMAS BRYANT, F.R.C.S., Assistant-Surgeon to Guy's Hospital.—Mr. Bryant arranges his analysis of cases under four tables, the first of which shows the frequency of the operation of lithotomy at the different periods of life, the mortality after the operation, and the comparative mortality at different periods of life. The second table shows the causes of death, as proved by post-mortem examination, the ages of the patients being also given, with the duration of the symptoms. The third table comprehends the assigned causes of death, not proved by post-mortem examination; and the fourth table shows the apparent influence of chloroform upon the mortality of lithotomy. With reference to the latter point, the weight of evidence appears to be unfavourable to the administration of chloroform in lithotomy. The mortality of the operation without the use of the anæsthetic was 11 per cent., and with it 16·9 per cent., the use of chloroform thus raising the mortality 50 per cent. Mr. Bryant explains this different degree of mortality by the suggestion, that since the introduction of chloroform surgeons have been induced to operate more frequently than formerly, and that many necessarily fatal cases are included among the number operated upon. When lithotomy is complicated with renal disease, the use of chloroform is injurious, and it is probable that the stimulating effect of the chloroform on the diseased kidneys may explain the increased mortality.

XVII. *On the Treatment of Acute Rheumatism, considered with regard to the Liability to Affections of the Heart under different Remedies.* By W. H. Dickinson, M.D., Assistant Physician to the Hospital for Sick Children, Curator of the Pathological Museum at St. George's Hospital.—The tables which, as Medical Registrar of St. George's Hospital for several years, the author collected, extended over five years, beginning with January, 1857, and ending with December, 1861. The cases were classified according to the treatment adopted, and the condition of the heart was noted throughout. Bleeding, in addition to other remedies, was adopted in eight cases, and the heart was affected in four. Calomel and opium were given in twenty-four cases, in combination with other medicines, and the heart was affected in six. Opium is considered by Dr. Dickinson as highly injurious in acute rheumatism. Seven cases treated with nitre, without salines, alkalies, or mercury, gave only one example of cardiac complication. Dr. Dickinson then reviews the saline treatment, the partial alkaline treatment, and the full alkaline treatment, the latter consisting in the daily administration of alkaline salts, varying in quantity from half an ounce to an ounce and a half daily. Forty-eight patients subjected to large doses of alkaline salts presented only one case of cardiac disease. The conclusion drawn is, that the carbonates of potash and soda, with their other salts which are converted into carbonates in the system, exert an especial curative power over rheumatic fever, and protect the patient from cardiac disease.

XVIII. *Amaurosis, consequent on Acute Abscess of the Antrum, produced by a Carious Tooth.* By James A. Salter, M.B., F.L.S., Surgeon-Dentist to Guy's Hospital.—Mr. Salter describes the case of a servant-girl who presented herself with an enormous swelling of the right side of the face, the eyelids being œdematous and closed, and pus streaming out from the lower lid. The second right upper bicuspid tooth had been recently removed, but the fangs of the first molar, and a carious dens sapientiæ remained. The stumps and the carious tooth were removed without difficulty, and when the fangs were extracted, the floor of the antrum was opened, and an abundant purulent discharge was poured into the mouth. The whole of the upper maxillary bone appeared to be involved in disease; the sight of the right eye was entirely gone, the globe was prominent, and there was conjunctivitis, with general and deep inflammation of the fibrous textures of the organ. The extraction of the teeth caused a considerable abatement in the severity of the symptoms, but the maxillary bone was necrosed, and large portions of it were removed. The patient eventually recovered, but the sight of the right eye was permanently lost, although the humours were all perfectly transparent, and the retina was healthy. Mr. Salter refers to two other cases of a somewhat similar character, one of which occurred in the practice of Mr. Pollock, of St. George's Hospital, and another, the details of which were recorded by Dr. Brück in Casper's 'Wochenschrift' for 1851. The latter case, however, was much more chronic than either Mr. Salter's or Mr. Pollock's case, and the loss of vision was less permanent.

XIX. *Two Cases of Extensive Arterial Obstruction from Separated Cardiac Vegetations, followed by Gangrene of the Lower Extremities, and Death.* By S. J. GOODFELLOW, M.D., F.R.C.P., Physician to, and Lecturer on Medicine at the Middlesex Hospital.—Dr. Goodfellow's first case was that of a sempstress, thirty years of age, who was admitted into the Middlesex Hospital, suffering from severe pain in the calf of the left leg and ankle, and a sense of numbness in the toes. There was a systolic murmur heard loudest near the apex of the heart, and the impulse of the organ was very strong and heaving. Dry gangrene of the left foot soon manifested itself and slowly extended, and there was pain and numbness of the right arm, but without discoloration. She sank from exhaustion, and after death the heart was found enlarged, and the curtains and edges of the mitral valve were covered with large vegetations; there was a firm coagulum in the right brachial artery, and there were also coagula in the abdominal aorta, the cœliac axis, the common iliacs, and several other arteries. The second case was that of a girl of seventeen, who had suffered from acute rheumatism, and in whom there was extensive cardiac dulness, and both diastolic and systolic murmurs. Gangrene developed itself in both feet, and she died; and on post-mortem examination, numerous soft vegetations were found upon the mitral valve, and firm coagula obstructed the right subclavian and part of the right axillary, the left carotid, and the common, external, and internal iliac arteries. Several of the minute arteries of the brain also appeared to be plugged with coagula.

XX. *Case of Iliac Aneurysm.* By JAMES SYME, F.R.S.E., Professor of Clinical Surgery in the University of Edinburgh, and Surgeon to the Queen in Scotland.—Mr. Syme dissents from the usually received doctrine that the whole extent of an artery included in the aneurysmal sac should be deemed of suspicious soundness, and he has repeatedly operated with success by laying open the cavity, and securing the orifice by ligatures, trusting to find a sound portion of artery within the sac. He relates a remarkable case which was thus treated. It was that of a sailor who injured his left groin, and soon afterwards perceived a small tumour, which gradually enlarged to an enormous size. Throughout the whole of the swelling there was a strong pulsation and a distinct aneurysmal bruit. Mr. Syme thrust a knife into the aneurysm, and found a mass of clots, which were removed, to the amount of six pounds by weight. Then, having availed himself of the screw clamp invented by Mr. Lister for compressing the aorta, Mr. Syme dissected the parts so as to bring the coats of the artery distinctly into view, and passed a ligature on each side of the opening by which the artery communicated with the sac. The patient went on favourably, and on the nineteenth day after the operation, all the ligatures came away together, and then the wound gradually contracted. But some months afterwards, the man caught cold and died of pleurisy, and on examining the part operated upon, it was found that the external iliac had been torn completely across, and its open mouth had caused some misapprehension as to its real nature at the time of the operation.

XXI. *Contribution to the Statistics of Cancer.* By W. M. Baker, M.R.C.S.—This paper is founded upon the notes of 500 of the cases of cancer, taken by Mr. Paget between the years 1843 and 1861, the original record of the cases having been made as they occurred, and without any reference to the particular kind of cancer, its seat, or its influence on the duration of life. In the 500 cases selected by Mr. Baker, only such cancers are included as are external, or may be called surgical, the so-called medical cases being omitted. It is shown that females are far more liable to cancer than males, but this is due to the cases of cancer of the breast, which are very numerous; and the proportion of females affected would be much larger if cancer of the uterus were included in Mr. Baker's tables. It appears, however, that on subtracting the cases in the reproductive organs and breast in both sexes, as well as those in internal organs, the proportion remaining is considerably higher in males than in females. As to the age most liable to cancer, it may almost be regarded as a rule that, for the external organs, medullary and melanotic are the cancers of youth and early adult age, scirrhous, that of middle life, and epithelial, that of middle and old age. Married women seem to be more liable to cancer of the breast than single women. The interesting question of the recurrence of cancer after operation cannot be accurately determined among the poor patients who attend at an hospital, because they are often lost sight of, unless the rapid return of the complaint obliges them to come back. Mr. Baker's paper also includes the influence of age on the recurrence of cancer, the influence of early and late operation, the influence of operation upon the duration of life, and many other important questions, but the results are not sufficiently definite to admit of their being included in an abstract.

XXII. *Report upon Syphilis, with reference to the more Mixed and Unusual Forms of the Primary Symptoms.* By Jeffery A. Marston, M.D.—The author of this paper is an army medical officer, and he treats of syphilis with especial reference to its more unusual, mixed, and anomalous forms. From Dr. Marston's experience, he is led to believe that the etiology and pathology of the disease are by no means so well-defined as the authors of the French school maintain. In model cases it is true that the distinctions between an infecting and a non-infecting sore can be easily drawn; but mixed cases frequently occur, offering great difficulty in their diagnosis. Dr. Marston arranges his materials under six heads: viz., 1. The varieties of infecting sore. 2. The results of auto-inoculation. 3. The occurrence of syphilitic infection after suppurating bubo. 4. The occurrence of constitutional symptoms following an urethral discharge clinically identical with gonorrhœa. 5. The bubon d'emblée. 6. The periods of incubation preceding the appearance of the two kinds of venereal sores, and the absence of any proof that we can guarantee against constitutional infection by any abortive treatment applied to the primary syphilitic lesion. In reference to the latter point, Dr. Marston relates three cases in which syphilitic sores were treated at the earliest possible period by the application of nitric acid, but in which, nevertheless, secondary symptoms supervened.

XXIII. *Case of Aneurysm of the External Iliac and Common Femoral Arteries, treated by Digital Pressure, with Observations.* By Henry Lee, F.R.C.S., Assistant-Surgeon to St. George's Hospital.—Mr. Lee's patient was a man, aged thirty-one, who, after a slight accident, experienced some pain in the left groin, which afterwards became swollen and painful. On his admission into the hospital, the whole of the thigh was considerably swollen, and some patches of livid discoloration existed on the outside of the limb. In the left groin was a tumour, four inches in diameter from side to side and from above downwards, pulsating strongly; and on the application of the stethoscope the aneurysmal bruit was distinctly heard, while a very distinct thrill could be felt in every part of the tumour. All pulsation in the part could be arrested by pressure upon the external iliac artery. At Mr. Lee's request, twelve of the students of the hospital undertook in turns to keep up digital pressure upon this artery, so far as it could be borne without inconvenience; and on the day following the adoption of this treatment, the pulsation in the swelling was decidedly diminished. It was determined, in consultation, to continue the treatment by pressure, under which the case partially improved, but the amendment was only temporary, and death ensued in about four months. On a post-mortem examination, it was found that the common femoral artery was completely divided, the greater part of the walls of the aneurysmal cavity being formed by the surrounding structures. Mr. Lee remarks that the digital pressure used in this case appears to have had the effect of causing the formation of a very complete and firm coagulum, and so far the treatment may be said to have been successful, but this coagulum gradually gave way, being unsupported by the coats of the artery.

XXIV. *Report of the Committee appointed to investigate the Subject of Suspended Animation.*—The Committee on Suspended Animation, appointed by the Royal Medical and Chirurgical Society, consisted of Dr. C. J. B. Williams, Dr. Brown-Séquard, Dr. Harley, Dr. Kirkes, Dr. Hyde Salter, Dr. J. B. Sanderson, Mr. W. S. Savory, and Dr.

Sieveking, and at the first meeting of this committee it was resolved to pursue their inquiries by means of experiments on living animals, and also by experiments on the dead human body. The Report accordingly consists of two parts, the first being devoted to the results of experiments on the lower animals, and the second to those obtained by experiments on the dead body. In the first series of experiments, dogs and other animals were asphyxiated (apnœa is the word used by the committee in preference to asphyxia), with a view of ascertaining the period which elapsed between the last respiration and the last pulsation of the heart; also to ascertain how long an animal might be in a state of apnœa without actually dying; and to determine the best treatment in cases of apnœa. The committee, in investigating this latter point, employed artificial respiration, the actual cautery, venesection, the cold splash and douche, the hot douche alternated with cold, galvanism, and puncture of the diaphragm, but none of these methods was of such unequivocal efficacy in a sufficient number of cases as to warrant the Committee in specially recommending the adoption of one in preference to the others. The second series of experiments were made upon the dead human subject, with the view of determining the value of the various methods which have been employed for alternately compressing and expanding the cavity of the chest in such a manner as to imitate the natural movements of the thoracic walls in breathing. The following methods were investigated—viz., 1. Pressure exerted by the hands on the anterior wall of the thorax, the body being in the prone posture; such pressure having for its object to expel a portion of the air contained in the chest; on relaxing the pressure the chest expands and air enters. 2. The postural, or so-called ready method, described by Dr. Marshall Hall, which consists essentially in "turning the body gently on the side and a little beyond, and then briskly on the face alternately;" and in making pressure along the back of the chest each time the body is brought into the prone position. 3. The method of Dr. Silvester, in which the action of the pectoral and other muscles passing from the shoulders to the parietes of the chest in deep inspiration, is imitated. An inspiratory effect is produced by extending the arms upwards by the sides of the head; on restoring them to their original position by the side of the body the expanded walls are allowed to resume their previous state, and expiration takes place, the quantity of air expelled being in proportion to that which had been previously inspired. The experiments were performed upon dead subjects in the metropolitan hospitals, and the results were obtained by an apparatus, devised for the purpose, and consisting of a kind of spirometer adjusted to a counterpoise. We regret that our limits preclude us from describing at length the series of interesting and valuable experiments instituted by the committee, but we may state generally that the conclusion at which they arrived was, that in the treatment of apnœa the method of Dr. Silvester was preferable to that of Dr. Marshall Hall, and that they recommend in such cases a modification of the former plan to be generally adopted.

Review X.

Du Mexique au Point de Vue de son Influence sur la Vie de l'Homme. Par D. Jourdanet, Docteur en Médecine des Facultés de Paris et de Mexico.—*Paris*, 1861. pp. 400.

The Influence of Mexico on the Life of Man. By Dr. Jourdanet.

It would be hardly possible to name a region of the earth which, within the same limited area, presents such a variety of physical conformation, or such a combination of the most favourable and the most prejudicial influences to which the human frame can be exposed, as the southern portion of the Republic of Mexico. From the littoral district by which it is bounded on the east to the elevated plateau on which the capital, Mexico, itself stands, the traveller will pass through scenes which are often made up of more vivid contrasts and more violent extremes than any which the tamer physiognomy of Europe can supply. In one part he will find, amidst the wildest fertility of vegetative growth, the haunts of some of the worst maladies to which flesh is heir; in another, a short ride will carry him from a soil so fertile and a climate so benign that the productions of tropical and temperate countries may alike be cultivated there with success, to a barren and desolate plain, almost entirely destitute of life; whilst in a third he may step, with a single stride, from the blazing effulgence of a vertical sun into a cold shadow that will chill him to the bone. Even before he sets his foot on a shore whose pestilence has become proverbial, the haggard and sallow countenance of the pilot by whom his vessel is conducted into Vera Cruz will warn him that he has arrived at a land where disease marches at a more rapid pace and leaves behind it a more permanent imprint of its attack upon the constitution than it does in the milder climates which he has quitted. So great, indeed, is the mortality of the eastern maritime districts of Mexico, and so heavy is the price, either in health or life, which the unacclimatized stranger is in most cases called upon to pay for his residence in them, that it would almost seem as if nature had surrounded the mineral treasures that lie hidden in its mountains with a barrier so repulsive that nothing but the most insatiable thirst for wealth would encounter the risk involved in their collection.

But, with all these drawbacks, Southern Mexico is by no means either uniformly unhealthy or unattractive. Within the limits of the *tierras templadas* there are districts in which all the advantages of the temperate are so admirably blended with all the charms of the tropical zone as to produce, by their combination, the nearest approach to perfection of physical existence with which we are acquainted. And even in Vera Cruz itself, where the dreaded *vomito* is an endemic scourge, its terrors are mainly confined to foreigners, the natives and those who are acclimatized enjoying a general immunity from disease which is fairly comparable with that of the average of European countries. So important, indeed, is it to take into consideration the element of acclimatization in all questions affecting the mortality of any locality, that it would be as unfair to estimate the sanitary character of the Gulf of Mexico by the ravages which yellow fever commits amongst the immigrant population there, as it would be to estimate that of London by the mortality which is exhibited by Lascars and other natives of the tropics who fall victims to the inclemency of our northern winters. However efficiently nature may have enabled man to withstand the prejudicial effects which a sudden change of external conditions—whether of climate, food, or habit—invariably produce upon him, such a change is always more or less attended with danger, and the period during which the process of adaptation is being carried out will be liable to disturbances of health against which it will require the greatest caution to guard. Hence the term "unhealthy," when applied to a locality, has within certain limits a relative significance, great mortality amongst strangers to it being often consistent with a reasonably healthy condition of the natives.

It is to this land of startling contrasts, upon which nature has apparently exhausted all the resources of her creative power, and whose unhappy destiny has made it the scene of political troubles which have at last culminated in open anarchy and a foreign intervention, that we propose for a brief period to transport our readers. Although our share as a nation in the late expedition is fortunately at an end, there is still sufficient interest left in the subject itself, and in the prospects of our brave allies who have continued the crusade on their own account, to excuse our devoting a larger portion of our space to the very interesting work which M. Jourdanet has given to the world on the climate of Mexico than we should probably feel justified in doing to a similar treatise on the climate of any other country. The conditions under which life must be maintained in the wide range of physical features which Mexico exhibits are in many respects so unique, and their operation has been so imperfectly studied, that we should gladly receive any contribution to our knowledge of the influences which they exert, from whatever source it might come. But when a writer adds to the weight which a nineteen years' sojourn in that country gives to his statements the acuteness of observation and the clearness of description of which M. Jourdanet's book gives such abundant evidence, its claim upon our attention is proportionately increased. We shall, therefore, make no apology for presenting to our readers such an outline of it as will enable them to form some idea of the very interesting country to which it relates, and to appreciate some of the important conclusions as to its physiological effects to which the author's long residence in it has led him.

The physical features which characterize the coast of the Mexican Gulf have been rendered so familiar by the numerous travellers who have visited it as to render any description of them unnecessary here; but there is one which is of such importance in a medical point of view, and which has hitherto received so little attention from ordinary writers, as to justify our dwelling for a short time upon that portion of M. Jourdanet's work which is specially devoted to it. If we look at a map of Central America, we shall see that the greater part of the eastern coast of Mexico, from Vera Cruz southwards, is divided between the States of Yucatan and Tabasco. These two States correspond to two broadly-defined districts, whose antagonistic characteristics offer a remarkable illustration of the influence exerted by soil in determining the conditions by which any given locality is distinguished. Washed by the waters of the same sea, whose temperature is 8° or 9° hotter than that of the Atlantic Ocean in the same parallel, and warmed by the same sun to a heat that often reaches 100° Fahr. in the shade, the nature of the soil in these two districts is so diverse as materially to affect the state of the atmosphere and the influence which it exerts upon the human economy in each. In Yucatan it is shallow and calcareous, covered in some parts by a scanty vegetation; in others, presenting to the eye the inhospitable surface of the underlying rocks by which it is penetrated; for the most part arid and destitute of rivers,* though here and there, especially near the sea, exhibiting during the rainy season accumulations of water which are rapidly dissipated as the heat increases. This soil, which has been swept during the greater part of the day by an inland breeze, that has been robbed of its moisture by the dense forests over which it has passed, heats and desiccates the atmosphere above it to such an extent that for a considerable portion of the day life is only rendered tolerable to the inhabitants by all the resources which can mitigate the intensity of its oppressive influence. The contrast which is offered to this condition of things by the neighbouring State of Tabasco is most striking. The greater part of this State is little better than an extensive marsh, from the liability of the numerous rivers by which it is intersected to overflow periodically, and from the rich alluvial deposit with which they inundate it. Under the combined influences of abundant moisture and a tropical sun, the growth of vegetation is everywhere so luxuriant that cultivation only is wanted to make this district one of the most fertile in the world. Yet the virulence with which intermittent fever in its worst form prevails here renders it barely habitable even to the acclimatized Indians and half castes who have taken up their abode in it, and the increase of whose numbers is almost permanently arrested by the mortality which that disease causes. To such an extent is the soil impregnated with the miasmatic elements to which decaying organic matter gives rise, that the act of opening it is said to bring to the white man almost certain death.

Here, then, we have an admirable opportunity for observing the relative effect produced by the extremes of dryness and moisture in the soil in modifying the physiological influence of a tropical sun upon the human constitution.

* Although the ordinary maps are rather ambiguous on this point, M. Jourdanet states that Yucatan is destitute of anything that can fairly be called a river, with the exception of the Uzumazinta, which separates it from Tabasco. The St. Francisco, on which the town of Campeachy is usually represented as located, exists only in the imagination of geographers.

"Amongst the affections most common in Yucatan," observes M. Jourdanet, "pulmonary phthisis occupies the first place; but it is not that slow consumption which throws a poetic interest over the smiling illusions as to the future under which those attacked by phthisis in Europe love to shelter themselves. The phthisis of Yucatan is an acute disease which destroys its victims rapidly, and hurries them to the grave without admitting the shadow of hope. At one time, leaping without an interval over the three classical periods of the disease, its subjects are brought by it to the extreme of emaciation in three or four months; at another, overwhelming the pulmonary vesicles by the rapid development of tuberculization, it kills the patient at the outset through the occurrence of softening. The frequency of this disease is very great. Endemic and hereditary at the same time, it seizes both on those who are predisposed to it and on those whose constitutions might have been supposed to guarantee them against its attacks." (p. 117.)

The local prevalence of phthisis in this district appears to have a direct relation to the dryness of the soil; patients in whom the disease is making rapid progress whilst residing at Campeachy, which is built upon a dry, calcareous soil, passing into a chronic condition of the affection when transferred to the humid and woody neighbourhood of Valladolid.

It may, perhaps, be doubted whether a notable proportion of the mortality which M. Jourdanet ascribes to the ravages of acute phthisis in Yucatan is not rather due to what would be called pneumonia with us; though the strongly inflammatory type which diseases of all kinds assume in this district must make it difficult to draw there even the very indistinct line of demarcation which European pathologists often recognise as separating the one disease from the other. We must also probably attribute to the dryness of the soil the excessive frequency of inflammatory affections of the intestinal tube in Yucatan, amongst which dysentery, often in an epidemic form, is especially fatal.

In Tabasco, on the other hand, inflammatory diseases are so rare that the liver, the organ against which the morbific influences of the district are chiefly directed, rarely exhibits any indication of inflammation; and still more rarely does hepatitis, when it does occur, terminate in abscess. But the most remarkable evidence in illustration of the non-inflammatory character of disease in Tabasco is to be found in the almost complete immunity from pneumonia and phthisis which that State enjoys. During a six months' residence in it, M. Jourdanet met with only one consumptive person, and he was a Frenchman who had laboured under the affection before arriving in it. Indeed, it would appear that the only cases of phthisis which are to be found in Tabasco are those which are sent there from Yucatan and other parts of Mexico, for the sake of the favourable influence upon the disease which its climate is known to exert.

To what, then, it may be asked, is the freedom from inflammatory affections and from phthisis which this district enjoys owing? and how is it that two regions which are in such close juxtaposition, and whose temperature is so nearly identical, should exhibit such differences in their influence upon the human constitution? To answer this question, our author recurs to the well-known views of M. Boudin on the subject of the antagonism of phthisis and intermittent fever, which have also been entertained by some authorities in our own country. As we have already stated, Tabasco is the constant seat of intermittent fever in some of its worst forms, and the intensity of the miasmatic element is so great as to stamp with its peculiar mark every organ and function of the economy which is susceptible of its influence. How all-prevailing that influence is may be read in the air of suffering which is so common to its inhabitants; in their pale and sickly countenances and languid gait; in the general appearance of depression which they present; and, by the physician, in the hypertrophy of their abdominal viscera which in most cases may be without difficulty diagnosed. It is to this influence, or rather to the conditions which give rise to it, that M. Jourdanet attributes the freedom from phthisis and inflammatory affections by which Tabasco is distinguished; and the explanation which he offers of their relation to one another is so ingenious as to deserve consideration.

Starting from the fact, so well known to the inhabitants of miasmatic districts, and those who have had much experience as travellers in them, that the paludal poison exercises its influence chiefly, if not exclusively, during the night, M. Jourdanet proceeds to inquire to what this limitation of activity is due? He rejects, as being open to objections which render it untenable, the view that this phenomenon arises from the condensation of atmospheric vapour which takes place during the colder hours of the night, and which carries down to the surface of the ground with it the malaria that has been dissipated into the higher strata of air during the day by the sun; and he attributes its production to the rapid development during the night of organic elements which takes place during the absence of the solar rays and of the oxydizing agencies (ozone?) by which their destruction is effected during the day. He believes that by this means the atmosphere of miasmatic localities becomes highly charged during the night, and remains so to a lesser extent by day, with emanations which, from their origin in decaying vegetable matter, are probably largely impregnated with hydro-carbonaceous compounds that have a strong affinity for oxygen. These emanations, thus impregnated, he supposes to be carried into the lungs with the inspired air, and to be absorbed into the blood, which would thus become loaded with the oxydizable material of which they consist.

We have not space to detail the ingenious reasons which M. Jourdanet adduces in support of his view that the destruction by oxydation of this hydro-carbonaceous pabulum takes place in the abdominal glands, but more especially in the liver and spleen, whose gradual hypertrophy he believes to result from the increased work thus thrown upon them. We will only remark that although they must at present be regarded as little more than hypothetical, there is nothing in them that is inconsistent with well-established physiological knowledge, but much to which recent teachings give considerable support. For our present purpose all that it is necessary to point out is the evident influence which the presence of such a readily oxydizable element in the blood as that the existence of which M. Jourdanet assumes, would have upon the progress of a disease which is characterized by such a proclivity to oxydation as phthisis is, and how it would retard the rapid and destructive disorganization of the tissues in which the force of the disease consists. Nor need we remind our readers that it is upon the assumed influence in retarding oxygenation of another class of hydro-carbonaceous compounds, the oils and fats, that the theory of the modern treatment of phthisis in great part rests. Whether the great prevalence of that affection in Yucatan is due merely, as M. Jourdanet suggests, to the increased amount of oxygen in its atmosphere, or even to an augmentation in its activity, is, we think, open to doubt; but that it is excessively frequent there, that it is scarcely known in the neighbouring state of Tabasco, and that the cause of its absence is in some way connected with the presence of marsh miasm in its most active form, are facts of which there can be none whatever. The bearing of these facts, as well as of any explanations which may be offered concerning them, is of the highest practical importance both in a pathological and a therapeutical point of view. A visit to the reeking savannahs of Tabasco, especially in the present unsettled state of Mexican affairs, may be neither a very safe nor a very agreeable mode of treatment for a consumptive patient, but if it can be only clearly established that an atmosphere such as that by which this district is distinguished exercises a marked retarding influence upon the progress of so ruthless and intractable a malady, human ingenuity will not long be wanting in discovering a locality where the remedy can be applied in a more modified and consequently in a more manageable form.

Before joining our author in his ascent towards the snowy altitudes of the interior, we must linger for a few instants by the sea-side to record the views which his experience leads him to propound of the fevers which are more or less endemic there. In the first place, he corroborates the opinion maintained by Dr. Baikie, and by most recent observers, that however different in symptoms and in fatality some of the best marked forms of tropical fever may appear to be, they are at bottom only varying phases of one morbid influence, arising from one set of causes, but manifesting its activity in different directions. Whether it be the bilious fever of the East Indies and the tropical ports of the Pacific, the dreaded *yellow jack*, or *el vomito* of the Mexican Gulf, or the milder gastric fever which prevails in the same locality—all, although at first sight specific and independent in their nature, are but modifications of the same type of disease, differing only in the intensity of its development. The best proof of this fact is that an attack of either of these forms of fever in most cases guarantees against a subsequent visit of the others. It would, indeed, seem, from some statements of M. Jourdanet's, that a person who has suffered from typhoïd fever in Europe may to some extent calculate upon an immunity from yellow fever in Mexico. At any rate, he strongly insists upon the protection from yellow fever which is conferred by the mild fever of acclimatization by which all immigrants are attacked soon after their arrival in the country, unless they should have the misfortune to be previously exposed to the more violent or epidemic forms of the disease. Of such practical importance does he consider this fact, that he recommends those who intend to take up their residence in a locality in which yellow fever prevails, such as Vera Cruz or Havanah, if possible to reside for some time before in some less frequented port where the small number of strangers prevents the epidemic element from acquiring any intensity. It is to this latter cause, and to the conditions to which it gives birth, that yellow fever, like all ochletic diseases, owes its greatest virulence, its progress under these circumstances aptly realizing the poet's phrase, *vires acquirit eundo.* M. Jourdanet even goes so far as to assert his conviction that any affection of a miasmatic origin which attacks a stranger after landing in the regions in which yellow fever reigns is a preservative against that malady. As to the treatment of yellow fever, M. Jourdanet gives us no new light. Indeed, in these days of enlightenment, in which the lancet, whether amongst its theoretical adherents or its uncompromising opponents, has equally fallen into neglect, it sounds somewhat strange to hear even the moderate praise which M. Jourdanet bestows upon its use in the treatment of fever. It is fortunate for themselves that our *confrères* in Central America have not yet embarked in the discussion of that *vexata quæstio*, the change of type in inflammation; and we may hope that it is no less so for their patients.

We must now hasten onwards to the description which M. Jourdanet gives of the physical influence of that portion of the Mexican Republic in which, for the reasons already mentioned, our readers will be more especially interested—the elevated table-lands of the interior.

The height of the Mexican plateau, the second which the traveller encounters during his upward journey from the coast, that of Anahuac being the first, may be better appreciated by a comparison with some of the most elevated table-lands of the Old World. The highest plateaux of Switzerland or Bavaria do not exceed 1800 feet; the highest in Castille is 2200 feet; and that of Auvergne, in France, is only 1087 feet above the level of the sea. And even in Asia, where the alternations of the earth's surface are exhibited on so much grander a scale than in Europe, the highest table-lands are considerably lower than the plain of Mexico. The extent and the uniformity of this great plain are best indicated by the fact that, though it is situated in the heart of some of the largest mountains in the world, it is so level that carriages may traverse the road which passes over it from Mexico to Santa Fé, a distance of nearly fifteen hundred miles, without experiencing the slightest inconvenience or hindrance. Although in nearly the same parallel of latitude as Vera Cruz, and in the possession of a sky and sun which for brilliancy are scarcely equalled by those of any other country, the temperature of the city of Mexico in the warmest summer does not exceed 80° in the shade, and so small are the variations to which it is liable that its mean temperature all the year round is 65° (about that of Naples), whilst its winters are so mild that snow rarely or never falls. Yet, with all this apparent geniality of climate, the sensation of cold in Mexico, when out of the direct rays of the sun, is constant and penetrating—a fact which shows us the fallacy of drawing any inference as to the suitability of a given climate for residence from the mere records of the thermometer. The great rarefaction of the atmosphere which takes place at this elevation (its density being about one-fourth less than at the level of the sea), and its increased capacity for heat, and for the absorption of water from the surface of the body by evaporation, are the main causes of the rapid refrigeration which persons who are not actively employed always experience in Mexico. It may, therefore, be readily imagined that all the evils which attend an unguarded exposure in such a climate as this are here felt in full force; and it is especially upon the infant portion of the population that their weight falls. The surface of their small bodies is disproportionately large when compared with the activity of their heat-producing functions, or with the bodies of adults, and when they are sent out into the air, as is too often the case, with a scantiness of clothing which is suggested by the seducing indications of the thermometer,* they fall ready victims to the keenness of the cold. Hence the frequency and severity of the bronchitic and pneumonic attacks by which they are decimated, and the more uniform complication of scarlatina with dropsy than is the case at a lower elevation. Amongst adults, also, bronchitis, pneumonia, pleurisy, and acute rheumatism are exceedingly prevalent from the operation of the same cause, as also a form of abdominal affection which M. Jourdanet describes as peritonitis, but which, from the symptoms accompanying it, looks much more like a severe kind of abdominal neuralgia—a supposition to which the general pathology of the district lends much support. It is probably rather to the extreme dryness of the atmosphere than to its low temperature that we must ascribe a peculiar affection of the throat which appears to be very common in Mexico, particularly amongst those who have lately arrived on the heights, consisting in great irritation of the fauces, accompanied with dryness of the parts, which soon leads to copious and continuous secretion. So common is this affection, that it almost bars European operatic singers from exercising their profession at Mexico until they have become so far acclimatized as to be relieved from the irritation of the throat engendered on their first arrival.

But the prejudicial influence of this refrigerating tendency of the Mexican climate is not confined to the production of acute disease. The general stagnation of the circulation to which it predisposes in persons who are not compelled by their duties to take active exercise,

* The author states that he has found in the month of January the difference between the temperature indicated by a thermometer suspended in his room, with the window open, and in the sun just outside the window, to be as much as 95° Fahrenheit! In such a climate as this the sun is even more essential than it is with us: and it is easy to see how the brilliancy of its beams and the life-giving warmth which it diffuses should have led the ancient inhabitants of Mexico to adopt a worship which was so especially congenial to the conditions by which they were surrounded.

leads to a state of apathy and unwillingness for exertion which help to maintain the original condition from which this state itself arises. In this respect the effect of atmospheric cold is so different on the plain to what it is at a lower level in Europe or other parts of the world, where, as a rule, it rather incites to than represses bodily activity, that it is worth while investigating the circumstances which induce it. And to do this will involve the consideration of one of the conditions of life in elevated regions to which we have not hitherto alluded.

It needs no demonstration to prove that a man who breathes air which, like that of the Mexican plateau, has one-fourth less density than that at the level of the sea, must, unless the rapidity of his respirations be proportionately increased, inhale one-fourth less air, and consequently one-fourth less oxygen, than he would do if its density were unaffected. The condition in which such a man lives is just that of an animal confined under the bell-glass of a partially exhausted receiver. It is a common idea, even amongst scientific authorities, derived probably from the fallacious results of experimentation on small animals under the exceptional conditions just referred to, that the deficiency in bulk of the air respired by the inhabitants of elevated regions is compensated by an increase in the frequency with which the act of breathing is performed. In opposition to this notion, M. Jourdanet asserts, from repeated observations both on himself and others, that the frequency of respiration on the plain of Mexico in a state of bodily repose is less than it is at the level of the sea, and that the act of sighing, by which defective respiration is characterized, is so constant there as to become habitual. We can, therefore, readily understand why the low temperature of these high regions fails to call forth the activity which is necessary to maintain the heat of the body. For, the general depression of the vital functions which the diminished respiration and the decreased oxygenation of the blood to which it gives rise, produces, reacts upon the muscular and nervous elements of the respiratory organs, as well as upon those of the body at large, and induces throughout the whole economy a tendency to repose and an unwillingness for exertion, which are thoroughly characteristic of the inhabitants of Upper Mexico. Hence the difference both in physical and political peculiarities which exists between them and those of the coast. The former are quiet in their manner, not easily excited, and loving repose to an extent which the constant bloodshed and strife in which they are involved would hardly render credible. With them the intensity of love and hatred are alike rare; or, if they hate at all, they conceal their hatred under the garb of a politeness which is less the result of studied cultivation than of the physical influences by which their temperament is determined. The Mexican of the coast, on the other hand, is possessed of a vivacity and an impulsiveness of disposition which brings his character into close resemblance with that of the modern Italian, and are the outgrowth of the intensified energy which a high temperature and an active respiration combine to excite in his system. The maritime State of Yucatan has probably given to the service of the Republic a larger number of remarkable men than any other in the Confederation. It is from the white race of the Gulf Coast that the turbulent element which is the source of the perpetual struggles by which the country is distracted is chiefly recruited. The large majority of the inhabitants of Upper Mexico are, as we have already stated, both from taste and by constitutional tendencies, lovers of peace; and it is only their apathy and indolence that has made them the victims of the ambitious intrigues by which a few active schemers are incessantly seeking to raise themselves to power.

It is, at first sight, somewhat singular that this imperfection of respiration is confined to the white, and more recent population of the plateau. The native Indian, whom the lapse of a lengthened residence in these regions has acclimatized, and whose bodily conformation appears to have gradually adapted itself to the abnormal conditions in which it is placed, exhibits an amplitude of chest which is altogether out of proportion to his diminutive height. In him, nature has compensated the diminished oxydation which the loosened density of the atmosphere involves, not by augmenting the frequency with which the act of respiration is performed, but by increasing the size of the respiratory apparatus itself. Hence he is enabled, like the *cargadores* of Peru, to carry burdens for a distance and at a rate which would be beyond the endurance of the white man. It is a common thing for him to undertake journeys of forty or forty-five miles a day, heavily laden, in the pursuit of the wretched subsistence which this occupation affords him. And yet, notwithstanding, he generally attains to a very advanced age, instances of Indians who have passed their one hundredth year being far from uncommon. Whether or not these facts give any support to the so-called "development theory" we will not now stop to inquire; they certainly show that nature will go some way towards accommodating the animal economy to exceptional circumstances in which it may happen to be placed, but they also indicate that the period of time which she demands for the production of a variation in form, which, compared with some of those that distinguish the different races of man, must be allowed to be trivial, is by no means inconsiderable.

The contrast which exists in this respect between the indigenes and the imported inhabitants of the plateau throws some light upon a question which is now attracting much interest both in political and scientific circles—viz., to what extent acclimatization of a race foreign to a given climate can be carried? Our readers are doubtless aware that the theory has been lately circulated* that acclimatization, except within restricted limits, is never perfect; that the tendency of transplanted races is always to degenerate; and therefore, that colonization, where carried on in a country that differs materially in climate from that inhabited by the parent stock, must end in failure. There can be little doubt that one of the strongest arguments in favour of this

* These views are maintained by Dr. Hunt, amongst others, in a paper read by him before the Ethnological Society, Feb. 4th, 1862, and by Dr. Moore, in his work on the Climate of India, recently noticed in our columns.

theory is to be found in the history of the Spanish American colonies, though it must not be forgotten that much of the degeneration, both political and social, which most of these exhibit, is due to influences totally unconnected with that of climate, and especially to intermarriage of the colonists with an inferior race. But, irrespective of this consideration, there are strong reasons for believing that even in the case of those whose fathers and forefathers for several generations back, have resided in the country, the European settler in Upper Mexico has not yet become fully adapted to the climate of these elevated regions, and that he has not yet, as it were, acquired that type of development which the circumstances by which he is surrounded appear calculated to impress upon him. The natural result of the struggle on the part of a race to maintain its position in the face of agencies which are unfavourable to its perfect development must be, for a time at least, degeneration. This is shown in the case of the fruit-trees which are cultivated on the Mexican plateau, most of which are the descendants of stocks imported from Europe. Trees which in the eastern hemisphere thrive and produce largely, under the same limits of temperature in Upper Mexico become tasteless, unproductive, and require the greatest care to bring them to maturity. The horses which, for the sake of their superior size and breeding, are imported from the United States, soon begin to fail in their wind, become invalided, and eventually die of rheumatism or pleurisy. The want of any cultivation by the Mexican administration of a reliable system of statistics, renders it impossible to speak positively as to the comparative mortality of the white and native races on the plateau. From his lengthened personal observation, however, M. Jourdanet asserts that on these great elevations the white man becomes old prematurely, and his life is comparatively shortened, though, as has been already remarked, the Indian often reaches an advanced age. It is but right, however, to state that this assertion is opposed to the conclusions of Humboldt, who thought that the balance of longevity was rather in favour of the white man.

It might naturally be expected that the conditions which exercise so marked an influence in diminishing the physiological activity of the body on the plain of Mexico would make themselves felt also in its pathological disturbances. And this is actually the case. The tendency of all diseases in these regions, of however acutely inflammatory a character they may be at the outset, is to rapidly assume an adynamic type. This is well illustrated by the aspect which the disease that has been made the *cheval de bataille* in the discussions that have recently taken place on the treatment of inflammation amongst ourselves—pneumonia—presents in these latitudes. Whilst at a comparatively short distance from these great elevations, on the Gulf Coast, for instance, pneumonia always assumes a markedly sthenic character, passes regularly through its three ordinarily described stages, and admits of the most active treatment without unsatisfactory results; upon the heights, on the other hand, the inflammatory phase is so transitory as to be often entirely absent, profound prostration setting in at a very early period of the disease, and alcohol and quinine being the main remedies upon which the practitioner can place any reliance. It would be hardly possible, we think, to have a more striking proof than this of how greatly the resistance of the organism to disease, and consequently, how much the character of disease itself, depends upon the varying influence of physical conditions to which too little attention has been paid by those who have been occupied in so fiercely discussing the possible occurrence of a change of type as an element in causing the acknowledged modification of treatment which inflammatory affections have undergone within the last half century. Here we find that the mere ascent of 7000 feet is sufficient to convert an active and sthenic disease into a profoundly adynamic one. So completely is this the case, that the medical men who come to settle in the city of Mexico after having practised for some time on the coast, can hardly realize at first that the pneumonia with which they have so frequently to deal is the same affection which they have been in the habit of treating so heroically lower down, and only abandon the lancet and tartar emetic, in which they have been accustomed to place their chief trust, when they find from experience to what fatal results these remedies conduct them.

What has been said of pneumonia applies equally to other forms of inflammation: they all assume, even from their origin, more or less of a typhoïd character. But genuine typhoïd fever, such as we know it in England and France, is comparatively rare in Upper Mexico, the most prevalent kind of fever there, which sometimes is extensively epidemic and fatal, being more nearly allied to what is called typhus than to any other recognised variety amongst us. It may not perhaps entirely square with the views of modern sanitary science to learn that the neglect of sanitary arrangements appears to exercise but a limited influence in originating the epidemics of fever by which the plain of Mexico is frequently ravaged. The town of Puebla, which is constantly visited by them, is described by M. Jourdanet as one of the cleanest in the world. Their prevalence is in his opinion mainly due to the general debilitating effect of the climate, and to the readiness with which on this account all ochletic diseases here assume an epidemic form. It was probably to the same cause that the enormous mortality of that singular disease, the Matlazahuatl, by which in past years the native Indians were devastated, is to be attributed. The ravages of this terrible malady, of which Torquemada affirms that no less than 2,000,000 Indians died in the year 1576, and which M. Jourdanet believes to have been an epidemic form of typhus, were committed not in populous towns, or in the haunts of yellow fever on the coast, but over the immense plains of Upper Mexico, and amidst the physical conditions which are usually considered essential to perfect hygiene.

We should, however, be in error if we were to attribute to the diminished density of the atmosphere alone the deficient oxydation upon which both the physiological and pathological peculiarities of the Mexican constitution are based. It is probable that no inconsiderable portion of it is due to the extreme dryness of the atmosphere with which that diminished density goes hand in hand. When we consider how essential a certain amount of moisture is to the proper performance of the process of respiratory endosmosis, we shall easily understand how that function must be impeded by any cause which reduces the moisture in the air below the amount which is necessary for its healthy discharge. Two facts mentioned by M. Jourdanet help to corroborate this supposition. In speaking of the change in the pathology of disease which occurs in Mexico annually on the advent of the rainy season, he remarks—

"The contrast is then the more striking, inasmuch as we pass from the most marked state of insalubrity to the best manifestations of public health. Bodily fatigue, respiratory embarrassment, vertigo, and dyspepsia give place to muscular activity, easy respiration, and to soundness of the nervous and digestive functions. The diseases of spring disappear, or lose as if by enchantment their epidemic character, and all for which this, the finest season of the year, can be blamed is the occurrence of a few cases of dysentery which appear during its latter days."

An equally strong confirmation is found in the fact that if we ascend the mountains which surround the Mexican plateau, and get within the limits of the condensed vapour which always hangs about the upper strata of mountainous regions, the anæmia which is so characteristic an element in the physiognomy of the natives of the plain will be found to have to a very considerable extent disappeared, and the pathology of the inhabitants to have acquired a proportionately active type. Whilst speaking of anæmia, we may mention that M. Jourdanet states that the anæmia of the plateau, unlike the affection of lower altitudes, is quite *un*amenable to iron, and yields only to quinine.

One of the most interesting chapters in M. Jourdanet's work is that in which he describes the influence of the atmosphere of these high regions upon the development and progress of phthisis. So many contradictory statements have been published with regard to the comparative influence of increased or diminished atmospheric pressure on this disease, and such diverse methods of treatment have been founded upon the theories to which they have given birth, that we are glad to avail ourselves of the opportunity which M. Jourdanet's experience offers, for testing the operation of one of these two opposite agencies upon a more extended scale than the conditions of any European country will admit. In brief, the evidence of M. Jourdanet goes to show that pulmonary phthisis is excessively rare in Upper Mexico. In Puebla, where, as has been remarked, an admirable sanitary *régime* prevails, it is almost unknown, and in the city of Mexico, where it is less rare, it is almost exclusively confined to the lower classes, whose habits, diet, and dwellings peculiarly predispose them to its attacks. But the proof of the antagonistic influence which the climate exercises upon the development of the disease does not rest only upon the freedom from it which the native-born inhabitants enjoy. M. Jourdanet records numerous cases which have fallen under his own observation, and also brings the testimony of other medical men to show that strangers in whom phthisis has already decidedly established itself experience, from the moment of taking up their residence on the plateau, an arrest of the affection, which, if not issuing in a restoration to perfect health, is at least very nearly equivalent to it. "I have the conviction," says M. Jourdanet, "which is shared by other worthy fellow-practitioners of mine, that the great majority of young persons in whom there is an hereditary predisposition to phthisis would escape the disease by a residence in Mexico." Without pronouncing positively as to the period during which it would be desirable to reside there before a return to the patient's native country could be considered safe, he inclines to the belief that a stay of three years is the shortest period which would be attended by any positive benefit.

Although we cannot endorse entirely the theory which M. Jourdanet proposes, as to the immunity from phthisis which these elevated regions enjoy being due to the imperfect oxydating properties of their atmosphere, we must admit that there is much to be said in its favour. There can be little doubt that whether the deposit of tubercle be in all, or even in the majority of cases, an inflammatory process or not, its maturation and disorganization are stages of its existence in which inflammation plays a very active part. That the non-stimulating properties of an atmosphere in which the proportion of oxygen is so notably diminished as in that of the plain of Mexico, should have considerable effect in checking the progress of these inflammatory processes is highly probable; but we are inclined to think that the dryness of the atmosphere, and the complete change in all the physical conditions of life which a phthisical patient arriving in these elevated plains from a low-lying locality would experience, must exercise no small influence in producing a general amelioration. At any rate, we can honestly say, that we know of no district, whether in the eastern or western hemisphere, which offers a better prospect of cure to the consumptive invalid than does Upper Mexico, if M. Jourdanet's statements are entitled to anything like a fair amount of credit, and we see no reason whatever for doubting them. The only drawback to putting them to a practical test is the unfortunate uncertainty both in respect of life and purse which at present prevails in that country. For a phthisical patient to be compelled to ride several hours in the *coupé* of a diligence on a sharp frosty night, in a state of nudity, to which the successive attacks of brigands had reduced him, as was the ill-luck of a recent traveller in these parts, would assuredly do more to expedite the termination of his malady than many months' residence there could do to arrest it. But whenever the long arm of a strong Government shall clear the roads of the banditti which now infests them, and render travelling even tolerably safe, we hope that the high claims which M. Jourdanet puts forward in behalf of the Mexican plateau, as a resort for those who are affected with phthisis, will have a speedy opportunity of being thoroughly investigated.

We had intended to refer to numerous other questions of interest which find a place in M. Jourdanet's pages, but we have already so exhausted the limits which we originally proposed to ourselves, that we cannot now do so. If the summary which we have given of some of the more important portions of his work should lead them to invest in it for their own perusal, we can promise them that they will have little reason to regret it. Nor is it one of the least of M. Jourdanet's recommendations, that his volume is characterized by a terseness and elegance of style which has added much to the pleasure we have received in reading it.

REVIEW XI.

The Renewal of Life: Clinical Lectures Illustrative of a Restorative System of Medicine given at St. Mary's Hospital. By THOMAS K. CHAMBERS, M.D., F.R.C.P., Physician to St. Mary's Hospital, &c. —*London*, 1863. pp. 420.

THIS small octavo volume is assuredly a sign of the times. Twenty years ago, had it been conceivable, it would not have been conceived; had it been possible, it would never have happened. Had its author been a living fact, had the manuscript been revised and ready, the type would not have been forthcoming. The two introductory chapters would have insured its rejection by the publisher's reader. Of questionable shape and title, its "introduction" a metaphysical puzzle, its doctrine one of denial and denunciation, the 'Renewal of Life' would have scared the legitimate old world dealer in medical literature as a foreshadowing of chaos in the physic to come.

> "Aliquid monstri alunt,—id quoniem nemine obstrude potest,
> Itur ad me."

And in refusing the risk with the possible scandal of a book like this, the Churchill of 1840 would have been in his generation wise. But in the literature as in the practice of physic, there has been change since then, and ours is the gain. A clever, thoughtful book, such as that before us, however much out of rule with settled opinion, however defiant in tone, if only readable, is now-a-days safe to be read. In the philosophy of disease, solemnity of phrase is no longer a condition of sale and perusal. By the critic of published essay, lecture, or dissertation, count is held of the line and the letter as of the thought that goes with them. If the author's purpose be clear, earnest, and strong, it is expected that he shall take the trouble to make it known in words and sentences that match with his meaning and with each other. There is a fair range held free for illustration by analogy, an indulgence even for fancy, if it lend help to the facts; and the author is not liked the less for a casual impertinence of style or suggestion. Even in the narrative of cases the most complicated and serious, there may be a certain grace of arrangement with animation of style, and this without prejudice to their use by the working practitioner in the service of every-day reference.

The book before us, to begin with its outside, might be anybody's book, as it meets the eye on counter or library-table. On its plain stamped chocolate binding there is no register, by date of time or place, of publication, no gilded blazon of the author's name: a small diagonal scroll alone is lettered with the mystic inscription of 'The Renewal of Life.' This contrived effect of announcement is novel, ingenious, sensational—successful it may have been; but is it professional? 'The Renewal of Life,' without phrase or condition! A challenge direct to the curiosity of all who live by their means and like to be alive. Who would not long, if only for a peep into the pages of a volume in which the great secret lies open for special and immediate use? Who would not take it home with him if he could? Renewal of Life! Words, if any, of awe and fascination! The world's one fixed wish, the mystery of all time to all inquirers in their widest range of thought, sentiment, or imagination! No wonder that the first edition passed away in two months of the autumn of 1862. And yet to many, the mystic and imaginative among its purchasers, what a vexed surprise, what aggravation of disappointment, when the leaves were opened, and they found themselves in presence of a "doctor's book." We confess to mirth and mischief more than is becoming, while mesmerist, spirit-rapper, electro-biologist, and other sectional mystics, gather round our fancy's lure, and, watching with closed eye, we send them fluttering into the decoy, where lurks the clinical professor of St. Mary's with physic at his side. To the illustrious and voluminous "strange story" teller, this titular equivocation must have seemed personal in its aim of annoyance. We looked for a dedication of this little volume to the ex-Secretary for the Colonies. Let him consider that by its endorsement of 'The Renewal of Life' the compliment is virtually paid.

There is, again we use the phrase—not ours alone—there is possible scandal to the profession in this the first page of a clever, useful, practical volume, by one of its most accomplished and responsible members. Already, and for some months past, in the rush of votaries to his shrine (the only one) of "restorative medicine," there must have been cause for painful misgiving to its presiding minister as to the nature of the impulse that brought them there. Life renewed under prescription by a "restoratist" physician from a "restoratist pharmacopœia," and that physician not the administering patentee of an universal specific, but a London-hospital physician, a Court physician, and a Fellow of the Royal College of Physicians of all England! What a paradise of hope to the battered veteran of life's long struggle, to the languishing youth of both sexes, to the wrinkled jade of fashion, to the debauched elders of the mess-room and the London clubs! Who would consult with a practitioner of ordinary designation, a mere common-place M.D., or an M.R.C.P., when he might with our author exchange his guinea for the bonus of a "renovated existence"? With the cauldron of youth, even the fleece of gold! There are fees that smell. Again, as we turn the leaves in front of every page from preface to index, our password through the volume is no other than its title, 'The Renewal of Life.' Decidedly a scandal! We force it back upon the author—it has been unduly obtruded upon us. We tell him it is not safe to hold.

As guessed from its outside, this volume might have been a bundle of tracts for the Tabernacle. It is time for our readers to know that it is in truth an assortment of bedside teachings to the young gentlemen in attendance on the medical practice of Dr. Chambers in the wards of the Paddington Hospital. Of twenty-nine chapters enumerated in the list of contents, twenty-seven are strictly professional; well calculated in their present form of publication to bring credit to the physician whose practice they illustrate, and to the school at which, as clinical lectures, they were first made available to his pupils. They afford abundant and most gratifying proof of the variety and extent of remedial means which are placed at the disposal of the medical officers of our large London hospitals, as of the thoughtful care and refined humanity which he who knows how to use them can make known in their use. Baths, diet, nursing, bedding, medicines (in many cases of unusual character and cost), are employed by Dr. Chambers without hesitation or stint for the relief of all who may need them. So far it is well that his book is made light for the general reader, and that it is endorsed with a fanciful title. Allowance made for a certain affectation of manner, rather amiable than otherwise, and a somewhat feminine freedom of narrative, Dr. Chambers "reads well" as a clinical lecturer in the oral treatise before us. His cases are well chosen and well told. They are sufficiently distinguished one from the other, and never overlay the principle which they are used to illustrate. He carefully inquires into the antecedents of each case, and considers it in all its symptoms and circumstances as a whole, providing, as far as in him lies, for all accidents, contingencies, and mixed results. Especially he individualizes his patients, and thus peoples his wards not merely with cases, but with men and women.

The ludicrous pretension of the title-page would have escaped much of the rebuke which it has encountered from the profession, had it not been followed by a preface and two introductory chapters conceived in the same spirit, and pitched on the same key. In October, 1862, Dr. Chambers proclaims a "system of therapeutics, which *must* daily become more and more the guide to practice, and which he makes bold to call 'Restorative.'" The absolute "must," accordingly works its daily will, and is not many days about it. In December of the same year, its triumph already achieved by the early call for a second edition, it subsides, with complacent security, in a preface of two lines, into the "encouragement of a hope." Of the tone and manner in which the author addresses himself to his lofty self-imposed task, there is sample and to spare in the heading prefixed to the first "Introductory Chapter":—

"THEORIES OF MEDICINE AT THE BEDSIDE.

"Necessity for a consistent theory of therapeutics—Examination of prevalent theories—Allopathy—Homœopathy—Evacuation—Counter-irritation—Stimulation, with tests of their applicability—Eclecticism and expectant medicine mere protests—The inherent error of these medical theories is the attributing a positive existence to disease—Disease truly a negation or deficiency of life—On this is grounded a proposal for a system of restorative medicine, and a consistent view of the aims of medical treatment."

A stirring paragraph this, the opening notice of the book! We commend all the headings of all the chapters to the special attention of our readers. There is spice in their pith. They are not of the conventional order. Here, at the close of the Introductory Chapter, in type as various as that which tests the optician's myopic scale (we print as Dr. Chambers has printed), is the manifesto of the resulting doctrine—

"Each mode of erring from health is reared up into an entity instead of an abstraction, a separate foe requiring special weapons for his discomfiture. I maintain, that unprejudiced observation can show this to be an utterly false view; that it has led of old, and cannot but lead in future, to unsafe practice if followed to its logical consequences, that all the good practice which may have sprung up at various times has arisen in opposition to it. The main hope for bringing therapeutics up to the level of modern science lies in discarding at once and for ever this traditional notion that disease is a positive existence. Then will the way be open for those true guiding principles which an advancing knowledge of nature is forcing upon us, that DISEASE is in all cases not a *positive existence*, but a *negation;* not a new *excess* of action, but a DEFICIENCY; not a *manifestation of life*, but PARTIAL DEATH. And therefore that the BUSINESS OF THE PHYSICIAN is, directly or indirectly, not *to take away* material, but to ADD; not to *diminish function*, but TO GIVE IT PLAY; not *to weaken life*, but TO RENEW LIFE. These are the principles of RESTORATIVE MEDICINE."

And is it so? Can it be that this accomplished physician, who has looked, as it were, into the eyes of the profession for twenty years past, and this under the lights of two great London hospitals, is it possible that Dr. Chambers, now of St. Mary's, late with St. George, should undertake to dazzle the retina, physical and intellectual, of his comrades, young and old, with a parade of spurious abstractions? There is one alternative—we receive it with no disrespect and without hesitation. In perplexing his readers, the clinical professor has mystified himself. This is not wantonly said. We have read the two "Introductory Chapters" as we have read the entire book, line by line, through and through; we have pondered over the hard bits, we have collated and speculated, and we have come to the conclusion that in his treatise or clinical lecture on "Death, Life, Disease, and Cure," the author of the 'Renewal of Life' did not know his own meaning. There is the shadow of an axiom in the dogma of "Disease in all cases a Deficiency," but it escapes the grasp of practical thought, and vanishes the moment it is questioned. The *bête noire* of an otherwise instructive and entertaining volume, we are weary of it, angry with it, and would gladly forget it, but in our respect for the author we must see where it leads him, and watch him through his trimestral editions. Dr. Chambers is a realist, with a passion for the abstract. His favours are not reciprocated; but he cannot keep away from his shadowy mistress, and he invariably suffers for his presumption. No impartial bystander would advise him to persevere in his suit. Under the dogma of "Disease in all cases a Deficiency," he lies entangled in a net of transcendental physics, with strivings of impossible utterance. In due time and soon he will break through, by effort of his own free will. It is too early as yet to offer help, but we call to him to struggle hard, while we keep count of the broken meshes. Our author, it has been seen, deals largely in the dogma; he is, moreover, plentifully stocked with definition of his own manufacture, so that the material for fabrication is always on hand. Finding that we could make nothing of the monster dogma in p. 14, from our apprehension of it in the plain received sense of its abstract terms, we hit upon the thought of looking out life, death, and disease in the succursal dictionary with which Dr. Chambers has supplied the students of his 'Renewal of Life;' and then again, attacking the mystic paragraph with the words in their paraphrastic sense. In p. 20, we read in capitals, that

"LIFE IS RENEWAL."

A little mystified, we test life in its restorative sense by the titular motto, "Renewal of Life," ever convenient at the top of the page; the result is, that for

"RENEWAL OF LIFE,"

we read

"RENEWAL OF RENEWAL."

Will the author suffer this corrected reading of the little diagonal scroll in the next edition of his revised clinical lectures? It would be nonsense, but it would be a signal improvement. In p. 19 of the second introductory, it is declared that "it is the form, or the individual, and indivisible life, which constitutes the self." So that one's life is synonymous with one's form, and both are identical with oneself. Let these synonyms take turn in the dogma, and it will be found to read most amusingly. But if so with life, how with disease? Disease is defined, positively and directly, in capitals with italics, as a "partial cessation of renewal;" 2ndly, "an arrest of renewal;" 3rdly, "a negation;" 4thly, "a deficiency;" 5thly, "a partial death;" 6thly, "Disease is that deranged balance of the vital functions which is inconvenient for the uses we put our bodies to;" 7thly, a "something less than life."

Negatively defined, "disease is not a positive existence," "it is not a new excess of action," "it is not a manifestation of life." By implication, "it is a mode of erring from health, an abstraction, not a motive cause," "not a positive existence," not "a separate end," not a peculiar manifestation of life, not a "property or substance to be overcome or taken away." Finally, it is "not an entity." As of disease, so necessarily of its symptoms, for what we apprehend in disease is entirely through what we observe of its symptoms. Thus pain, inflammation, spasm, hæmorrhage, fever, mania, symptoms one and all in their infinite variety, like the disease which they constitute, are "abstractions" and "nonentities;" and on this "true guiding principle," by this "single aim," their treatment is for the future to be directed. There is only one restoratist physician in the English College of Physicians. Would it be safe, in a consultation from which he was absent, to broach this nonentity doctrine for the relief of a grandee with the gout? Let the reader make copy of these several expressions for disease wherever that word comes in context with life and death (there occur many such passages in the volume), and we promise him the amusement, on paper, of a Highland reel. Once in movement, the three sophisticated ideas soon forego all separate privilege of place, and, under the exigencies of the dance, pass rapidly, through bewildered vision, into utter confusion of identity. Some of these verbal transpositions are under our eye, but we have not the space in which to transcribe them. Here are a few selected sentences in sample of what the author is able to do for himself, working with his own tools on his own soil: "A very advanced degree of partial death was seen not to be inconsistent with life; a less degree is not inconsistent even with active usefulness." Again, the permanent necessity for the wellbeing of the animal organism that waste should be balanced by supply, a truth recognised time out of mind by all physicians in all ages and all countries, in the phraseology of "restorative medicine" is translated thus:

"Thus there are two departments carried on simultaneously, the destructive and constructive business, and upon their harmony and completeness depends the perfection of life which we call health. Both are necessary, but the deficiency of both or either, or the preponderance of one over the other in various parts, or their deficiency in one part while others remain active, constitutes a deficiency of life—a disease."

Perhaps the most novel effect in the volume is that which is furnished by the duel of life and death which is always being fought out in any given portion of the inside lining of the body. It is in the public interest that the hypochondriac and the lady pathologist, whose talk is ever of mucous membrane and its ailments, should know for the future what they meddle with according to the "true preachings" of restorative medicine:

"Every one associates cancer and degeneration with fatality; but perhaps it is not quite such a familiar idea to see partial death in a cold in the head or relaxed throat. However you may smile at the notion, it is a true one, and I should advise your taking the next opportunity which a catarrh gives you of seeing the truth in its bearings. It is almost worth while to catch a cold on purpose, so important is the lesson, so much more valuable probably than the lecture you miss by nursing it.

"Mucous globules are a baby tissue strangled in its birth. Instead of uniting into a continuous web to clothe with epithelium the surface of the membrane, they float off helpless from deficient vitality. The business of mucous membranes is to be covered with epithelium, not to throw off mucus. And when they are doing the latter, they are so far forth in a state of diminished life.

"It is not the business of healthy mucous membranes to be covered with mucus at all, and when they are so, it is a sign of deficient life, local or general. Mucous globules consist of young epithelium, that is, of epithelium which ought to have lived, remained adherent to the basement membrane, and performed the local duties, but which has died in its youth, and become a tenacious fluid instead of a continuous solid.

"Remember that, in spite of their name, it is not the business of mucous membrane to secrete mucus. Be careful not to look upon mucous secretion as augmented life—it is, in fact, partial death."

This is what comes of tampering, by definition, with the abstract.

We leave it for the critics of fifty years hence. It is announced as the physic of the future.

There is one synonym of disease which, had it occurred to the author, would have helped him to much that he has missed in his bedside teachings of pathology and therapeutics. In "disorder," as expressed by an infinite variety of symptoms, there is no implied necessity of either excess or deficiency. Once committed to his great negation of entity in disease, the restoratist renounces all calculation of quantity, all estimate of quality, in his context and comparison of symptoms. Nonentities and abstractions can neither be measured nor sorted. The substantive term may remain, and pass current for such value as may be supposed to belong to it, but the small circulation is at once arrested. There is an end of the adjectives. A mild or malignant "nonentity," a contagious "abstraction," an eruptive "deficiency," an acute or chronic "partial death," a hypertrophic "arrest of renewal," are subtleties that no plain-dealing nosologist is as yet prepared to encounter. Thus, had he been true to his "leading principles," had he kept to his "single aim," had he held on by the staple fastenings of his "golden chain," the author of the 'Renewal' must have invented a new phraseology for the incidents of disturbed health before he could have addressed his pupils as their clinical teacher in the wards. Watchful and prudent in his line of march, the captain of the movement, as might be expected, is the first to become aware of the difficulties which he has thrown in his own way. Bold dogmatist as he is, he stands against his monster negation even in the hour of its birth. As we have seen in his second Introductory Chapter, he actually does make allotments of his great abstraction, and measures out "partial death" in definite and varying quantities. But, in the attempt, he proves his mistake. As pathologist and dialectician, he sees at once that to conquer and secure the positive by negatives alone is not within the privilege of man's capacity. "The deranged balance" cannot be weighted by "something less than life." He declines to lecture in an unknown tongue; declares for the "orderly mind;" comes to terms with "the people who talk of diseases;" borrows a popular nosology from the class-book of his pupils, and through the rest of the volume discourses to them of disease as if he believed in its existence.

Our main objection to the "prologue" of the 'Renewal' is that it proclaims what does not follow. It leads nowhere. Full of sound and fury, it means but this, The "system" does not exist. The "doctrine" is a delusion. In these ungracious truths we take but little part. Herein, the book is critic of itself. Examined page by page through its entire course of twenty-seven lectures, the 'Renewal of Life' is independent of system, unhampered by design, and entirely innocent of all consecutive purpose. It abounds in heresies. A few ingenious analogies do duty for principles, but doctrine there is none. Observations abound, wide, clear, and well-directed. Reflection is not wanting, frank, fearless, lively, and pertinent. There is a sufficiently high level, but nothing to elevate. True, there is a claim for distinction by alliance, mutual and all-pervading, between the sovereign idea of the title-page and the heads of the tributary chapters. A "golden chain," we have the author's word for it, holds together all that is worth binding of treasured restoratist thought, of scattered restorative principle; and in this catenary type of strength and beauty we are desired to recognise, through multiplicity of attachment, the cohesion and solidity of "system." We have examined this linked mainstay of medicine in its restoratist truth wherever we could get hold of it, from its double staple in the two "introductories" to its "key-stone and arched foundation." The metaphors are not our own. We have tested it, link by link, through the twenty-seven intermediate chapters. It does not bind, it does not hold. It is not all gold; indubitable pinchbeck in many of the rings. Mere hooks and eyes, some of the principal links, yet curiously wrought and most amusingly "dropped in." The chain of gold has evidently been an after-thought to the lectures, forged and beaten out since they were ready for type. It has been "let in" at random, with small care in the polish. In the lectures properly so called, there is no result of doctrine to match with the lofty promise of the two introductory chapters. The principle that "men weary for" transpires nowhere in its pages; the laws of therapeutical action are uncodified to the end of the volume. Of the many who denounce the 'Renewal' for its title, some few say what they can of it that is civil. Because it is queer, they call it original. But in truth our restoratist author is very seldom, and not overmuch, original. Like certain dealers in duplicate art in the London thoroughfare, he is a refresher of other men's faces, and exhibits curiosity in store; but he does not paint his own pictures, and never invents. His originality, such as it is, is not of his own seeking. It lies in his way, and he finds it. To all reflecting men it has ever been an easy truth that power and material are essential for the growth, maintenance, and repair of living structure in its functional variety, and never more so than under the damage and disturbance of disease. Our author is a shrewd observer of the times, and does good service to himself and to the public in again proclaiming this patent truth; the more so as he words it in a way likely to arrest attention from those whom it is necessary to captivate as well as convince. It comes in just now on the top of the tide. It is the fashion in the physic of the day. On the old-fashioned principle of "balancing income with expenditure," "adjusting supply to waste," and so on, Dr. Chambers is a "restoratist physician" and "renewer of life;" but only to the same extent with any plain, shrewd veteran of the village surgery, whose one rule with his twenty patients is to "set them up" and "keep them going."

Four-and-thirty years ago, in certain lectures delivered in the theatre of the London College of Physicians, it was argued, not without care and earnestness, that "inflammation" is essentially the same with "nutrition" in the wide sense of the terms—altered, imperfect, perverted, hurried nutrition as it might be, but still nutrition, vital, constant, and self-regulating; that much—very much, of inflammation is salutary, preservative, essential for the life of the structure inflamed; for the salvation, by recovery, of the patient. It was, moreover, insisted on, that pus, the educt of inflammation, was not in any case a new strange principle in the business of the system, but an alteration by excess or deficiency of the normal constituents, one or more, of the blood. Inflammation in many, if not in most cases, was, under circumstances, to be suffered, encouraged within a limit, checked if exceeding it ("conciliated" was the phrase), and brought under guidance to its self-appointed end.

This teaching was repeated till men and boys wearied of it in the wards of St. George's Hospital, that the last evidence of failing nutrition in the exhaustion of chronic disease is a pleuritis, a pericarditis, a peritonitis, an enteritis; that in such case to bleed and to starve is to give the *coup de grace* to the patient, and that the rule of respect for the blood, in quantity as in quality, applies to all degrees as to the extreme measure of inflammation or altered nutrition. "Respect hæmorrhage—take off your hat to inflammation," were the familiar versions of two clinical axioms that were perpetually kept before the pupil who condescended to think. "Bleed, if bleeding be expedient, to remove weight, to relieve the great centres from pressure; bleed to get action, to diffuse it, to carry it out and through—not to depress it —not to cut it short. Bleed on mechanical, not on molecular functional principles: to lift, not to trample out."

With wine and bark, fish or ordinary diet were repeatedly entered on the same card in these bad old days, when all rapid curative nutrition was stigmatized as "inflammation," and all inflammation was denounced as destruction by fire; when mercury pushed to salivation was the panacea for fibrinous exudation in exhausted organisms and habits the most cachectic; when the ulcerated lining membrane of the small or larger bowel was further ravaged by calomel at short intervals of time, and by drastic purges in the last hours of the night. We blame these "principles of treatment" in North Italy and elsewhere, now in 1863. It was but yesterday that the English practitioner was the only one "who knew inflammation when he saw it, how to stand up to it, and how to beat it down." When Englishmen died of acute disease in Rome and Lombardy, the taunt that went from hence across the Alps was not less arrogant in tone than that which glorifies our "stimulators" of the new school by disparagement of the Turin medici who bled and purged Cavour. The only difference is that the two ideas are utterly reversed with respect to the two nationalities at issue.

"Respect the blood," "spare the blood," "supply the blood," "renew the blood," were, as we chance to know, household words of constant earnest appeal to all parties concerned, in the wards of St. George's Hospital for certain lustra before and after the author of 'Restorative Medicine' became a student of Hyde-park Corner. A few other sayings occur to our memory, of familiar instance, at the bedside of patients of whom many must have been under the close observation of Dr. Chambers during his "Decennium Clinicum." Of *tonics*, "nothing is more tonic than a full dose of jalap, if needed and well timed." "Cases vary infinitely." Diseases, as commonly described, are conventional and complete over much. "They are always more or less too much in the abstract, by sense and phrase." "Treat the case and the patient rather than the disease." Common enough this last rule of practice! "Consider well in every case what is WANTING, as well as what is present, of symptom and character; for in clinical medicine the negative often helps more than the positive." "Nosologies and nomenclatures are of necessity evil, but being evil, they still are necessary." "By the true student of disease as it is, the several cases under his care would be best distinguished by numerals rather than by the nosological term for the supposed disease, often faulty, always imperfect." One more instance of a practical axiom which Dr. Chambers might have heard, day by day, in the wards and theatre of the hospital where he stored the material and much of the thought that have made his Decennium a work of useful reference for himself and others. About the best sentence of 'The Renewal of Life' is the one which we here transcribe from page 290 of Chapter XXI., on Indigestion. "It is never too late to try and administer to the failing organ the most potent of all medicines, the healthy human blood of the patient himself." These words—these very words—we can speak to them with full personal authority, were of constant recurrence in teachings and talkings to which we have listened with many others in due course of many a valuable lecture and clinical instruction at St. George's Hospital. It was the one favourite phrase of him who was never weary of uttering it. By force of repetition it still lingers in our ears. These maxims, worded more or less with a difference, were no doubt common enough at other hospitals, in other schools, in consultations, and in societies. The physician of St. Mary's maintains the good old restorative principle throughout his volume, cleverly and agreeably for the most part, yet damages it at times by his fanciful illustrations, and wantonness of paradox. Decidedly, in a sectarian point of view, restorative medicine has hitherto made no way. No reduction of premium on life policies has as yet been announced from the Hand-in-Hand Assurance Office. There is no call for fresh tables, no revised calculations from actuaries in general. There is as yet no acknowledged right of monopoly for the mart of life in its renewal. The "restoratist physician" cannot take up his patent. It fails in the specification. The restoratist M.D., if there be more than one, ignores his own rank, and keeps his degree in his pocket. Dr. Chambers, in his life-renewing capacity, is still the coming man. We fix Dr. Chambers with his self-assumed titles of "Restoratist" and "Life Renewer" with the less hesitation, as the rank with which they invest him is of his own creation, and he must be content to hold it with all its drawbacks of equivocal signification and liability. Moreover, the author of the two 'Introductories,' amiable, well-bred physician as he is, is singularly given to the calling of names. Allopathist, homœopathist, evacuator, stimulator, counter-irritator, eclectic, expector; these are his brothers of the profession, all, as it would seem, of legitimate parentage in the regular line of descent. We admit no such right of settlement under our family roof, and must beg to decline, with one exception, the entire connexion. It would be like living in a menagerie. To any one addressing us under any of these terms, with the one exception of eclectic, we should be inclined to answer, with Dr. Johnson, "Sir, you are abusive." Among whom has Dr. Chambers been living, that he should catalogue us thus? Certainly, in his day at St. George's, the physician who ordered the application of a blister or a double dose of house medicine was not therefore ticketed by the pupils as counter-irritator or evacuator. Passing on to the next bed, the same prescriber would have become a stimulator, an allopathist, an eclectic, as it might be; never a homœopath, for in such case he would soon have been alone on his round. Homœopathy was never placed at this great school of medicine, as Dr. Chambers has placed it, in the same category with certain terms in physic that are not utterly discreditable. Occasionally, some fifteen or twenty years ago, a fussy, twaddling governor or two would declaim against the inveterate prejudice of "the medical men" in this school of the Hunters, as instanced by their derision of the practice of homœopathy. "Denunciation," they would say, "is not argument," and "the subject of human life is too serious for ridicule; or, it might be, some weak brother of the outer world would timidly, yet logically, demand that the experiences on which Hahnemann had based his doctrines should be tried." It availed nothing. Homœopathy was from the first put aside as an impertinence, and shunned with pertinacity as a bore. Certain persons, otherwise physicians, prefer to designate themselves as homœopaths or homœopathists. Every man, so willing, has a right to abuse himself; but when, exceeding this liberty, they proceed to further glorify their own folly by stigmatizing as "allopaths" all who refuse to be addressed as homœopaths, the case is clearly one of quarrel and bad language. There is no special harm or discredit in the name of allopath, excepting as far as it is suggestive, by analogy of sound and sense, of homœopath; and so construed, it at once becomes abusive.

It must not be supposed by the outside public, to whom this volume is parenthetically addressed, that the rough and ready workers of the profession are such pragmatical simpletons as to suffer with approval the classification to which we have alluded, or so tame as to let it go forth to the public without the protest of, "Don't believe it."

Of the 'Renewal,' one soon comes to learn that it is not everywhere safe reading, excepting to its author. In page 7 of first "Introductory," there is a stumbling-block against which most plain understandings, if left to themselves, would be safe to come down:

"Above all things, avoid prejudice and scepticism. Consider it your duty *nullius jurare in verba magistri*, to be quite free of all rules, systems, and hero-worship, to try for yourselves the experiments (they are very few) on which dogmata have been based, to test the results intelligently, and then to believe in them firmly." (p. 7.)

Bad advice this to students, and stale bad advice; a spurious truism, happily not dangerous, being impossible to carry out. That physic may in any sense be taught, there must be rule, there must be system. There should be faith. The "aims, the ends," the "connecting links for which men weary to bind together the scattered fragments of medical knowledge," the "true guiding principles by which to modify the laws of medical action," the "bad theory which is better than none," what are all these but the varied expressions of rule and system as essential in the teaching of physic?" The 'Renewal of Life,' how else is it set before us in the very front of its published leaves? By "Clinical Lectures illustrative of a Restorative System of Medicine." And again in the next page, "the first two lectures show a connecting link in the system of therapeutics which must daily become more and more the guide to practice, and which I have made bold to call 'restorative.'" Thus we are told to "hold on by the links" in the title-page, and to "let go all" before the middle of the first chapter. The paragraph from which we have transcribed this denunciation of all rule and system should be read in context with a sentence in page 1, not as a fair sample of the book in its general character and completeness, for the 'Renewal' is in many respects thoughtful, clever, and high-toned; not as illustrative of the "application of pathology and therapeutics by others," but as "specially teaching the application of these sciences" by the restoratist author himself to his patients in the wards; as indicating the principles on which he acts and the right road by which to conduct the sick to health, by applying the knowledge that has been gained by him and his pupils in the observation of disease. Beyond this paragraph few readers would care to advance, unless advised by previous explorers that it was about the very worst in the book, and that once out of the "introductories," they would find very instructive, pleasant reading. With a triteness insulting even to a junior student, it cautions, "above all, against prejudice and scepticism." In the next sentence scepticism is inculcated on the student class as a duty. Finally and again, they are adjured to "firm belief," under the assurance that they may try for themselves the very few experiments on which dogmata have been based, and abide by the results when intelligibly tested. 'Restorative Medicine,' a dogma, one or more, resting on a few experiments—"very few"—tried by the student himself, and "intelligently tested," still by the student, for the result! The "restoratist physician," quite free from all rules, systems, and hero-worship, neither prejudiced nor sceptical, a professor of physic in the links, the apostle of a golden chain! Our author is so ready with imputations of scepticism, so exacting in his requisitions for unlimited trust in the development of his metaphysical dogma, that one is piqued into asking what are his received rules of evidence, and how far they entitle him to adjudicate in matters of doubt and belief. Evidently of credulous temperament, he condescends to homœopathy as though it were the honest vagary of legitimate medical science. The tone in which he discusses the theory and practice of this rampant imposture is indulgent, as if he were in conclave with its simpleton votaries in a Belgravian drawing-room. Through strychnine and digitalis he exercises a special privilege of control over the contractile fibres of heart and arteries, which has been conceded to no other physician now living or in times past. There is a chapter of eleven pages special on pepsine. In the carrying out of the essential principles of restoration by the scientific discharge of his "god-like occupation" for the relief of hysterical nursemaids and others, he is led to believe strange things of valerian, assafœtida, castor, and garlic. He tampers unbecomingly with drugs under his own formula of "mistura pythonis," in his exposure of the stercoraceous specific for phthisis. Continually rating the sceptic, he is never so successful as when he doubts and disbelieves, in his criticism of bygone theories and errors still prevalent in the practical treatment of disease.

He has a hankering after logic. We can describe his fancy for syllogism in no other way, unless we euphonize it into coquetting and flirtation. By turns courted and abused, logic, as it seems to us, is used in the volume before us with little more of real regard than attaches to any other word convenient for sound in the scattered register of dictionary harmonics. In passages professedly controversial of a scientific work, logic is a taking word, and has been often accepted for argument in full discharge of many an unbalanced account. In page 6 of the 'Renewal of Life,' a demand that the experiment on which a doctrine is based should be tried, is good because it is good, because it is "logical." At the close of the same chapter, "unprejudiced observation," conducted to logical consequences, is enabled to show that the old view of "disease" as "anything but a negation," was utterly false, that "in future it cannot but tend to unsafe practice," and that "all the good practice that may have sprung up at various times has arisen in opposition to it." "Food is logical, and argues in a circle." What does this mean? No disparagement, we may presume, to the "art of reasoning," that it qualifies life in its source, and moves in the most perfect of mathematical figures. Again, for giving force to an elaborate taunt levelled against order, method, and nosology, in the chapter on "Death, Life, Disease, and Cure," "the bold hand of a relentless logic" is made responsible for "consequences the most fatal to science." In page 89 of the chapter on "Anæmia and Bloodletting," the "logical French" are placed in unfavourable comparison with our "fortunately illogical countrymen," under reference to the bleeding and starvation statistics of the two countries. Finally, against the "illogical practice in extremes of both countries," Dr. Chambers feels bound to raise his voice. In this frequent perversion of a word of exact sense, there is a carelessness of style, if not a looseness of thought, that ill becomes the author who discourses on the "renewal of life." Logic is not the word to play with. Let Dr. Chambers renounce all use of it in the editions to come. It is not required of Dr. Chambers that he should chop logic with his pupils, for in physic the syllogistic art, as we have seen, does not protect from error, and sometimes spoils the truth. Without stay or help from exact reason in the exploration of what is set before us, and working together as we are bound to do, especially are we pledged to strive for candour in the opinions we profess, and good faith in the statements by which they are approved. In the intercourse of clinical medicine, entire frankness is the equivalent for the subtlety of conflicting argument. To observe, to reflect, to declare, is the one true working syllogism of bedside dietetics. To be reserved is to sophisticate. Concealment is, it may be, the betrayal of what is safe for conclusion. These common-place, old-world thoughts, are forced upon us, as, searching for Dr. Chambers' rules of evidence, we come upon the passage which we here transcribe from the chapter on Pneumonia:

> "But in all diseases which have been under treatment before yours, pray never let a word escape your lips, or a thought dwell upon your minds, about the patient being worse for the means previously employed. Most probably the harm done even by the most unsuitable drug is much less than we suppose. And it can hardly be but what *some*, at least, of the treatment has added to his chances of life more than if he had been let alone. Besides, we are all infinitely fallible, God knows, and it is not for us to judge of circumstances we have not seen." (p. 168.)

How, if our patient perishing under the diffluent stage of advanced pneumonia, had been under other treatment than ours—mercurial, antimonial, sanguinary, destructive; how if he or she had been gorged with strong meats, drenched into intoxication with brandy for weeks before our ministrations were invited! Is "no thought of these things to be suffered to dwell on our minds"? Are we forbidden to allude to them in consultation with others? Such affected ignorance would impose upon no one. It would be injurious to our patient, dishonest to our employers, discreditable to our guild, and unfailingly damaging in the long run to our character as consultants. It would be held a conspiracy for the future to "call us in." We are sorry to read such advice to the students under stamp of such authority, and in print that has been revised. The observations offered in support of it are confused and contradictory. The final acknowledgment of universal fallibility, with its deprecating clause of corresponding charity, is in painful contrast with the denunciations and dogmatic statements of the introductory chapter. Here is instance, it would seem, of the compensation balance that has been applied with careful adjustment to the working necessities of the 'Renewal of Life.' Somewhere or other in this volume, to every one who is willing to look for it, there is redress, under complaint, for whatever transpires in its pages of scandal or offence. The foreshadowings of mysticism in the title and introduction pass without reproach into working daylight in the hospital wards. To the warm glowing æsthetics of hysteria there is a set-off in the tabular statistics of the Chapters on "Continued Low Fever," and on the "Use of Alcohol." Paradox is supplemented by commonplace. The lion roar of defiance and exclusive dogma in "Theories at the Bedside," subsides into arbitration by physiology and universal charity for all but the sceptic in "L'Envoi."

There is a chapter wanting in this little book. There is a topic untouched in its pages, yet one of such paramount interest and importance in the development of their purpose, that to leave it out, like

Hamlet in the playbill, is to proclaim an absurdity. It is a radical shortcoming in the 'Renewal of Life,' that it offers not one word of remark on the waste and renewal of the vital powers by the vicissitudes of a function to which all others are, in truth, subordinate. A moment's thought, and to pathologist as to physiologist our meaning is known, without help from a name. There is a rule, it would seem, of intellectual obliquity and moral cowardice in the consultations and published opinions of medical men, in face of the one special inquiry, which it is their exclusive business to carry out and carry through. Of physicians, the greater number shirk it altogether, or studiously mystify the results to which it leads; while some few approach it, under apology, and with the reserve of conscious impropriety. To the author of the book before us, all notice of the function to which we allude was by himself forbidden. To set it forward and examine it in its wide range of influence, direct or by sympathy, in health or disease, was more than he dared to do. Once committed to his title of 'Renewal of Life,' had he proceeded under it to discourse of the reproductive agencies; in their double relation of blood and nerve with all else in the system, the scandal would have been fatal. Yet clever, thoughtful, and philosophical as our author is, how ill satisfied must he have been with a 'Renewal of Life,' in which the generative function found no place.

Having finished off his "Death, Life, Disease, and Cure" with the supplemental little chapter of "Objections Answered," the clinical professor goes fairly to his work in the wards. To follow him through his course of twenty-seven lectures and "lecturettes," would be to supersede the book and the author. Our essay would become a volume. Besides, at the bedside Dr. Chambers is in no need of an interpreter. We have already been with him there; we have made his cases our own; we have taken part in his campaigns; we have studied them in their commentaries. There is much to controvert, more to approve, a little that we fail to understand, a good deal that we should wish to supply. As might be expected, the two cardinal errors of modern physiology prevail everywhere through the work. As usual, they overlay all hope or pretence of permanent doctrine in the science of disease. No thought is taken of the blood in its mass and totality as an organ, primary, universal, instantaneous, simultaneous, in strict truth electrical in its agency through all else in the system. These, its incessant life-producing, life-destroying, life-renewing influences are too often, with conventional looseness of phrase, ascribed to the "nerve," "the nerves," or the "nervous system." To the nerve, local, limited, partial, secondary, subordinate to the structure from which it originates, and of which it is never more than a part; to the "nervous system," an expression true in its anatomical sense, convenient for the demonstrator, spurious in its physiology, as implying uniformity of function in structures various and dissimilar in their molecular organism and collective arrangement; flat, cylindrical, hard. pulpy, with or without ganglions, single, shapeless, double, symmetrical; obeying, escaping, or defying the will; incessant or remittent in their functional uses, sleepless from life's end to life's end, or under a diurnal necessity of repose;—the "nervous system," a combination, not the less marvellous, of countless bits and parts, from which, as considered separately from all other organized structures, nothing but its own nutrition originates, and which is capable of no common simultaneous influence but through the universal blood.

A sample of word-painting in the narration of a selected case, under the heading of Hysteria, may be of use in the revision of these clinical discourses for their many forthcoming editions. On our part, it would be a duty undischarged if we failed to draw the attention of the author to another instance of the false position in which he has placed himself, under his title of 'Renewal of Life,' when taken in context with the lively portraiture and luxuriant illustration of passages such as we here transcribe—

"The first bed we come to is occupied by E. J., a nurserymaid, aged seventeen, who was brought here from her bed in her night-dress, wrapped up in blankets, and admitted as an 'urgency' during the week. . . . . She has, you observe, a finely tinted, readily flushing skin, delicately chiselled features, quickly answering the movements of the mind, and a peculiar look about the eyes, which requires a separate sentence to itself. The balls are large; the sclerotic of a transparent sky-blue; the pupil much dilated, giving a dark appearance to a naturally light eye. The conjunctiva is smooth and bright, and is readily overspread with tears on slight emotion. The eyelashes are long and curled; the eyelids large, and especially full towards the outer canthus, giving a drooping, longing expression to the face. Altogether, she has what ladies technically call 'a sweet expression.'" (p. 247–8.)

Now we do not hesitate to say that this is not the right style of description for the clinical instructor of two or three score young gentlemen assembled round the bed of a "finely-tinted, delicately chiselled" nursemaid of seventeen, languishing under emotional hysteria in the open wards of a London hospital. It is, in the mildest phrase of criticism, equivocal. Had E. J., the pretty orphan nursemaid of seventeen, been a fat pasty kitchen-wench of twenty-five, she would not have been photographed in the wards of St. Mary's. The sequel of this most interesting young creature's history is not related. The last clinical report of her is, that after a course of shower-baths she was swallowing valerian three times in the day, as one of "several substances which have a special action on the nervous functions which serve the emotions." May we not conclude that, thereupon "restored" and well "braced up in her nerves," she retired from the health-giving shrine of Paddington with "renewed life," unconscious, as we hope, that she had been "oculis subjecta fidelibus," as the life-model of æsthetical medicine to the student class of the St. Mary's academy. There is a duplicate patient in this case in whose symptoms we are not less interested than in those of his prototype. Who was the clinical clerk of the restoratist professor on November 8, 1861? He must have been in daily, hourly danger. Were his troubles amenable to valerian under the douche? Where, and what is he now?

One homely, strictly practical question to the restoratist author, as to his curative treatment of hysteria, and we pass again from the clinical wards of the hospital to the pen-and-paper seclusion of the library. "Wonderful is the power of the stomach!" Has Dr. Chambers no faith in the potentiality of the bowels? "The cinchona is designed to have the same effect by bracing the surface of the mucous membrane, restraining its secretion of sticky mucus, and thus allowing the gastric juice to reach the food." (p. 257.) And what of the mucous surface of the duodenum, of the small and larger intestines, of the "sticky mucus" that prevents assimilation along their many yards of continuous secreting surface? Can it be that Dr. Chambers says nothing of purgatives in hysteria, because he never employs them? What, as critics, we should most desire, would be not to write another word on the 'Renewal of Life,' but to talk it over, line by line, with its author. Can we say more for the amusement and instruction that lies before us than by assuring our readers, with no breach of courtesy, that we prefer the author's company to theirs. Commending them heartily to each other, we leave them together in the wards. What remains of our task lies further on in the volume.

"Any of the full-bodied wines are better for acute diseases than spirits. Port, perhaps, exhibits in its commoner varieties more of the good qualities that a wine should have, than the produce of other grapes. But if the expense is no object, thoroughly good high-priced champagne exhilarates more, is easier digested, and does the good without the harm better than any of its rivals." (p. 413.)

We had come to the end of page 413 in Lecture XXIX., "On the Use of Alkohol" for the renewal of life, and had satisfied ourselves by these genial, most practical sentences, that over the bottle, as at the bedside, our clinical instructor was the best of good company. We had resolved, never in better humour, to hold on with him word by word to the last. We had forgotten the dogma, having finally renounced it with all its "answered objections," nearly four hundred pages earlier in the volume. Link by link we had released ourselves from the "golden chain." We turn the leaf, and find that we are reading French. L'Envoi! What is L'Envoi? What has it to do with alkohol, high-priced champagne, or the renewal of life? As a help to its conventional sense, we may tell our readers that we saw L'Envoi the other day doing duty as the heading of the last chapter of a serial sensation novel in one of the magazines. Does it mean, "Go, little book"? When Dr. Chambers takes leave of his pupils at Paddington, why should he talk French to them? Why "L'Envoi" rather than "Conclusion"? The phrase is fantastic,—a conceit. Let it be reformed in the editions to come. We read on, and find to our dismay that L'Envoi has a meaning beyond that of the author's retiring bow—that it again introduces us to doctrine, system, and definition. Once more we find ourselves face to face with the dogma. L'Envoi is a serious, emphatic appeal to the bedside pupils in behalf of physiology—a denunciation of scepticism, and in its last sentence a glorification of the art of healing, addressed by exception to nonprofessionals and mankind in general. Physiology "the key-stone of all medical knowledge, and arched foundation on which future practice must be based." Practice to rest on the arched foundation of physiology. But if on physiology, on what and on whose physiology? for no man's physiology is exactly that of his neighbour. On the physiology that confounds the nerves in a system, and ignores the blood as an organ? Besides, the physiology of any one day discredits that of the day before. So eager is inquiry, so rapid is publication. The metaphor does not hold, and never will. Physiology is behindhand with the work, and physic can't wait. Bit by bit, for ages past, the ground has been partially cleared. Labour is abundant; but arched foundation as yet there cannot be. There is no cement that will bind—there are not enough bricks in the yard. But little is settled as yet in the physiology of nutrition, secretion, or molecular action of any kind in the great context of organized functional life. Physiology, as at present understood, is doctrine rather than knowledge. The laws of glandular secretion, of venous absorption, of the breathing process, of the contractile force in muscles, are still under searching revision. Reasons the most plausible might be found in physiology for methods of treatment the most conflicting in any selected case of disease. The true physician smiles mournfully at the stock paragraph of "consummate medical skill exhausting the resources of science and art" for the relief of a sick prince or other death-stricken dignitary.

But even were the arch of physiology complete from abutment to keystone, it would suffice, but as one of many, for substructure of the hospital workshop. Practical medicine needs every sick chamber for a lecture-room, and counts its professors by the thousands. Physiology is, in truth, available in practical medicine to but a limited extent. As a professor and practiser of cures, the cases are comparatively rare in which the physician has gathered hint or counsel from the teachings of conventional physiology. Opium, cinchona, bark, chloroform, have not been enucleated from a problem—are not the results of physiological induction. It is of easy proof, that in the running account of mutual service between physiology and practical medicine, physiology is by far the most indebted of the two. Practical medicine is always enriching physiology by experiment, chance capture, and observation. Its contributions, in fact, are too little costly for account, too unremitting for store. Physiology has given more results of practical value to surgery than to physic. To the surgeon it is a pioneer—disciplined, daring, and ready; to the physician, for the most part, a blind and halting guide. The operation of tracheotomy — those for hernia, hæmorrhoïds, varicose veins, indeed all approved measures in surgery, rest, as they were instituted, on physiological principles.

For the discredit which of late years has attached to much of medical practice in this country, the false logic and loose analogies of a vain, spurious physiology are mainly responsible. During certain decades of our medical history the scandals of a shameless hydra empiricism were glorified under the blazon of "true pathological principles" and "improved practical science." Yet, had the popular practising physicians of the decades 1820, '30, '40, been physiologists to the extent even of believing in the blood as a vital necessity of structure and function throughout the body, enormous suffering would have been prevented, countless lives preserved; and we should not now have been under the triple ban of homœopathy, excessive stimulation, and phlebotomyphobia.

Is not the physiologist the only true pathologist? Who cares to answer the question? It reads like a truism—so does much that betrays. *Cave canem!* Beware of the dogma! Let the reader take thought before, by one civil word of assent, he affirms with the author, that in the treatment of disease he has nothing positive before him—that cough, spasm, fever in all its variety, are mere "deficiencies," that inflammation is in no way a "manifestation of life," that pain is an "abstraction." In "restorative medicine" this is pathology, the one true theory of disease and its treatment. Starting from this end of the proposition, who but a fanatic would consent to receive his notions of life and health by reading his lesson backward from a pathology which rests on a dogma and begins by a negation? There is no way but this of getting at the physiology of which L'Envoi is the apotheosis. At home or abroad, in lecture, treatise, or by example, the medical student, moving in his curriculum, gets no hint from his more orthodox teachers of what it most concerns him to know. They plead blindness to the one light by which the "restoratist" finds his way from life in its abundance to the necessities of disease. Let a manual of "restoratist" physiology be published in advance of the next edition of the 'Renewal,' that henceforward there may be a fair start and clear course for all regular entries who "go in to win" with full recognition of the "obvious truth, that the best and only mode of arriving at a knowledge of the "deficiencies of vital powers on diseases, is by a knowledge of those powers of which they are deficiencies." So shall the therapeutics of Dr. Chambers become, under science, a "system," and his dogma take rank as a doctrine. When all is fairly before us, it may be found that Dr. Chambers' physiology does lead to Dr. Chambers' pathology; but, as a general maxim, we demur to the axiom, that physiology is the only true guide to pathology. There are many good practical truths in pathology with which physiology has but little to do, while much that is spurious is of physiological origin. A great deal of good pathology has been spoiled by bad physiology in the attempt to make the pathological facts square with physiological doctrine.

Practical experience and judgment will often supersede physiology in the forecast of disease, and with full consent of the physiologist himself. They are true qualities, and paramount, when true, in the management of illness.

"To non-professional persons I would say, see and believe that the art of healing is a true thing, not a set of rules, not a doctrine, but a real means of adding to life and happiness. See—for you can see if you like—that it is founded, not on opinion, on traditional notions, but on a sure knowledge of God's works." (p. 415.)

This sentence assumes two mischievous errors—that the theory and practice of medicine are irrespective of rule and doctrine, and that rule and doctrine are incompatible with truth and reality. How does Dr. Chambers reconcile this tirade against rule and doctrine with his axiom on the top of the same page, that the only road to true pathology and the most certain advance of improved therapeutics is through physiology?

In collating this last very questionable passage with one which we here transcribe from page 247, it will seem that Dr. Chambers has not always been of the same way of thinking as to the natural order of precedence in the curriculum of medical science. "The true use of morbid anatomy is to teach physiology, not the art of medicine; and it is the art of medicine which must be the prime object of your work in the hospital wards and in the business of your lives." Here, then, in the relation of teachers and the things taught, there is between physiology and pathology a direct transposition of place. Physiology is taught by pathology, while the art of medicine is made to stand apart from both. There is one way of conciliating this apparent confusion of thought and corresponding inconsistency of statement. It may be that Dr. Chambers does not include the science of morbid anatomy in his sense of pathology, in which case the sequence of instruction would run thus:—Anatomy, morbid anatomy, physiology, pathology, therapeutics. We make offer of this compromise in our own disfavour to the author of the 'Renewal,' not, however, believing that he will close with it. In pulling through the cross-currents and ill-defined shallows of "restorative medicine," as critical oarsmen we have often been thrown suddenly aback. Should the sciences of pathology and morbid anatomy now be recognised in the restoratist academy as distinct the one from the other, we are content to suffer under a double taunt of approved and familiar frequency. We have already confessed to the capture of many a "crab." It is possible that, in our misinterpretation of restoratist definition, we may have realized a "mare's nest."

We have handled the two passages last quoted with more care than seems to belong to them. It is that there is distinct recognition in them both of the principles and divisions established in regular medical education. And yet we cannot at all make out from them, or from any other passages in the 'Renewal,' how Dr. Chambers' physiology (whatever it may be) leads up either to his own pathology or to that of the physicians associated with him in charge of the sick. They lend us no help towards a better understanding of the restorative system of medicine as shown by "special application of the services of pathology and therapeutics" to the clinical necessities of Dr. Chambers' patients in the wards of St. Mary's Hospital. We have examined the much-vaunted "golden chain," with more care than he who wrought it, from end to end, and we are unable to make out how and where it hangs on to disease, as disease is in common understood, so as to work with consent of the many in the task of prevention and cure.

More than once in these pages, in the headings of chapters and elsewhere, we come upon the mention of a restoratist pharmacopœia. We fear that the phrase must be received only in a figurative sense—" Pharmacopœia Restorativa!" It is too good to be true. We glean what we can from scattered phrases in the volume as to the rudiments and raw material of this supplementary work, should it ever be taken in hand.

Here is what must be considered a prolusion in the matter of supply: "I look upon these viscera (stomach and bowels) as a means of cure, as an agency for me to employ for the renewal of life—in fact, as part of my materia medica."

"The restoratist physician should look at his pharmacopœia with an eye to the ultimate benefit of the patient before him, to a goal beyond that of the immediate effects. He should make his chief thought how each of the reagents employed will finally touch life. The tools with which he has to work, and which he knows best as trustworthy, may be fairly divided into CONSTRUCTIVES, ARRESTERS, and DESTRUCTIVES, according as the end they accomplish affects metamorphosis; and on this division he rests his practice." (pp. 31–2.)

In this great classification of remedies it will be observed that there is no mention of alteratives. Like "disorder" in the restoratist pathology, they are superseded.

In L'Envoi we are told with much earnestness, that "the most certain way of advancing the art of healing is to search out the essential actions of physical agents." And thus it transpires of valerian, assafœtida, castor, musk, and garlic, that they are "bracers-up of the nerves," having a "special action on those nervous functions which serve the emotions." To strychnine, as to quinine, is ascribed a great and peculiar elementary power. Pepsine has an entire chapter of eleven pages to itself; "it is a valuable and safe remedy, and an important aid to rational medicine." Very much more credit is claimed for it than Dr. Pavy would be inclined to endorse. Antimony and mercury "are destructive drugs;" digitalis is a direct tonic to the contractile fibres of the heart and artery; strychnine co-operates with digitalis and iron to the same end. This latter, moreover, "tones and invigorates the involuntary muscles of the stomach more invariably than any other remedy."

For the graduate in "Restoratist Medicine" it is the more to be wished that there should be an accredited system or pharmacologia of restoratist therapeutics, as certain passages in the 'Renewal,' in the matter of prescription, are somewhat difficult to reconcile.

"I believe M. Boudault prepares a powder in which strychnine is combined with pepsine and lactic acid. I presume it is for cases of this sort that it is intended; but I do not like mixing up ingredients in that way, and prefer the strychnine alone, as it is the real active agent." (p. 395.)

"Iron, digitalis, and strychnine may be combined in the same draught, in the same pill, or in the same powder, without inconvenience." (p. 229.)

"Where you give digitalis, make it a rule to add iron and sometimes strychnine to the dose, as you thus get the full advantage of the digitalis, and avoid some of its possible evils. Iron prevents nausea, and strychnine co-operates in strengthening and regulating the action of a weakened heart." (p. 179.)

"You will observe that the medicinal treatment is a union of destruction and construction, so as to try and alter, as far as possible, the whole habit of the system—to cause by destruction a demand for new material, the supply of which is guaranteed by the iron. The mercury causes a general increase of metamorphosis, the waste from which is directed to the kidneys by the squill and nitre. The digitalis tends to relieve congestion by increasing the activity and tone of the blood-stream. So that, by a union of virtues, the combination prescribed in the pills will rarely fail to prove a powerful diuretic." (p. 118.)

It is quite in keeping with the title and earlier passages of this volume, that we should read such a sentence as this at its close—"The sooner a sceptic leaves our profession the better." Ungracious words these, and following others of a still more ungracious kind. Are they personal in their application by the author? Are they written at some one? And is that some one himself? Is Dr. Chambers not entirely comfortable as to the extent and security of his own faith in the renewal and restoration of life? It may be remarked, that he never loses an opportunity of scolding the sceptics. "The sooner the sceptic leaves our profession the better." We think not, we hope not, for in such case Dr. Chambers would be one of the first to go. In physic, who is sceptical, if he be not? Does he not doubt and consider? To allopathists, homœopathists, or regular orthodox practitioner, by application of his own rule, what is he but a sceptic under notice to quit? Better sceptic than mystic or dogmatist; fanatic worse than both. It is from the scepticism of thirty years past that Dr. Chambers has received the inspirations of restorative medicine. Many of the best thoughts in his 'Renewal,' nearly all that he believes to be original, he owes to the scepticism that has gone before him. Here is sample under the author's own hand of his readiness to demur on fitting occasions to the exaction of established doctrine—

"Does not the daily growing hesitation of the conscientious, the louder and louder bluster of those who assert that they alone are 'orthodox,' show a rapidly diminishing faith in our fathers' principles of medical treatment? And does not this diminishing faith in an age of advancing knowledge show those principles to be really unsound? . . . . It is of no use to mince the matter—the root of the whole five principles is unsound; they are based on false foundations and all the results of increased knowledge can only show us more and more their falseness. . . . . In fine, all confess, either by words or deeds, that a true guiding principle—a single aim—in the treatment of disease is wanting." (pp. 12, 13.)

If this be the "true preaching," how difficult the rule of faith! It may justify an appeal, it does not warrant a denunciation. Dr. Chambers deplores the sad results of venesection in times but recently past. He insists on the necessity of renewing the blood's current in vessels from which it has been drained by artificial means. He feeds while he bleeds, and gives good reason for so doing. In the treatment of pneumonia, he says nothing of antimony or calomel—

"Nothing has been said about antimony and mercury—drugs formerly much used in pneumonia. They are destructives; and I cannot see that there is anything to be destroyed in this disease, or that there is anything whose destruction would aid the employment of direct restorative treatment. When I used them I was frequently obliged to leave them off on account of bad symptoms attributable to their agency, and I always felt doubtful if success in prosperous cases could be traced to it."

How, if in 1830-40, thinking and writing thus, Dr. Chambers had been responsible for practice in the wards of a London hospital! In these mad bad decades of reckless bleeding and purging, of starvation and indiscriminate calomel, was not scepticism the one true religion? One may search long and painfully for a something to believe in, complaining that it can't be found, and yet believe in much that has been found before.

L'Envoi still on the road! A book-post, by free translation, with a large delivery. We glance downwards for further directions, and will go with it to its final address, "To non-professional persons." As the title, so the valediction of this volume. We have done with L'Envoi. In it there is not a sentence that in honest goodwill to the author we would not willingly expunge. Away from the introductories and their dogma, Dr. Chambers talks useful intelligible physic for twenty-seven chapters, in plain, pleasant language, and is evidently most at home in truth and the vernacular. Having mastered the idiom, he is unable to recover the lingo. In L'Envoi, we are pleased to note it, his tone of defiance is subdued to one of appeal and invocation. There is misgiving in the dogma. He speaks with bated breath. It is in no harsh spirit of criticism, certainly not from carelessness in the perusal of the volume before us, that we reject this its last chapter. We have read the 'Renewal' through and through, and are all the better for it. We are set, as far as our will can work, on having it read by others. It would be capital reading for the working physician, pupil, or professor, were it again sent into the world, sound in body and limb, but deprived of head and tail. Let it renounce truth and principle, unpack its system, cancel its title-page, send L'Envoi back to the dictionary, and begin with the third chapter. So shall it no longer peck at its own vitals, and lash itself approvingly because it has done itself a mischief.

This so-called review of the 'Renewal of Life,' which, under compulsion of time and space, we now bring unwillingly to a close, is, we feel and know, a most imperfect report, in a critical sense, of the hived thought, the heaped-up experience, the charm and ingenuity of language by which the author transfers the skilled labour of years in exchange for an hour's pleased attention from his readers. There is not a page of this little volume that can fairly be dismissed with a simple unqualified sentence of censure or approval. It is in truth a many-sided book, and as such should be approached by those who undertake to report on it for the information of others. Rich in material, original in reflection, ingenious in analogy, clear and graceful in narrative, frank, fearless, and humane in spirit, the 'Renewal of Life,' irrespective of the dogma, has already taken rank in the working literature of the profession. And the dogma itself, all allowance made, has no doubt worked in some measure for good. The 'Restorative

System of Medicine' is little more than the acknowledgment, under a new phrase, of the necessity for waste and supply in due proportion, while healthy growth and function are conditions of the system. This very common, but essential truth, the British practitioner for many years had failed to remember. It is well to remind him of it from time to time, and Dr. Chambers well discharges this self-imposed duty. The most agreeable of dogmatists, he proves to the satisfaction of all, that to be learned in matters medical, it is not necessary to be dull. Crabbe's often-quoted library lines would read without point on the fly-leaf of 'Renewal of Life :'

> "Ye dull deluders, truth's destructive foes,
> Ye sons of fiction clad in stupid prose."

Moreover, to the outside public the 'Renewal' is not without its value. To them it shows, and with full effect, what was never shown before. It photographs the London physician at his work. It is well that our employers should know what charges they put upon us, and in what temper we meet them. Duties urgent, multiform, harassing, disagreeable; incident and character in never-ending variety; prejudices that must be humoured, yet cannot be suffered; wilfulness to be withstood and beaten down; inquiry that ranges with the patient's life; sympathy, reserve, address; all that we doctors observe, do, say, or suffer, find their place in these pages, and stay with the thoughtful for notice. It is here on the record—and who would turn from it?—how, in sick chamber, dead-house, or hospital ward, the gentleman leech has his right of precedence, and is forward among the first; and everywhere throughout the volume it transpires that what would be vulgarized and held of no account by the trader in routine prescription, can be refined into grace and wisdom by the well-trained æsthetical physician. Better than all, by letter as in spirit, there comes from these clinical readings full, quiet assurance for the sick, that the art which holds intercourse, through science, with disease, is here in England progressively good and essentially humane. And thus, edition after edition, our amiable restoratist will be always doing the man medical a good turn with his client, and helping to right us with the world at large. 'The Renewal of Life,' with much that is equivocal, holds out to the profession what few have been able to offer. It is a book with the heart in it.

REVIEW XII.

1. *A Treatise on the Continued Fevers of Great Britain.* By CHARLES MURCHISON, M.D., &c.—*London*, 1862. pp. 638.
2. *Lectures on the Distinctive Characters, Pathology, and Treatment of Continued Fevers, delivered at the Royal College of Physicians of London.* By ALEXANDER TWEEDIE, M.D., F.R.S.—*London*, 1862. pp. 301.
3. *Ten Lectures Introductory to the Study of Fever.* By ANDREW ANDERSON, M.D., Lecturer on the Practice of Medicine in Anderson's University, Glasgow.—*London*, 1861. pp. 180.
4. *Typhoïd Fever in India: its Clinical Characters, Morbid Anatomy, and Etiological Relations.* By W. HANBURY, Surgeon 33rd Foot. (Trans. Medical and Physical Society of Bombay, No. 7, New Series.)—*Bombay*, 1861.
5. *Further Observations on Typhus and Typhoïd Fevers, as seen in Dublin; especially the United Form they assumed during the first half of the year* 1862. By HENRY KENNEDY, A.B., M.B.—*Dublin*, 1862. pp. 28.
6. *Remarks on the Changes which are supposed to have taken place in the Type of Continued Fever.* By CHARLES MURCHISON, M.D. (From the 'Edinburgh Medical Journal,' August, 1858.) pp. 6.
7. *Syllabus of the Lectures on the Causes of Fevers, delivered during the Session* 1860–61. With an Etiological Nosology. By THOMAS LAYCOCK, M.D., F.R.S.E., &c. &c.—*Edinburgh*, 1861. pp. 19.
8. *On Malaria and Miasmata, and their Influence in the Production of Typhus and Typhoïd Fevers, Cholera, and the Exanthemata.* By THOS. HERBERT BARKER, M.D., F.R.S. Ed.—*London*, 1863. pp. 251.
9. *Clinical Medicine: Observations recorded at the Bedside, with Commentaries.* By W. T. GAIRDNER.—*Edinburgh*, 1862. pp. 741.

THE literature of fever has lately received accessions not unworthy the importance of the subject. Amongst the contributions of British observers, the work of Dr. Murchison and the Lectures of Dr. Tweedie occupy the most prominent position. The former comprises a most conscientious and painstaking inquiry into the identity or non-identity of the poisons producing the different forms of continued fever, an extended and laborious historical summary of the various epidemics which have been chronicled by successive generations of physicians, and an essay on etiology, geographical distribution, phenomena, diagnosis, prognosis, and treatment, which for scope, accuracy of observation, and research has seldom been surpassed. The publication of such a book marks an era in pyretology. Dr. Tweedie's lectures, also, the fruit of a long professional life devoted to the study and observation of fever, are in a practical point of view of high merit. To these we have added, at the head of this article, a list of works and pamphlets, all of which, in differing degrees of excellence, are evidence of the attention which the subject is at present attracting, whilst several amongst them, as the Lectures of Dr. Anderson, the experimental essay of Dr. Barker, the Observations of Dr. Kennedy, and Mr. Hanbury's account of typhoïd fever in India, are valuable contributions to the scientific capital of medicine. The section of Dr. Gairdner's work on fever as seen in Edinburgh has already occupied our attention. But we shall have again occasion to examine its conclusions in the light thrown on them by facts obtained in other and more extended fields of observation.

Typhus, typhoïd or enteric, relapsing, and simple fever are the four forms almost universally allowed and recognised in England. Typical cases of typhus differ as clearly from typical cases of enteric fever as do cases of measles and scarlatina. Thus far there are few dissentients. But the closeness of relation existing between these forms, whether they be mere varieties or true species, whether their origin depend on one and the same cause, or whether the poisons producing them be entirely different, whether there be not a large class of indeterminate cases which it is impossible to refer with certainty to either form, these are subjects on which various shades and differences of opinion are still held by highly competent observers.

In discussing these and similar questions, the origin and affinities of simple fever need not be included. It is universally allowed that a non-specific febrile attack may result from exposure to the sun, fatigue, and excesses; that such a fever is non-contagious, and neither occurs as an endemic or epidemic. Limiting ourselves, therefore, to the consideration of typhus, enteric, and relapsing fevers, we may first inquire the grounds on which those who still assert the essential identity of these diseases base their arguments. It is clear that unless we are prepared to admit that not only the continued fevers, but small-pox, scarlatina, measles, plague, and the intermittent and remittent fevers, all have origin in one and the same cause, no argument can be founded on the broad resemblance of the phenomena of the febrile condition. In all we have the same elevated temperature, the same increased tissue-metamorphosis, the same affection of the nervous system, and in all we may have the same symptoms of uræmic poisoning (typhoïd symptoms) from the retention of the product of tissue-metamorphosis in the blood. It will also be at once admitted that some striking phenomena presented by two or each of the three diseases under consideration fail for the same reason to support the doctrine of identity of cause. Each of the group is contagious, although in different degrees, but so are plague and the true exanthemata. The activity of the contagious property in typhus and relapsing fever is far greater than that of enteric, but the tendency to spread by contagion in the two former is less than that observed in small-pox and scarlatina. Again, as in the exanthemata, one attack of typhus or of enteric fever confers an immunity from subsequent attacks, although there is now abundant evidence that typhus does not protect from enteric, nor enteric from typhus, any more than measles gives immunity from small-pox. To have suffered from relapsing fever does not shield the individual from subsequent attacks, nor from attacks of typhus, although it may be noticed that there are very few observed cases in which a person who has gone through an attack of typhus has contracted relapsing fever at a time when both diseases have prevailed. Wardell has recorded an instance in which relapsing fever occurred a fortnight after an attack of genuine typhus. Dr. Murchison observes that it is probable in some of the supposed instances of the occurrence of relapsing fever after typhus, the former disease was in reality enteric fever; but nevertheless, on the whole evidence, it seems clear that typhus does not afford the same degree of immunity from relapsing fever that it does from a second attack of typhus. Like also the true exanthemata, two members of the group, typhus and enteric fever, present distinctive cutaneous eruptions, which in well-marked cases are entirely different in characters, time of appearance, and course. Thus far, then, we have only evidence which in no way proves a closer relation between these fevers than may be safely asserted to exist between the exanthematous fevers, whilst the same resemblances might be urged to support an equally near affinity with the latter group.

The question then recurs, on what ascertained facts do the impugners of the doctrine of non-identity of cause reason? One of the chief supporters of non-identity is Dr. Kennedy, of Dublin. This gentleman, whose large experience entitles everything he advances on the subject of fever to be received with attention, has, in his recent paper published in the last volume of the 'Dublin Quarterly Journal,' based an argument on the circumstance that cases which it is difficult or impossible to refer to either the typhus or enteric species are to be constantly met with. He impugns the value of Dr. Jenner's conclusions on the ground that they were based on 64 cases only, and that as these were all fatal, his conclusions were founded on a minority of instances, inasmuch as the majority of fever cases recover. He appeals to the evidence of Huss, whose observations were made on 3000 cases, and spread over twenty years, and who asserts the frequent occurrence of intermediate forms of fever exhibiting the symptoms of both petechial and abdominal (typhus and enteric), and the non-discovery of affection of the intestinal glands in cases marked by enteric symptoms; whilst, on the contrary, the specific lesions of typhoïd were found by him in certain instances in which the symptoms exhibited the petechial character. Dr. Kennedy quotes the account given by Huss of an epidemic in which fever "broke out amongst 250 soldiers, all between twenty and forty years of age, and placed under exactly the like hygienic conditions, and yet the disease assumed the distinct form of typhus in one part of the cases, of typhoïd in another, and a third took an intermediate form"—64 cases occurred. Huss also has recorded an instance in which, "in one house, 17 cases occurred within a fortnight, of which 10 were typhus and 7 typhoïd."

Another of Dr. Kennedy's arguments is based on his own experience as well as on that of Huss. It is that during the same epidemic the type of fever will undergo a change. At the commencement of the epidemics described by Huss, typhus was the prevailing form, whilst later enteric cases were more common. This fact is supposed to be more consistent with the hypothesis of one poison than of two. The occasional occurrence of ulceration of the ileum in cases of scarlatina and small-pox, as noted by Flint, Huss, and Anderson, is advanced to prove that there is nothing specific in the peculiar lesions of enteric fever. In fact, Dr. Kennedy goes so far as to hint that the ulcers of the ileum in enteric fever may, like the ulcerations of the intestinal glands in phthisis, be referable to the strumous constitution. He also urges, that certain symptoms which have been supposed by some writers to be characteristic of enteric fever, have been observed in typhus, and those of typhus in enteric. He instances the experience of Professor Flint, of Buffalo, who found that diarrhœa was present in one-half of the cases of typhoïd observed by him, and in one-third of the cases of typhus—a prominent symptom in the former cases, but mild in the latter; and who speaks of active persistent delirium as being characteristic of typhoïd. Dr. Kennedy refers to another symptom, intestinal hæmorrhage—supposed by Dr. Jenner to be characteristic of enteric fever—as having been observed by himself in 30 cases of well-marked typhus. In those of his cases, however, which were fatal, no ulceration was found after death. Epistaxis again, which has been far more frequently observed in enteric fever, is, according to Dr. Kennedy, equally common to both types. In his paper, Dr. Kennedy gives a short account of 40 cases of serious fever which came under his notice during two years commencing June, 1860. Amongst them he relates several cases that occurred in the first half of 1862, in which, with a skin densely spotted, there existed diarrhœa and tympany. In the only one of these which proved fatal the ileum was not ulcerated, although it was very red in distinct patches, and the more so nearer the cæcum. The cæcum was ulcerated in patches, one of which was as large as a shilling, and there were small but distinct ulcers in the ascending colon. The glands were unaffected. This case proved fatal on the twenty-fifth day of the fever. The following is the summary which Dr. Kennedy gives of the two years' experience—

"With two points of exception they (the 40 cases noted) may, I believe, be taken as a fair sketch of the types of fever prevailing in Dublin within the last two, and some former years. These types were the typhus, typhoïd, and gastric. Leaving out the latter for the present, the reader will have observed the striking contrasts which these cases present: we find cases of typhus in both young and old;—of typhus without spots;—of typhoïd with none, with one or two, or with an extensive crop of them;—of typhus with the brain wonderfully free;—cases of typhoïd, but more numerous, the same;—of both typhus and typhoïd, in which the state of the tongue and parts about were identical from sordes;—many cases of either type with the chest not engaged at all, or so slightly as not to call for treatment;—instances of both types with and without tympany;—cases of either kind entirely free from hæmorrhage, a freedom remarkable when compared with former years;—and lastly, the modified types of fever which the present year (1862) has disclosed." (pp. 20, 21.)

Another argument advanced by Dr. Kennedy against what he terms "these refinements in diagnosis," is the protean character of enteric fever. He appeals to the authority of Louis and Dr. Gairdner, now of Glasgow, both strenuous advocates of the doctrine of essential difference, in support of the difficulty of diagnosing typhoïd.

"Sometimes," says Louis, "the diarrhœa and meteorism were the most prominent; at times the depression of strength, delirium, spasmodic motions of all kinds; and according as one or the other was most prominent, the disease had the appearance of putrid or ataxic fever; sometimes, likewise, that of inflammatory fever; in some individuals there was no delirium, or it was very slight, and, notwithstanding the most grave lesions, the calmness continued until death. Notwithstanding these different aspects, the affection was constantly the same; the principal disorder did not change."

In a lecture, first published in the 'Lancet' of July 21st, 1860, Dr. Gairdner writes:

"Nothing then can be more variable or less characteristic than the general symptoms of the [typhoïd] fever. I have seen it resolve itself in ten days with the symptoms of febricula, or of a mild remittent fever; I have seen it, on the other hand, last nearly as many weeks, and pass imperceptibly into organic disease. It mimics, in turn, not only all other fevers, but many other general or local diseases; phthisis, pneumonia, meningitis, perhaps more frequently than most others."

Before ending his chain of reasoning, Dr. Kennedy refers to the structural pathological differences which are supposed to mark the distinction between the two fevers. He argues that the case in which he observed ulceration of the cæcum and colon may fairly be referred to the enteric type, although the glands of the ileum had undergone no change; first, because in a third of Louis' cases the colon and cæcum were affected, and, secondly, because it is generally admitted that "the affection of Peyer's glands does not necessarily bear a relative proportion to the fever." He also asserts, on the authority of Dr. Gordon, that cases occurred in the spring of 1862, in which ulceration of Peyer's glands was accompanied by the symptoms of typhus, "including a crop of genuine petechiæ." Dr. Kennedy winds up his argument by expressing his belief "*that the two fevers known as typhus and typhoïd are the result of a single poison, and that no other hypothesis can explain so well all the difficulties of the case.*"

We have elsewhere observed that we are glad that this question has been thus reopened in this country, for assuredly there are few which have a more important bearing on hygienic and preventive medicine. We would allow to Dr. Kennedy's arguments their full power, but that they are strong enough to support the conclusion he draws is a position which a careful review of the whole evidence does not lead us to assume.

If we now turn to the writings of the supporters of the non-identity theory, we shall find that many of Dr. Kennedy's arguments have been anticipated, that others admit of answer, and that a different explanation may be given to his facts. It is true that cases do occur which it is difficult to diagnose, but, as long ago urged by Louis, the difficulty is not greater than is occasionally encountered in other diseases about which ordinarily there is none. Cases of scarlatina, and even of smallpox, are now and then met with which are only recognised by external circumstances, and not by the intrinsic characteristics of the disease. The most characteristic symptom of typhoïd, the eruption of lenticular rose spots appearing in successive crops, may doubtless be absent, or so insignificantly developed as to be excluded in estimating the case. But so, in like manner, scarlatina faucium may exist without any efflorescence on the skin, and the nature of the affection might be entirely overlooked unless it were clearly traced to contagion, or happened to spread and give rise to the perfectly developed form of the disease in others. So with the mulberry rash of typhus, it also may be absent, but the disease in this form is capable of communicating the characteristic spotted fever to persons brought within its influence. As in the exanthemata, so both in typhus and typhoïd, the eruption is the most diagnostic symptom, and when wanting in the case of enteric fever the difficulty of diagnosis may be insurmountable. In what proportion of cases this difficulty is likely to be met with in this country is shown by Dr. Murchison, who has examined the records of the London Fever Hospital, extending over a period of ten years. With regard to the eruption of enteric fever, Dr. Murchison writes:

"The eruption is not invariably present, but is perhaps more common than is generally believed. Of 1820 cases admitted into the London Fever Hospital during ten years, it was noted in all except 224, or 12·3 per cent.; and in many of the 224 cases, the fact of the spots not being observed was probably due to their not having been looked for with sufficient care. . . . . They are more frequently absent in patients over thirty and under ten years of age, than in patients between ten and thirty. Of 1413 cases admitted into the Fever Hospital, between the ages of ten and thirty, the eruption was observed in all except 142, or 10 per cent.; of 252 patients over thirty, it was not observed in 40, or in nearly 16 per cent.; and of 107 cases under ten, it was not noted in 37, or 34½ per cent." (pp. 470, 471.)

In the case of typhus—

"Of 3506 cases, the eruption was noted in 3103, and was not observed in 403 cases, or in 11·5 per cent. It was as often absent in males as in females; thus of 1737 males, it was absent in 193 (11·11 per cent), and of 1769 females in 210 (11·87). In children it was oftener absent than in adults. Thus, the mean age of the patients in whom the eruption was present was 29·74, and of those in whom it was absent only 26·28. Again, of 398 cases, where there was no eruption, 119, or 30 per cent., were below 15 years of age, while of 3058 cases with the eruption, only 444, or 14 per cent., were below 15 years. In other words, of 563 cases below 15 years, the eruption was absent in 119, or in 21 per cent., whereas of 2943 cases above 15, it was observed in all but 279 cases, or 9 per cent.; and of 17 cases below 5 years, it was absent in 7. These figures exaggerate the proportion of cases with no eruption, for in some it had no doubt disappeared before the patient's admission, and in others, where there was only slight mottling, it had probably escaped observation. Of 90 cases noted by myself in 1856, the eruption was present in all but 6 patients, 3 of whom did not come under observation until a late period of the disease; and of 1107 cases admitted into the Fever Hospital during the first six months of 1862, the eruption was noted in all but 51." (p. 130.)

The above extracts prove that there is always a certain proportion of cases in which the sign which furnishes beyond doubt the most certain criterion is absent; but whilst the same may be affirmed of a large number of other diseases, subordinate points of resemblance cannot prove identity of cause as long as a majority of typical cases of both fevers occur which are as clearly and specifically different as any generically allied nosological species. The confusion depends on the imperfections of our powers of diagnosis; it is not chargeable on the order of Nature.

We should pass over Dr. Kennedy's comparison of the extent of Jenner's observations with those of Huss, merely remarking that Jenner's cases, although few in number, were most accurately observed and described, whilst, as is pointed out by Dr. Murchison, Huss includes all the spots met with in fever, which are neither petechiæ nor sudamina, under the term *tâches lenticulaires* (synonymous with his eruption typhoïde), confounding therefore the florid spots which in typhus precede, and are converted into petechiæ, with the lenticular rose-spots of enteric fever—a fact that entirely explains his assertion of the frequent co-existence of *tâches rosées lenticulaires* with the petechiæ of typhus. But it is to be remembered that ever since the publication of Dr. Jenner's memoir, which first brought conviction to the minds of English physicians of the essential difference of the two forms of fever, testimony on the point has been steadily accumulating.

In the first place, there is the mass of evidence accumulated at the London Fever Hospital. From the register and case-books of that hospital, Dr. Murchison has ascertained that 5326 patients suffering from the two forms of fever were admitted during ten years. Of these, 3506 cases were diagnosed as typhus, and 1820 cases as enteric fever. It frequently happened that several members of one family or residents in one house were admitted at one time, or followed each other in quick succession. But with two exceptions, cases of typhus and enteric were never brought from the same house, except after the intervention of months or even years. One of the two exceptions is well known, it having been investigated by Dr. Jenner. "A boy, aged sixteen, was admitted on September 19th, 1848, with enteric fever, and on October 10th his father was admitted with typhus. But the mother of the boy had visited him in the Fever Hospital, whence she might have carried the poison of typhus to her husband. The father, moreover, had been little exposed to the contagion emanating from the son, who, being a vagabond at variance with his father, *was from home when he was taken sick.*" The other exception admits of ready explanation. In November and December, 1851, four servants suffering from enteric fever were admitted from a hotel in the Haymarket. In the following January a servant was admitted with typhus from the same house. But the typhus patient was one of the four who had previously had enteric fever. She had only left the Fever Hospital about ten days when she was re-admitted, and the typhus by which she was attacked had doubtless been contracted during convalescence in the hospital. These observations have been verified at other of the London hospitals. Dr. Murchison quotes the experience of Dr. A. P. Stewart at the Middlesex Hospital, of Drs. Peacock and Wilks at the Royal

Free, St. Thomas's, and Guy's Hospitals, each of whom has invariably found the same distinction of origin. In Edinburgh valuable and, from the limit of the area of observation, even more crucial evidence has been given by Reid, Goodsir, Waters, Robertson, and Gairdner. The following extract is from Dr. Gairdner's work:—

"There is this advantage in a small field, that you can be much more sure of exhausting your facts. No one can pretend to have had access to all, or nearly all the fever cases of London, during however short a period. But in Edinburgh, Dr. Begbie and myself probably have seen, or have had the means of knowing about, very nearly all the fever cases; and therefore, when I declare to you that within my experience for ten years past, no instance has occurred of a decided origin of enteric fever in a group of typhus cases, or of typhus fever in a group of enteric cases, I am entitled to say that I have obtained very strong evidence in corroboration of the view that these two diseases are in reality different diseases, and not mere varieties of the same disease. Last summer I made a very careful survey of the whole fever-field of Edinburgh (if I may call it so) for several months together. It was not an epidemic season; but I gathered about thirty cases of typhus and twelve of enteric fever, and into the whole details of these I inquired with the greatest possible minuteness, visiting every one of the fever localities, except one or two in which I was quite sure that the cases were isolated. The result was that in no case could I light upon a suspicion that typhus had given rise to anything but typhus, or enteric fever to anything but enteric fever. The details of this inquiry were published in September, 1859." (pp. 119, 120.)

Since 1859, Dr. Gairdner has pursued the same line of investigation, and the results are in exact accordance with those to which he had been previously conducted. We might bring other testimony to bear out the same conclusion. Dr. Murchison quotes Dr. A. P. Stewart, to the effect that, during the years 1836–37–38, the latter saw upwards of 3000 cases of fever in Glasgow, but in no instance did he see typhus and enteric occurring together in the same family or house. It may, however, be argued, that one positive fact is worth a large number of negative ones, and that the little epidemic amongst the soldiers, and the cases of enteric and typhus observed by Huss in one house, cannot be easily set aside by any amount of non-occurrences. If we admit the accuracy of Huss's observations it by no means follows, however, that they warrant the conclusions which himself and Dr. Kennedy would draw from them. In the first place, if typhus and enteric fever arise from the same poison—if they are mere varieties of the same disease—instead of Huss's facts being derived from one small epidemic and one house, they ought to be furnished by every epidemic and in the majority of instances. The paucity and rarity of the facts is a powerful argument against the meaning which they are supposed to convey. They are perfectly explicable on the assumption of the occasional co-existence of two distinct poisons, but from their very fewness they are inexplicable on the assumption of the sole existence of one poison which is capable constantly of producing two diseases. No one contends that small-pox and scarlatina are the product of one morbid poison, because in a particular epidemic they are co-existent, or because, as in a case lately observed by ourselves, they have appeared in the same house within a few days of each other, and no such deduction can be safely drawn from the not infrequent prevalence of typhus and typhoïd fevers in one season, or the occasional, although very rare, appearance of the two diseases in the same house or family.

With regard to the occurrence of intermediate forms in the course of an epidemic season when both typhus and enteric fever are prevalent, we may safely refer the large majority of their examples to faulty diagnosis, dependent either on the absence of eruption or on an unusual development of the symptoms of uræmic poisoning (typhoïd symptoms) in enteric cases—by no means an unfrequent occurrence, and a probable explanation of the cases we have instanced in our review of Dr. Gairdner's work—or, as in the work of Huss, on the want of a just discrimination between the early stage of the rash of typhus and the eruption of typhoïd. But it by no means militates against the specific difference of the two poisons to admit that a person may at one and the same time be under the influence of both, and may thus exhibit signs and symptoms of a mixed character. This is no more than has been observed in the case of other diseases, such as variola and scarlatina, and variola and measles. In vol. xxx. of the 'Medico-Chirurgical Transactions' is a paper by Mr. J. F. Marson, referred to by Dr. Murchison, in which the writer states that he had seen in the Small-pox and Vaccination Hospital, in the course of eleven years, seven persons who had small-pox and scarlatina simultaneously; whilst he adduces a number of authorities who have noticed the co-existence not only of small-pox and scarlatina, but of small-pox and measles, small-pox and hooping-cough, and hooping-cough and measles. The old Hunterian doctrine, "that no two different fevers can exist in the same constitution, nor two local diseases in the same part, at the same time," has been long exploded, together with many other theories which the authority of Hunter invested with the dignity of doctrines. Dr. Murchison has demonstrated in his work, that scarlatina may be found occurring with enteric fever, and typhus may be engrafted on small-pox. Of the former, he has noticed 8 cases; of the latter, 1 case occurred at the Fever Hospital, under the care of Dr. Buchanan. Unless, therefore, these diseases have a common origin, the co-existence of typhus and enteric fever in certain rare cases cannot be urged in proof of their identity. Three instances of such co-existence have been noticed by Dr. Murchison at the London Fever Hospital. In two of these the patients were admitted with enteric fever, were exposed to the contagion of typhus in the wards, and contracted the latter disease. The third was originally a case of typhus. The earlier symptoms of the third case were a well-marked typhus eruption, composed of spots and mottling; a dry, brown tongue; confined bowels; heavy, confused expression; small pupils; and low, wandering delirium. The typhus eruption faded on the eleventh day, "and was succeeded by lenticular rose-spots, which came out in successive crops for more than a week, and were accompanied by diarrhœa, abdominal tenderness, red tongue, and dilated pupils." The patient had been subjected to the contagion of typhus, but had also been brought from an ill-drained and very offensive house. In the first case mentioned by Dr. Murchison, it was not until the boy had been exposed in the ward from June 3rd to June 17th, and on the twenty-second day of his illness, that the typhus eruption was superadded to the already existing rose-coloured spots of his original disease; the two were co-existent from June 17th to June 21st; the typhus "mottling" continued until June 26th. Such cases, and the frequent contraction of typhus by patients convalescent from enteric fever, who have been exposed in hospitals to the contagion of the former, is a powerful argument for the adoption of separate wards for each form of disease. It is manifest that the change of type observed by Huss and Kennedy in the same epidemic period is as explicable on the supposition of two poisons, the one exerting the greater influence during one portion of the period, the other at another, as of one poison modified and altered by some unknown cause. In the face of the vast mass of evidence accumulated in support of the essential difference of the two fevers, we think we are bound to adopt the former theory until there be distinct proof that the contagion of typhus can produce enteric fever, and that of enteric can give rise to typhus.

That there is nothing specific in the intestinal lesions of enteric fever, and that this is proved by rare and exceptional cases of ulceration of the ileum in scarlatina and small-pox, and by the occurrence of intestinal ulcers in phthisis, bearing some resemblance to the ulcers of typhoïd, is an argument which we believe will not stand investigation. Flint, Huss, and Dr. Anderson of Glasgow, have recorded four cases in which ulcers of the ileum occurred in scarlatina. Dr. Anderson has also related an instance of confluent small-pox in which there was the same lesion. As his cases, and the context in which they are mentioned, have a most important bearing on the subject under consideration, we shall here quote them in full:

"We now come to inquire what relations, if any, exist between typhus and the enteric fever. 'Glaring instances' of the diseases, as we have seen, are sufficiently distinct; their symptoms, course, and morbid anatomy being utterly diverse, except in so far as *any* two fevers may assume types and phases which bring them, therapeutically considered, into alliance. But what are we to make of cases like that which I shall now read to you from my note-book?

"J. F——, aged twenty-seven, a domestic servant, died on the eighteenth day of a fever characterized by the presence of a very copious and livid typhous eruption over the body and extremities. On inspection of the body we found the mucous membrane of the duodenum covered with enlarged solitary glands, softened and ulcerated, so that in some places the peritoneum alone remained of the coats of the bowels. Peyer's glands were enlarged throughout the greater part of the jejunum and ileum, a patch in the lower part of the latter being about five inches long, and an excavated ulcer existed in its vicinity. The small and large intestines were besprinkled with numerous enlarged solitary follicles.

"The generalizing faculty would readily conclude from this case that typhus and enteric fever are identical; but listen to two more:

"Jane S—— died of æsthenic confluent *small-pox*, at the period of maturation. On opening the body we found on the mucous surface of the ileum a number of enlarged Peyer's glands, two of which were ulcerated.

"Mary S——, aged sixteen, was admitted on the fourth day of fever. There was copious *scarlatinous* efflorescence; she had sore throat, and a tongue white and florid at the tip and edges. Two days after, she died, having passed a large quantity of blood by stool. On inspection we found the whole of the ileum of a deep red on its mucous surface, and thickly besprinkled with enlarged solitary follicles. Peyer's glands were also much enlarged and ulcerated, and many swollen follicles were scattered over the colon.

"Now can we suppose it possible that small-pox, scarlatina, and typhus are *all* identical with enteric fever, because these cases prove that the special lesions of that disease may coexist with each of them?—Thus, by a *reductio ad absurdum*, the position falls." (pp. 114, 115.)

Dr. Anderson then relates a case of typhus occurring during convalescence from enteric fever in proof of their non-identity, and proceeds to explain the cases we have quoted on the supposition that the lesions were the result of enteric fever "mixed up" with typhus, variola, and scarlatina. The coexistence of the latter group with intestinal lesion, he remarks, "is no greater proof of their identity than a man's having the misfortune to break *both* his arm and his leg by a fall would be a proof that these injuries were, in fact, the same." It will be observed, however, in reading these cases, that no mention is made of any enlargement of the mesenteric glands, a constant concomitant of the intestinal lesions of enteric fever. It may also be doubted whether the ulceration which was found were the real typhoïd lesion. In the case of fever the duodenum was extensively diseased, its solitary glands enlarged, softened, and ulcerated—a comparatively rare occurrence in enteric fever. In 15 cases examined by Jenner, and in 20 dissected by Murchison, there was no ulceration in this portion of the intestine. With regard to the cases of scarlatina and variola, it may be observed that not only in these diseases, but in cholera, erysipelas, and pyæmia the solitary and agminate glands of the ileum are occasionally enlarged, and may in solitary instances be found ulcerated, although the small, shallow ulcers then occurring are very different from the ulcers of enteric fever. It will also be observed that Dr. Anderson does not describe the characters of the ulcers he found. It is quite possible, however, that his own explanation is the correct one, and that in these instances two different diseases were simultaneously producing their effects. But when we turn to the positive evidence of the occurrence of intestinal lesion in enteric fever, and of its absence in typhus, the weight of proof, we think, becomes conclusive. During the past fourteen years many hundreds of bodies have been dissected at the London Fever Hospital, and in every case in which during the course of the fever successive crops of lenticular rose spots had been observed, the abdominal lesions of enteric fever were invariably present, whilst in every case marked by the characteristic rash of typhus the abdominal lesions of enteric fever were as invariably absent. Dr. Murchison asserts expressly that there have occurred no exceptions to these broad facts. It is true that in typhus the intestines are not always healthy, but what are the characters of the lesions they present? In 15 of 75 cases observed by Jenner and Jacquot, the duodenum presented various degrees of softening and injection. Softening and injection of the mucous membrane of the jejunum and ileum have also been observed in a small proportion of cases by the same investigators and by Reid. In 50 cases of typhus examined by Gerhard and Pennock, of Philadelphia, in 1836, Peyer's glands were entirely unaffected in every instance save one, and that a case of doubtful diagnosis. In this the agminated glands were reddened and a little thickened, but there was no ulceration or deposit in the submucous tissue. Of the cases examined by Dr. A. P. Stewart, at Glasgow, in 2 out of 21 the patches were slightly elevated; but he says expressly that the appearance was not that characterizing enteric fever. In Dr. Jenner's 43 cases, Peyer's patches were healthy in all save 3; in one there was tubercular ulceration, in a second there had been dysentery, and the inflammation extending high up the ileum had involved the mucous membrane covering the patches; in the third there was mere injection of one patch, but no ulceration. Similar evidence has been given by Drs. Peacock and Wilks, by Jacquot in his work on the typhus of the Crimean campaigns, by Barrallier in his history of typhus as it occurred at the hulks of Toulon. Barrallier observes: "Je ne jamais observé sur les 166 sujets nécropsiés pendant les deux épidémies du bagne, aucune des altérations des plaques de Peyer et des follicules de Brunner, que l'on rencontre dans la fièvre typhoïde." Godelier bears the same testimony with regard to 8 fatal cases of typhus examined by him in the hospital of Val de Grace; and lastly, Dr. Murchison, who has dissected 54 cases, asserts:

"In not one was there any deposit in or ulceration of Peyer's patches, at all resembling the appearances found after death from 'typhoïd fever.' In six only of the cases the glands were slightly more prominent than usual, but not more so than is seen after death from many diseases. In three of the six cases, the glands presented the appearance compared by French pathologists to a newly-shaven beard." (p. 233.)

With regard to Dr. Kennedy's case, in which the cæcum and ascending colon were the seat of ulceration, it may be observed that injection, inflammation, the effusion of lymph in patches, and ulceration of the large bowel, have all been observed in typhus. Dysentery is, in fact, a not unfrequent concomitant of the fever, and in the absence of the characteristic affection of the glands of the ileum, ulceration of the great intestine is no proof of close relationship between the typhus and enteric species.

Again, all observers who have had considerable opportunities of observing the peculiar alterations produced in the agminate and solitary glands in enteric fever, are agreed that they present essential differences from the tuberculous ulcers met with in phthisis. The first stage of the typhoïd lesion consists in the deposition of morbid material. This has been found as early as the end of the first day of the disease, although Trousseau's observations led him to conclude that it did not occur before the fourth or fifth day. The two varieties of the diseased Peyer's patch which are found about the eighth or tenth day of the fever—the *plaques dures* and *plaques molles* of French pathologists—are believed by Dr. Murchison to depend on the amount of the deposit which, with Goodsir, he asserts is observed *in the first place* in the interior of the glandulæ. In the *plaques molles* the deposit is small in quantity, and is retained in the glandules which give the rugose character to the patch peculiar to this form of affection; in the *plaques dures* the glandules burst and discharge their contents into the submucous tissue, and hence the patch presents a smoother surface. In the second stage ulceration commences. In the case of the *plaques molles*, the mucous membrane softens and becomes abraded in one or more places, which unite and extend into one large ulcer, which may extend to various depths through the subjacent tissue. In the *plaques dures* the whole of the diseased patch, with the superimposed mucous membrane, becomes detached either in one or more sloughs, leaving behind an ulcerated surface. Between the twelfth and twenty-first days these sloughs will frequently be found loosely attached to the ulcers, coloured yellowish-brown from the imbibition of bile, or having a dark spongy aspect from infiltrated blood. The third stage is the period between the separation of the morbid deposit or of the sloughs and the commencement of cicatrization. It is then that the typhoïd ulcer presents its characteristic appearance. Seated in the lower third of the small intestine, increasing in number as the ileo-cæcal valve is approached, they are of different sizes, from a line to an inch and a half in diameter, whilst in close proximity to the valve several ulcers may run together, producing an excavated surface of several inches in extent. If the ulcer correspond to an entire Peyer's patch, it is elliptical; if to a solitary gland, circular; or irregular in the case of the union of several ulcers or the destruction of only a portion of a patch. They are situated in the part of the intestine opposite the attachment of the mesentery, and their long diameter corresponds with the longitudinal axis of the gut. They do not form a zone encircling the intestine, as may sometimes be noticed in the case of tuberculous ulceration. Their margin is formed of a well-defined fringe of mucous membrane detached from the submucous tissue, which is well seen on floating the intestine in water. There is no thickening or induration of the edge, or deposition at the base of the ulcer, though sometimes fragments of yellow sloughs may be found adhering in these situations. Such is the description given by Dr. Murchison of the special lesion of enteric fever. Dr. Tweedie's account closely corresponds with it, and both descriptions entirely agree with that of Rokitansky. We need not describe the fourth stage of the ulcer—that of cicatrization. We have dwelt thus long on the subject to enforce the differences between these lesions and those of phthisis. In the latter, if the ulcerations be large, they extend round the circumference of the intestine; their bases and edges are thickened and indurated with tubercle, which gives them a cup-shaped appearance, and although the agminated and solitary glands of the ileum are affected as in typhoïd fever, and hence the *locale* of the two species of lesion is the same, yet in phthisis ulceration extends beyond the glandular patches, frequently to a considerable extent, and we frequently find tubercular depositions, the hard semi-cartilaginous-like granulations of Louis, or tuberculous granulations in the process of softening in other parts of the intestine, sometimes over its entire tract. Lastly, the cicatrization of the ulcers of enteric fever does not cause any diminution of the calibre of the gut, whilst, according to Louis and Corbin, tuberculous ulcerations frequently entail contraction of the gut around them.

That the occurrence of diarrhœa, intestinal hæmorrhage, tympany, and epistaxis, in a minority of cases of typhus fever, and of "typhoïd," and head symptoms in those cases of enteric fever to which we have already referred, is no argument for their essential identity, scarcely requires demonstration. Cerebral and the so-called "typhoïd" symptoms are not peculiar to any of the idiopathic fevers. They are in all probability directly caused by the imperfect elimination of the products of increased tissue metamorphosis from the system, and if the occurrence of convulsions or stupor, of low muttering delirium and contracted pupil, of dry brown tongue and sordes in cases of enteric fever, prove it to have the same cause as typhus, they will equally prove that all cases of uræmic poisoning are produced by typhus-poison. The symptoms have doubtless a common cause, but that cause may be the concomitant of any idiopathic fever, as well as of many other affections. So also hæmorrhages from the mucous tracts—petechiæ in the true sense of the term—diarrhœa, and tympany, are phenomena which are not in themselves specific. Petechiæ—the escape of dissolved hæmatine into the cutis—take the place of the true eruption of typhus in most cases of that fever, but they are neither essential to nor diagnostic of the disease; they occur in other fevers, often in relapsing, more rarely in enteric, and they are seen in the latter stages of various non-febrile maladies. It is true that the diarrhœa of enteric fever is peculiar in character, and occurs in a large proportion of cases. The yellow-ochrey appearance, offensive ammoniacal odour, and alkaline reaction of the stools, due, according to Dr. Parkes, to carbonate of ammonia, are characteristic. But diarrhœa is not constant. It was absent in 7 of 100 cases noted by Murchison, and in 4 there was absolute constipation. Similar observations have been made by every observer, and the same is true of hæmorrhage from the bowels, epistaxis, dilatation of the pupil, abdominal pain and tenderness, meteorism and gurgling. They are all commonly met with in typhoïd fever, but they are neither peculiar to it nor necessary to its existence. On the other hand, whilst constipation is the rule, it is no new observation that diarrhœa occasionally complicates typhus. Dysentery, as we have already noticed, may be its concomitant; although they are not frequently seen together in the epidemics of England, their coincidence has been observed in some epidemics in Ireland, and was common in the French army in the Crimea. When diarrhœa or dysentery occur in typhus, they may be attended with abdominal pain and meteorism; and especially if the disease be complicated with scurvy, but even without such complication, hæmorrhage from the bowels or epistaxis may also possibly be encountered. With regard to intestinal hæmorrhage, Dr. Tweedie's experience furnishes a well-marked case of typhus in which it proved fatal. He observes:

"Intestinal hæmorrhage is peculiar, or nearly so, to enteric fever, for although it may occur in ordinary typhus, it is very rare. I had a case of typhus in the hospital some time ago, in which this appeared to be the destroying cause; and on opening the body, *Peyer's patches were found to be sound*, but the mucous membrane of the lower portion of the ileum, cæcum, and commencement of the colon exhibited a red swollen appearance. In this patient there was the well-marked mulberry (typhus) eruption to assist the diagnosis. (p. 72.)

The intestinal hæmorrhage of enteric fever is only peculiar as an accident dependent on the specific lesions of that disease. When, as in Dr. Kennedy's thirty cases before referred to, it arises independently of any specific ulceration, it proves no closer connexion with enteric fever than it does with purpura hæmorrhagica, or malignant small-pox.

Neither does the protean character of enteric fever sustain in any degree Dr. Kennedy's argument. Typhoïd fever is, in the vast majority of cases, far more easy to discriminate than acute phthisis, and we may state on the authority of Dr. Walshe, that the diagnosis between acute phthisis and enteric fever is sometimes a matter of the greatest difficulty. But no one on that ground would argue that the two diseases had one and the same cause.

Dr. Kennedy, following Huss, founds an argument on the analogy of scarlatina, which, he asserts, presents just as striking differences in itself as do the typhus and enteric fevers. Without staying to inquire whether the differences in scarlatina are not differences in degree rather than in kind, there is one fact which affords a complete answer to this reasoning. The mildest case of scarlatina may give rise by contagion to the most malignant. The contagion of typhus has never been proved to give rise to typhoïd in persons exposed to it, nor that of typhoïd to typhus.

We have thus far confined ourselves to the special consideration of Dr. Kennedy's arguments. But there are differences between the two species of fevers which his reasoning leaves entirely untouched. In the first place, the history of typhus is a history of great epidemics. This is written on every page of the epidemiology of Europe from the times of Fracastorius of Verona and Cardanus of Pavia to our own. On the other hand, enteric fever, wherever it has been distinguished, has been found to prevail endemically over smaller areas. Our limits do not permit us to illustrate this point; we can only refer our readers to the admirable history of the two diseases compiled by Dr. Murchison. The geographical distribution of the two fevers, as far as it has been ascertained, is not the same. The great habitat of typhus has hitherto been the temperate latitudes of Europe and North America. It has prevailed in every country of Europe from the Crimea to Sweden, and although it has not been recorded as occurring amongst the Laplanders and Esquimaux, the writings of Schleisner appear to prove that it has often swept over Iceland. In Britain and in Ireland, it has ever been peculiarly rife, and it has also spread amongst the populations of Canada and the American States. It has not hitherto been observed in Australia or New Zealand, and there is no satisfactory evidence of its occurrence in Africa, Asia, or the tropical parts of America. It is true that cases of so-called typhus have been reported in India, Brazil, Mexico, and other parts of South America, but the descriptions given of the disease when critically examined do not answer to the well-known European typhus. We must allow that this argument will be weakened, if Dr. Murchison's opinion of the close analogy, if not identity, between typhus and bubonic plague be hereafter found to be correct. Unlike typhus, however, enteric fever appears to be a denizen of all countries and all latitudes. Not only in nearly every country of Europe is it endemic, but its existence in India is now established by numerous observers. Mr. Hanbury's account of the occurrence of enteric fever at Deesa is additional evidence on this point. It is true that in his cases the rose-coloured eruption was not observed; this he attributes to the fact that the patients were covered with prickly heat —lichen tropicus—but ulceration of the agminate glands was found in every autopsy, and the cause of the fever was clearly traced to open cesspools and sewers. Typhoïd fever has been seen by the French in Algeria, and has been noted by Griesinger in Egypt, by Oelsner in the Isle of Bourbon. The late Dr. McWilliam encountered it in the ill-fated Niger expedition, and there is also other evidence that it is one of the fevers of Sierra Leone. Enteric fever is endemic in North America, from Greenland to the Gulf of Mexico. It occurs in Central America and California, and is reported as extremely common in Brazil and Peru. The same fever has been also found in New Zealand and Van Dieman's Land. Again, typhus may occur at any age, but is essentially a disease of adult life; enteric fever, on the contrary, although not confined to, is most common during the periods of youth and adolescence. The contrast between the two fevers in this respect is amply shown by the records of the London Fever Hospital. The following table, constructed by Dr. Murchison from the register of 3456 cases of typhus and 1772 cases of enteric fever admitted during ten years well illustrates this point of difference.

	Per cent. of typhus cases.	Per cent. of enteric cases.
Under 10 years there were	5·78	6·04
„ 15 „	16·3	20·14
From 15 to 25 years	30·12	52·08
„ 25 and upwards	53·58	27·76
„ 30 „	43·66	14·22
„ 40 „	26·47	5·19
„ 50 „	11·92	1·46
„ 60 „	4·68	·5

Dr. Murchison argues justly that the real comparative liability to typhus at ages above thirty is much greater than these numbers at first sight show, because the total population above that age is scarcely more than one-third of that below, and in a certain proportion of individuals, the liability to take the fever will have been removed by their already having passed through it. In the case of enteric fever, on the other hand, it follows, from the smaller relative number of the population above thirty, that persons under that age are twice as liable to this form of fever as persons over thirty. Another point of difference is the duration of the two diseases. The mean duration of 53 uncomplicated cases of typhus in which recovery took place, observed by Dr. Murchison, was 14·13 days; that of 10 uncomplicated fatal cases was 15 days; but the mean duration of 75 cases of enteric fever which recovered was 24·6 days; and that of 12 fatal cases of the same fever was 22·08 days.

There is another most essential point of distinction between the two fevers, with which is bound up much of the practical importance of the whole question of their community or diversity of origin. We mean the difference in the activity of their contagious properties. The belief that typhus is contagious has now received such abundant support from such various sources that we shall make no extracts from the numerous facts which Dr. Murchison has collected in its support. The contagious properties of enteric fever, on the other hand, are so feeble in their development that they are denied *in toto* by many most competent observers, as Andral, Chomel, and others. Nevertheless, the cases collected by Dr. Murchison, although few in number, leave little doubt of the possibility of its being communicated by the sick. From them we extract the following:—

"In the year 1858, one of the nurses at the King's College Hospital, between twenty-five and thirty years of age, contracted well-marked pythogenic fever, and died. Immediately before her seizure, she had been engaged in nursing a patient ill of the disease. None of the other nurses or patients in the hospital caught the fever, which could not therefore be supposed to have a local origin."

"Some years ago two young men met in London: A. came from the Isle of Wight, where there was no fever; B. came from a village in Cambridge, where pythogenic fever was prevalent. B. was ill at the time of meeting. Both proceeded to Edinburgh, where B. had a well-marked attack of pythogenic fever. A. lived in the same house, and nursed B., and he also took the fever, although all the other residents in the house escaped." (p. 431.)

Another case, of even a still more convincing character, is quoted from Dr. W. Budd—

"On the 16th of December, 1858, a lad about twelve years old was brought from a boarding-school, at Cardiff, in the first stage of pythogenic fever, to his father's home, which was a farmhouse, situated on the crest of a hill, five miles to the west of Cardiff. Fever had not occurred at this farm within the memory of man. Before the arrival of the infected lad the family were in the enjoyment of good health, and the neighbouring village and farms were entirely free from fever; yet shortly after his arrival, three sisters, two brothers, two servants, and a hired nurse all contracted the fever." (p. 433.)

Yet the contrast in the activity of contagious property is well shown by the broad fact, that during Dr. Murchison's connexion with the Fever Hospital, only 2 cases of enteric fever have appeared to originate there, although 1048 cases have been admitted; whilst in the same period, 36 of the patients and attendants have contracted typhus, the number of admissions of that disease having been 2581.

Whatever the specific poisons of typhus and enteric fever may be, whether they originate *de novo*, or whether pre-existing germs are only called into activity under certain conditions, the conditions under which their genesis takes place or their activity is called into play, appear to be essentially different. For the origination or development of the typhus poison, the circumstances necessary are the over-crowding of squalid human beings with deficient ventilation, whilst among the most powerful predisposing causes are starvation and destitution. These facts are proved by the history of gaols, ships, workhouses in time of famine, beleaguered garrisons and armies. The circumstances giving rise to enteric fever, on the contrary, are neither overcrowding nor deficient ventilation, although the latter, by preventing the diffusion and dilution of the poison, may enhance its activity. The facts collected by Dr. Murchison go far to prove that enteric fever is contracted during exposure to emanations from sewage and putrefying fæcal or animal matter, or by such matters being conveyed into the system through the medium of drinking water. Whether, as Dr. Murchison believes, the poison of enteric fever is generated spontaneously by the fermentation of fæcal matter, or whether, as is maintained by Dr. W. Budd, the poison, although contained in the sewage, is always derived from the alvine evacuations of a person suffering from the disease, in which excreta the poison is concentrated, the drain or sewer becoming "a direct continuation" of the diseased intestine, is as yet an open question. Dr. Budd's view is certainly supported by the fact of the contagious properties of enteric fever, but it does not admit of proof, whilst we are utterly ignorant of the true nature of the poison. On the other hand, the arguments and facts adduced by Dr. Murchison against it are not readily answered. He instances several outbreaks in which it is most difficult to conceive that the poison could have been introduced into the drain, some cases in which the source of fever appeared to be the decomposition of sewage in drains which had been previously choked up and cut off from the general drainage, and others where enteric fever has arisen in isolated country houses and villages far away from any source whence it might have been imported. He argues, that enteric fever should more constantly spread in fever hospitals, where nurses and patients are constantly exposed to emanations from the night-stools, if the poison passed in its full potency from the diseased intestines of the patients. He states that he fed a pig for six months on the stools of patients suffering from enteric fever; the animal got fat, and when it was killed its intestines were perfectly healthy. The condition of the stools in enteric fever he supposes to be inimical to the existence of an animal poison, analogous to that contained in the pus of small-pox. The stools are invariably alkaline, charged with the products of the decomposition of animal matter in the form of ammonia and ammoniaco-magnesian phosphate, and they always exhibit the greatest tendency to decomposition or fermentation. The sloughs cast off from the ulcerated intestines are dead and putrid, and therefore, from the analogy of the poison of vaccinia and variola, should cease to exert any effect, even if we suppose the poison to reside in the morbid deposit. Lastly, he contends that the influence of temperature on the activity of the poison is more in unison with the supposition that it is the result of fermentation or decomposition, than with that of its existence as a specific poison derived from the sick.

These arguments are not conclusive; but they must be allowed to throw considerable doubt on the theory advanced by Dr. Budd and Professor von Gietl. On the other hand, there are sufficiently close points of analogy between all the exanthematous and continued fevers to suggest strongly the idea that their poisons, although specifically different, are generically allied. And whilst we have the best reason for believing that of small-pox to be organic matter, we shall not readily refer those of the other members of the group to the inorganic world. One thing is, we think, certain, that none of these subtle agents can have their origin apart from animal matter; they cannot be produced by vegetable decay, and it is yet to be proved that they can be imitated by the combinations of the laboratory. It is not sufficient to show that "patches of ulceration may be produced along the intestinal canal of a dog, by making it inhale sulphide of ammonium, as has been asserted by Dr. Richardson, although this is scarcely supported by Dr. Barker's experiments, who found that the intestinal mucous surface, under similar circumstances, was only "somewhat injected;" or that diarrhœa and vomiting should be observed in animals made to breathe sulphuretted hydrogen or cesspool gases: we must have evidence that a fever with a definite eruption, a regular course, the power of propagation by contagion, and the occurrence of which shall protect from a second attack, can be so generated, before the requirements of the problem can be held to be worked out. We would here, however, guard against being misunderstood: we do not contend that the poisons are anything more than organic matter, analogous, it may be, to the poison of hydrophobia or syphilis; we do not assert that they exist as distinct organisms, and we are ready to admit that there is the highest probability that they may originate *de novo*. The latter admission, however, is not fatal to the supposition that they are propagated as organized germs, at least it cannot be held to be so until the vexed question of spontaneous generation is finally set at rest. Dr. Barker, whose experiments on the gaseous poisons are of the highest interest, insists strongly on the fact that inorganic poisons, although they may produce symptoms which shall imitate those of the continued and exanthematous fevers, do not give rise to the diseases themselves. We quote two paragraphs from his concluding summary—

"The fact of the organic nature of the poisons (producing typhus and typhoïd fevers, cholera, and the exanthemata) is proved by their power of reproduction, their communicability from person to person, their destruction at a temperature extremely high, and the suspension of their activity as poisons at a temperature extremely low." (pp. 223–4.)

"Inorganic poisons are not competent to the production of communicable disease, the symptoms they produce being confined to the body in which they (the symptoms) are demonstrated." (p. 226.)

In this review we have examined the arguments for the identity of the poisons producing typhus and enteric fever, impressed with the conviction that it is of the highest importance that their difference, if it exist, should be acknowledged in all measures taken for the prevention or arrest of these diseases. We believe that the balance of evidence is in favour of the opinion that these poisons are different species, as distinct as the species of undoubted and complex organisms. Whether such gradationary varieties may exist between them as are at all analogous to those which in the vegetable and animal world make the definition of the terms species and variety the naturalist's difficulty, is a question which can only be answered by accumulated observations. It cannot be held to be at present settled in the affirmative. Practically, we are forced to the conclusion that the different species of fever, and their producing poisons, form well-defined separate groups, which cannot be confounded without abrogating all nosological distinctions.

Finally, there is an argument derived from the treatment of these fevers which bears strongly upon the question of their identity. It is thus stated by Dr. Tweedie, the therapeutical portion of whose lectures we should gladly have analysed for our readers had space permitted—

"The results of the treatment of the two forms should not be overlooked in discussing their identity. As a general rule, we find that remedies have a much more striking influence on the one than on the other. Stimulants are more early required, and to a much greater amount, in typhus; and if any local complication arise in enteric fever, depleting remedies are much better borne than in typhus. I remember Dr. Jenner remarking to me, as he investigated the records of the cases kept in the Fever Hospital, that when the leading characters of the two diseases had not been noted, or, at all events, so carefully as they have been within the last decennial period, he could, in the absence of the diagnostic symptoms, form a tolerably good idea of the type of the disease by the treatment that had been adopted; for while in the typhus cases considerable quantities of wine and brandy had been prescribed, in the enteric, stimulants had been either withheld or administered in sparing amount." (pp. 43-4.)

In this review it will be observed we have abstained from making use of the term "pythogenic," proposed by Dr. Murchison for enteric fever. Our reasons have been that the use of the word pledges to the reception of a theory which, however probable, cannot at present be held to be undoubtedly established, and we are adverse to the introduction of a new name in a nomenclature already redundant. The term "enteric," although in the first instance coined under the auspices of a mistaken pathology, in its present acceptation is connected with no theory, whilst it clearly and correctly distinguishes the disease.

There are many other portions of the works before us we had intended to notice; as the nature and affinities of relapsing fever; the necessity assumed by some writers, amongst whom is Dr. Anderson, of erecting the gastric type into a separate group, and the connexion of that type with enteric fever; the opinion held by Dr. Murchison of the close relationship, if not identity, of typhus and true plague; but these we must content ourselves with suggesting for the consideration of our readers. The whole field of continued fever is so vast, that it should be the purpose of the reviewer rather to indicate its features than vainly to endeavour to exhaust its soil.

REVIEW XIII.

Jaundice; its Pathology and Treatment, with the Application of Physiological Chemistry to the Detection and Treatment of Diseases of the Liver and Pancreas. By GEORGE HARLEY, M.D., &c.—*London*, 1863. pp. 136.

THE subject of jaundice is one on which a flood of light has been thrown by recent researches in minute anatomy, pathology, and chemistry. For though jaundice is no pathological entity, but only a symptom, yet, like albuminuria, it is a symptom so limited to affections of a particular organ, and associated with certain definite pathological conditions, that it as fairly constitutes a legitimate subject for medical discussion and analysis as if it were a disease *per se*. To the practical medical man it is well, indeed, to have the manifold causes of that combination of symptoms, which even the uneducated at once recognise as jaundice and an affection of the liver, clearly and fully set forth, and a differential diagnosis established, in order that he may obtain a clear conception of the actual lesion in existence, and apply to it a rational and successful treatment.

Dr. Harley enumerates twenty-six distinct causes of jaundice, a goodly number for the physician to bear in mind when he is confronted by a case, and has to determine to which among them the particular instance is due. Fortunately, however, with regard to some of the number, the diagnosis is easy, and in the case of all is facilitated by their being grouped under certain heads. Thus, in the treatise before us, jaundice is tabulated as occurring—1, in diseases of the liver; 2, in diseases of the bile-ducts; 3, in affections of other organs of the body exerting an influence on the biliary secretion; 4, in a variety of zymotic diseases; 5, as a result of the injurious effects of certain poisons.

This enumeration of causes is of utility in practice, still it is deficient in a pathological basis; thus, for example, the statement that jaundice is associated with zymotic disease, or follows as a result of certain poisons, affords no insight into the lesion of the hepatic function and structure which produces it, and which would rightly be referred to one of the previous groups of causes.

A classification of the causes of jaundice, we cannot help thinking them philosophical, is attempted by Frerichs—viz., according as this morbid state is owing: "1, to obstruction to the escape of bile; 2, to diminished circulation of blood in the liver, and consequent abnormal diffusion, both of these conditions giving rise to an increased imbibition of bile into the blood, and in both cases the liver being more or less directly implicated; 3, obstructed metamorphosis, or a diminished consumption of bile in the blood." In this account of the causes, Frerichs ignores, as Dr. Harley observes, the existence of jaundice as a result of suppression, and introduces "two perfectly new elements—namely, abnormal diffusion and diminished consumption;

the latter theory being, of course, founded on the supposition that bile, after playing its part in the digestive process, is reabsorbed into the circulation, again to perform another function in the animal economy before its final excretion as effete matter."

On the contrary, Dr. Budd recognised two primary causes of jaundice—viz., 1, mechanical obstruction, followed by absorption; and 2, suppression of the biliary secretion from some lesion of the liver, whereby the ingredients of the bile accumulate in the blood; and although "not prepared to admit the justice of the views held regarding the origin and function of the bile on which these views are based," Dr. Harley nevertheless believes the conclusions themselves have the support of modern research. Accordingly, when he proceeds to treat of the mechanism of jaundice, he does so under the two heads—1, of jaundice from suppression; and 2, jaundice from re-absorption. Under the former head is ranged jaundice from enervation, from disordered hepatic circulation, and from absence of secreting substance; whilst under the latter is placed jaundice from congenital deficiency and from accidental obstruction of bile-ducts.

The admission that jaundice may arise from suppression of the biliary secretion involves necessarily a belief that at least the colouring matters of the bile have a prior existence in the blood, and are only separated by, not formed in the liver itself. This view was ably sustained by Kühne,* who pointed to the altered hæmatin of the blood as the source of the colouring matter of the bile, or of the biliverdine of chemists. Hence, if the excretory function of the liver be arrested, the biliverdine accumulates in the blood, and produces the characteristic discoloration of jaundice, just as when the excretory action of the kidney fails, urea accumulates in the blood and causes uræmic poisoning.

This hypothesis of jaundice from suppression recommends itself by its simplicity, but there are difficulties in the way of accepting it entirely; and it has met with a formidable opponent in Frerichs. One difficulty that might suggest itself is the absence of jaundice in cases of acute hepatitis. In such instances, says Dr. Harley, only a portion of the liver is affected, and enough hepatic tissue is left to excrete the bile and prevent accumulation. And a similar explanation is offered to account for the absence of jaundice in many cases of congestion—viz., that some people have more liver-tissue than absolutely required for the wants of the system, an excess which compensates for defective action induced by disease. These explanations are not, we consider, sufficient and satisfactory. Animals which have had their livers removed are said not to have become jaundiced; and we can cite a case we have recently seen, in which there was suppression of bile, and yet no jaundice of the skin, and no bile passed in the urine. Granting that the bile colouring matter, or the biliverdine, owes its origin to the hæmatin of the blood, there must, in our opinion, be something necessary to determine the transformation

* Virchow's Archiv, vol. xiv. 1858.

of the hæmatin into biliverdine, and that this something must be looked for in the liver.

Dr. Harley supports Kühne in opposition to Frerichs, on the moot question of the occurrence of the bile-acids in the urine, and on that of the conversion of those acids into bile-pigment; and from the examination of these questions seeks to deduce some practical results of great value in diagnosis; and few will, we apprehend, contend, now that Hoppe's test for bile-acids is known, for the accuracy of Frerichs' opinion, that those acids are decomposed in the blood, and consequently never to be detected in the urine. However, their presence or their absence is not constant; and the author of this treatise on jaundice makes known to us "that in jaundice from suppression the liver does not secrete bile, consequently no bile-acids are formed, none can enter the circulation, and they are therefore not to be detected in the urine. In jaundice from obstruction, on the other hand, bile is secreted and absorbed into the blood, and the bile-acids . . . . are eliminated by the kidneys, or appear in the urine."

On this difference, therefore, in the constituents of the urine, Dr. Harley establishes a means of diagnosing the one type of jaundice from the other. On turning to Kühne's paper, we see that that experimenter remarks, that "in icterus caused by closure of the ductus communis choledochus, the urine *always* contains bile-acids and bile-colouring matter." He therefore appears to have recognised at least one half of the condition which Dr. Harley notes, if he did not further determine the other half, that, viz., of the absence of bile-acids in jaundice from suppression, as asserted in the treatise before us. Moreover, the test proposed is not absolute; for, as the author points out (p. 33), "in cases of acute atrophy of the liver, the jaundice, although chiefly due to suppression, is complicated with re-absorption of bile;" for the suppression is incomplete; and in the illustrative case cited bile-acids were found, on analysis, in the urine.

Frerichs pointed out, and Dr. Harley agrees with him, that the abnormal products tyrosine and leucine are never absent in acute atrophy, and, as the latter adds, are present also in chronic atrophy of the liver.

The theory of Frerichs, of the conversion of the bile-acids into biliary pigments is satisfactorily, we think, disposed of by Kühne and the author before us, who also disprove by experiments the statement of the first-named physician, that the biliary acids are not to be found in the blood even after bile has been injected into the circulation.

The detection of *melanine* in the urine is pointed out as a valuable diagnostic sign of melanotic cancer of the liver, and the importance of determining the relative quantity of urea and uric acid in the urine insisted on, and illustrated by a good example; but many readers would have been pleased to have had the differential diagnosis of jaundice in connexion with various lesions of the liver marked out in more detail.

On the matter of treatment, Dr. Harley has some valuable remarks and suggestions, premising that successful treatment must depend on the recognition of the existing cause, and its removal or mitigation. However, he does not deem it needful to consider seriatim each of the many ascertained causes, and to point out the course of treatment they severally require, but confines his "remarks chiefly to the therapeutical action of those remedies which we are constantly employing in the treatment of jaundice. The first remedy that merits special notice is mercury."

Whilst admitting the frequent benefit of this drug he deplores its indiscriminate employment in hepatic disease; and although he will not side with those who decry its use, because physiological researches have shown that it does not stimulate the liver to secrete bile, yet he is of opinion "that it has an important indirect effect in reinducing the biliary secretion, and thereby curing certain cases of jaundice." He regards mercury as a powerful antiphlogistic, lessening the volume of the blood by its purgative properties, and impoverishing it by its direct action in reducing the red corpuscles by its prolonged use. "From this it is easy to understand how mercury acts in inflammatory affections; and as in the majority of the cases of jaundice from suppression the stoppage of the biliary secretion is due to active congestion of the liver, mercury proves beneficial in such cases, not by stimulating the biliary secretion, but simply by removing the obstacle to its re-establishment—namely, the hepatic congestion." If this view be correct, it becomes at once evident that mercury, or any lowering medicine, must be injurious in obstruction of the gall-duct by hastening the fatal termination. Dr. Harley recognises it as established, that mineral acids and soluble alkalies rank first among substances that excite the flow of bile, and that the latter, particularly carbonate of soda, "are of the utmost advantage in preventing and arresting the deposition of gall-stones." This efficacy of alkalies is ascribed to their power of increasing "the amount of glycocholate and taurocholate of soda present in the bile, both of which substances, separately or combined, retain cholesterine in a soluble form, and, as is well known, by far the greater number of biliary calculi are composed almost entirely of pure cholesterine." The carbonate of soda has the further advantage of counteracting the uric acid diathesis, so commonly found in connexion with the formation of gall-stones. Dr. Harley particularly directs attention to the efficacy of benzoic acid in jaundice arising from suppression, given in the form of pill two or three times a day. Its *modus operandi* he cannot explain, though one thing seems clear—viz., "that it hastens re-absorption from the tissues, and elimination from the body, of the bile pigment." Podophyllin he finds a good purgative, and to increase the flow of bile, but, as a rule, prefers mercurials to it, where not contraindicated. In cases of obstruction it will be mischievous, and whenever biliary calculi exist in the gall-bladder its operation will be injurious, by increasing the flow of bile; there is "nothing more conducive to the deposition of biliary calculi than a well-filled gall-bladder."

The virtues of the vaunted medicines for effecting a solution of gall-stones whilst within the gall-bladder, Dr. Harley justly ignores from correct physiological principles; and he is bold enough to recommend, "in cases of permanent occlusion of the duct, in which there is great distension of the gall-bladder, the establishment of an artificial biliary fistula"—a recommendation, which, perhaps, but few practitioners will feel disposed to carry out, particularly as the cause of such occlusion is frequently, directly or indirectly, of more moment in the life of the patient than the mechanical closure of the hepatic duct it produces.

The foregoing remarks and extracts will indicate the general character of the work before us. It is not a specimen of book-making; it pretends to no exhaustive examination of the books and opinions of others, but places concisely before us the views of its author, and the experiments and observations on which they are based. We look upon it as a valuable contribution to the pathology of jaundice, and anticipate from the author's pen some more weighty work on hepatic disorders, when additional experience and experimental inquiry have supplied the necessary matter; for there are yet many debateable points even in the matter of jaundice to be determined, and, no doubt, much awaiting elucidation from the prosecution of physiology and chemistry combined, in the pathology of the liver and its secretion.

PART SECOND.

Bibliographical Record.

Art. I.—*A Manual of Elementary Chemistry, Theoretical and Practical.* By George Fownes, F.R.S. Ninth Edition. Edited by H. Bence Jones, M.D., and A. W. Hofmann.—*London*, 1863. pp. 820.

In every part this new edition shows marks of careful revision. All the deficiencies existing in the eighth edition have been noticed and supplied, while the results of recent chemical discoveries have been duly recorded in a concentrated yet intelligible form. We now regularly expect a new issue of this excellent and compact text-book at intervals of two years; and although we find on each occasion many new facts noticed, yet the volume has not yet attained that awkward obesity which has befallen similar works, and rendered their designation of "handbook" anything but appropriate.

The chief matters introduced into the present edition are the following: An outline of the method of spectrum-analysis, with an account of the important discoveries already made by its means, such as the detection of cæsium and rubidium, two additions to the alkaline metals made by Bunsen and Kirchhoff in 1860 and 1861, and Mr. W. Crookes' discovery, in March, 1861, of a third metal, allied to lead and silver. The elaborate researches of Mr. Graham on diffusion and dialysis are concisely noticed (pp. 136–138), with appropriate woodcuts of the apparatus employed. In Organic Chemistry, an account of the poly-atomic alkohols, and a chapter on the new colouring matters from coal-tar, are among the most conspicuous additions. A notice of Pettenkoffer's striking experiments on respiration will be found in the section on Animal Chemistry (pp. 734–735). Of the accuracy with which they were carried out, some idea may be formed from the fact that the carbonic acid and water produced by the combustion of a stearine candle in the apparatus could be weighed as accurately as in an organic analysis, while the apparatus itself was on such a scale that a man could breathe and move in it as in an ordinary dwelling-room, for twenty-four hours at least. The chapter with which the volume closes, on the Notation of Gerhardt, has been modified and enlarged in accordance with recent discoveries. The additions, indeed, in every part of the volume are numerous, but those to which we have specially alluded are perhaps of paramount interest. It is scarcely possible, for instance, to estimate too highly the importance of the process termed *dialysis*. Without doubt separations are continually being effected in the animal economy by means of the unequal diffusibility of "crystalloïds" and "colloïds;" so that this distinction throws light on several physiological actions, while it has already been employed in the preparation of pure aqueous solutions, otherwise unattainable, of peroxide of iron, &c. The method has also been practically applied in the arts, and has suggested an explanation of certain obscure geological effects.

ART. II.—*A Manual of Animal Physiology, for the Use of Non-Medical Students, &c.* By JOHN SHEA, M.D.—*London,* 1863. pp. 240.

LET the author speak for himself as to his object—"To present to the non-medical student a book by means of which the acquirement of some knowledge of physiology may be rendered comparatively easy." Admitting that the information is correct, so far as it goes, we submit that it does not go far enough to attain the object of physiology made easy. On the contrary, to our apprehension, its condensation is too condensed. The non-medical student may, however, use it as a kind of index of subjects by which he may be guided to larger manuals in preparing for examination. So large a range of topics as are embraced by the term "animal physiology," crammed into 232 small octavo pages, would perplex even a Carpenter.

ART. III.—*Elementary Treatise on Physics, Experimental and Applied.* By Prof. GANOT. Translated and edited from the Ninth Edition by E. ATKINSON, Ph.D.—*London,* 1862.

WE regard with much satisfaction the appearance of Ganot's excellent treatise in an English dress. Its information is recent, and its descriptions, for the most part, are readily intelligible; its style also is easy. There are some parts of the work where we cannot but think that further emendations and additions by Dr. Atkinson would have been of great service. For example, more attention might have been given to the labours of such English physicists as Tyndall, Graham, Gassiot, Grove, and many others we might name. It would not be just to this compact and handy volume to compare it with the much larger work of Jamin; yet, allowance being made for difference of scope and size, we think this new issue of Ganot's treatise will be found very acceptable as a text-book for schools and colleges.

ART. IV.—*Reports of the Manchester and Salford Sanitary Association for* 1861 *and* 1862.

CONSPICUOUS for energy and zeal among local sanitary associations is that of Manchester and Salford. It has been at work for several years, and has done no inconsiderable amount of good, and will do still more if it steadily perseveres in its philanthropic work. The average death-rate has fallen from 36 to 32 in the thousand—a fact which, while it is a proof of benefits already effected, only serves to show the large extent of hurtful influences to healthy existence that still prevail among the population, and continue to occasion an excessive proportion of sickness and mortality. During the last two years a new and very important feature has been added to the labours of the Association, and one which promises to be not only very useful in the carrying out of local efforts for the mitigation and prevention of disease, but also highly interesting to all sanitary reformers and medical statists. This is the regular weekly publication of all fresh cases of disease occurring in the practice of the parochial medical officers among the out-door and in-door paupers, and also in the principal charitable institutions for the relief of the sick, as well as of the births and deaths among the community, together with a record of meteorological phenomena. The value of such statistical information, by affording prompt and authentic intelligence of the amount and the character of all prevailing sicknesses, among the working classes more especially, and of their virulence and fatality as discovered by comparing the number of deaths with the number of attacks, was at once felt and appreciated. During the past year, when it was of more than ordinary moment that the health of the district should be correctly known, its usefulness was very strikingly shown; the weekly return served as a barometer of the health of the unemployed poor, and it was appealed to with confidence, not only by the inhabitants, but also by inquirers from a distance, as by Sir J. K. Shuttleworth, on behalf of the great Central Relief Committee, and by Dr. Buchanan, the medical officer deputed by the Privy Council to inquire into the sanitary state of the cotton districts. Had there been no regular registers of disease, the desired information could not have been given, beyond the uncertain and unsatisfactory estimates founded on the mortuary returns. Great praise is due to all who have assisted in this undertaking, and in an especial manner to the parochial medical officers of Manchester and Salford, for their hearty co-operation, for without their aid the work could not have been done. Their example has only to be followed by their brethren in other large towns, &c., throughout the country, and an inestimable boon will be conferred on medical science.

The disease and weather tables, compiled from the data in the returns, and projected in curves, show at a glance the prevalence of the principal zymotic diseases, &c., throughout the year, and will greatly facilitate the study of the natural history of these maladies in the district. Their value is not a little enhanced by being accompanied with similar tables illustrative of the health-state of one of the metropolitan districts, obtained from the monthly returns of disease in the parish of Marylebone, and furnished by Dr. R. D. Thomson, its able officer of health. "It will be seen," it is remarked, "that notwithstanding the differing influences of position, climate, manufactures, occupation, &c., bearing upon the inhabitants of the two places, there is a remarkable correspondence between the undulations of several of the (zymotic) diseases, showing the influence of some widely-spread agencies over their production and propagation." It is only by means of such statistical tables, coupled with topographical charts indicating the diffusion of these maladies and the localities and spots most affected by them in the course of the year, that we can hope to advance our scientific knowledge of epidemic and endemic disease. This subject well deserves the consideration of the Association of Medical Officers of Health of the metropolis, as regards the metropolitan area.

ART. V.—*Mentone, the Riviera, Corsica, and Biarritz, as Winter Climates.* By J. HENRY BENNET, M.D., &c. Second Edition.—*London*, 1862. pp. 288.

WE have so lately noticed the first edition of Dr. Bennet's little work, that we might legitimately have contented ourselves with merely announcing its re-appearance in a somewhat enlarged form. The present edition is, however, so distinctly an improvement upon its predecessor that we may, perhaps, be permitted to devote a little more space to it than we are usually in the habit of according to "Second Editions" of such works as this. In addition to the further experience of the Mentonian climate, which another year's residence in it has given the author, and which enables him not only to speak more positively as to its beneficial influence on phthisical patients than he did before, but also has given him the means of acquiring a fuller knowledge of the pathological conditions which are best fitted to profit by a resort to it, he has added considerably to the value of his work as a handbook for visitors, by the notes on the physical features and natural history of the locality which he has introduced into it. Thus we have a brief account of the geology of the neighbourhood, including a notice of the curious bone caverns which have only recently been discovered there. The birds, fish, and plants also come in for a passing notice in various parts of the book, indicating that Dr. Bennet is by no means an unobservant traveller.

But the most interesting *addendum* to its former contents is a description of a visit which the author made to the adjacent island of Corsica, for the purpose of discovering some spot which might serve as a suitable retreat for consumptive patients, for whom Mentone and the other winter resorts are too hot during the summer. Although not entirely successful in this respect, he had the good fortune to make a conquest of another kind, for the cause of sanitary science, in the discovery of a most eligible winter residence, which appears to be in every particular adapted for phthisical patients. This is the hitherto unfrequented, but charming little town of Ajaccio, on the western side of the island, which Dr. Bennet characterizes as "unquestionably one of the most lovely spots in Europe, and one of the cleanest and most smiling little towns he has ever seen." The possession of such attractions as a climate slightly warmer than that of Nice, an unexceptionably healthy locality, hospitable and intelligent society, and, to crown all, in the winter season, a very tolerable Italian-opera company, is quite sufficient to enable us to appreciate Dr. Bennet's assertion, that had he not already settled in Mentone he would certainly have himself migrated to Ajaccio. In fact, like Captain Macheath in the play, he appears ready to exclaim—

> "How happy could I be with either,
> Were t'other dear charmer away!"

The only drawback to the almost Elysian delights which Corsica holds out to the health-seeking traveller is the extreme prevalence in it, especially during the summer and autumn seasons, and on the eastern coast, of malaria. If, however, there is any foundation for the views which have been promulgated by M. Boudin and others, as to a natural antagonism between phthisis and ague, in favour of which, to say the least of it, some very forcible arguments have been adduced, it may be worth while considering whether consumptive patients might not find in the prevalence of intermittent fever to a certain degree an element of safety. At any rate, the objection which it presents does not appear to our minds nearly so grave as it apparently does to that of Dr. Bennet, whom we must congratulate upon his activity in opening out to the weather-bound *poitrinaire* yet new havens of retreat. If the man who makes two blades of wheat grow where only one grew before is a benefactor to his species, surely he who enlarges the resources of medical art, whether by the addition of new diagnostics for the discovery of disease, of new medicaments for its cure, or of new combinations of hygienic agencies for its prevention or arrest, is also entitled in no small degree to the gratitude of his fellow-men.

We will only say in conclusion, that Dr. Bennet has also appended to this edition a short account of Biarritz, and that it is supplied with a good map of Corsica, and an exceedingly attractive frontispiece representing a view of Mentone.

ART. VI.—*Chemistry.* By W. T. BRANDE, D.C.L., F.R.S.L. and E., and A. S. TAYLOR, M.D., F.R.S.—*London,* 1863. pp. 892.

THIS solid volume is, in some respects, a dictionary of chemical facts. The arrangement, indeed, is not alphabetical, yet the work will serve for reference, since a description of their most salient properties will be found appended to all the substances, the names of which are recorded therein. The volume has been prepared with some care, and embraces a notice of some of the important new additions to chemical science. Mr. Graham's discovery of the *dialytic* process, Professor Bunsen's detection, by means of spectrum analysis, of two new metals, *cæsium* and *rubidium,* followed, in March, 1861, by the announcement by Mr. W. Crookes of his discovery of a new metallic element, *thallium,* more remarkable in some of its properties than either cæsium or rubidium; all these, and many other fresh facts of interest and importance, are duly chronicled by Drs. Brande and Taylor. As might have been anticipated from the special reputations of its authors, this manual is full of valuable and interesting information regarding metallurgy, toxicology, and the properties of inorganic and organic substances used in medicine. Nevertheless, we are compelled to admit that there is a sad lack of system in the compilation; that facts of great moment, such as those of homologous and heterologous series, and of substitution, are scarcely recognised; that the formulæ assigned to many important bodies are inconsistent with their known relations to the substances whence they have been derived; that several compounds of high theoretical interest, whose existence hypothesis predicted and experiment verified, are either omitted or merely named; and lastly, that an unreasonable outcry is raised against all attempts to bring into order the chaos of chemical phenomena. We might support all the foregoing objections by very many instances, but the task would not be pleasant or profitable, and so we prefer to adduce but a single illustration to corroborate our criticism, while any of our readers who may desire further proof of its justness need not search long in the volume before they find it. According to Drs. Brande and Taylor (p. 316), olefiant gas is C_2H_2, while its chloride is C_2H_2Cl (p. 318).

By the retention of these obsolete formulæ not only is the connexion of olefiant gas with alcohol lost, but violence is done to the chemical and physical relations of the whole system of its own derivatives. According to the old notation even, the related formulæ are intelligible enough—C_4H_5O, HO for alcohol, C_4H_5O, C_4H_5O for ether, and C_4H_4 for olefiant gas; but the formula C_2H_2 disarranges everything. Again, though from marsh gas the hydride of *methyle*, C_2H_3,H, and therefore other compounds of methyle are readily made, yet our authors assign to this body (p. 309) the formula CH_2, thus dooming it to an unhappy isolation from its methylic brethren. Drs. Brande and Taylor, in fact, prefer empirical to rational formulæ, unless the latter are at least five-and-twenty years old. We commend to Dr. Hofmann's attention their remarks on "Ammonia," pp. 663, 664. They seem to have excluded triethyl-phosphine from their work on account of its unwarrantable assumption of the rank of an ammonia, though places have been found for barley-sugar and pink saucers.

ART. VII.—*Medicinskt Archiv utgifoet af Lävarne vid Carolinska Institutet i Stockholm.* Redigeradt af E. A. KEY, C. J. ROSSANDER, och S. G. TROILIUS. Första Bandet, första häftet.—*Stockholm*, 1863. 8vo, pp. 264.

Archives of Medicine, published by the Physicians to the Carolinean Institute in Stockholm. Edited by E. A. KEY, Professor of Pathological Anatomy; C. J. ROSSANDER, Adjunct in Surgery; and S. G. TROILIUS, Adjunct in Medicine.

THE new journal whose title is quoted above is intended to supply a want long felt in Swedish medical literature—viz., that of a medium for the communication of papers and essays too long for publication in the 'Hygiea,' without division among several numbers—a plan "disadvantageous to the reader, but particularly so to the author." The 'Medicinskt Archiv' will not appear at any fixed periods, but it is intended that two or three numbers shall be published every year, and that each number shall contain two or three essays, presenting as great a variety of subjects as possible. The first issue contains two essays: one by Oskar Th. Sandahl, "On the Effects of Condensed Air on the Human Organism in a Physiological and in a Therapeutic Point of View;" the other by Carl J. Rossander, "A Critique upon the Method of Operating for Cataract." These papers we hope to notice at length on some future occasion.

ART. VIII.—*A Practical Treatise on Dental Medicine; being a Compendium of Medical Science as connected with the Study of Dental Surgery.* By THOS. E. BOND, A.M., M.D., Professor of Special Pathology and Therapeutics in the Baltimore College of Dental Surgery. Third Edition, revised, corrected, and enlarged.—*Philadelphia*, 1863.

WE embraced the opportunity on a former occasion, while reviewing a work on the teeth, to notice the position of the dental profession in this country, and the efforts then being made for its organization. We are now enabled to record a settlement of the differences that prevailed at that time, and also to record the fact that the College of Dentists, which was commenced on the principle of absolute separation from the established medical corporations, has been dissolved. The only special qualification now to be obtained by dentists in this country is that of Dental Licentiate of the Royal College of Surgeons. The public in this have a guarantee that the dental practitioner is qualified for the exercise of his art, and can easily guard against falling into the hands of the ignorant and incompetent. We have ever considered that the practice of dentistry should be associated with the breadth-giving studies of surgery, physiology, and pathology. This view of the subject is expressed by the author of the present work, who says:

"The Baltimore College of Dental Surgery was organized with the design of teaching dentistry as a regular branch of medicine, in which relation only it can be regarded as a scientific pursuit, and the practice of it esteemed a profession. The study of particular parts as isolated from the rest, with a view to treat certain local affections as independent phenomena, has long since fallen into disuse, and every physician and surgeon is expected to become conversant with all of medicine as necessary to the proper care of any one of the organs of the body."

The author's object has been "to present to the reader a digest of information prepared with particular reference to the morbid connexions certainly existing between the teeth and the rest of the body," and he has succeeded in producing a work containing a large amount of well-arranged surgical as well as medical knowledge bearing on the special subject, all of which is of practical applicability. Though we were well aware that the microscope was not a favourite study with many of the American dental writers, we were scarcely prepared for the following on such a subject as the teeth:

"Unfortunately it has now become the fashion to study pathology in the corpse-house rather than by the bed-side, to make microscopical inquisitions of disease upon the dead rather than to observe its phenomena in the living.

From the very necessity of the case, this necrological research falls into the hands of the young and inexperienced, and these become writers and teachers before they have been to the only sure school of medicine—the chamber of the sick. As the teeth may be seen by the naked eye, they are not likely to be considered of much importance. Had they been discernible only by the microscope, they doubtless would have received due consideration. It is said that the celebrated Pennant, by the use of the microscope, lost the use of his eyes; I fear that this is too commonly the fate of his successors."

The result of such ideas is apparent in the book, as the following example will show:—"The enamel is a crystalline mineral substance, and possesses no vital organization." Independently, however, of such extreme views, or extremely expressed views, we are of opinion that the student, or young practitioner of dental surgery, will gain much by the careful reading of this work.

ART. IX.—*A Clinical Report on Cancer of the Female Sexual Organs.* By THOMAS HAWKES TANNER, M.D., F.L.S., &c.—*London,* 1863. pp. 60.

THE nature of this essay is what its title intimates—a tabulated clinical report of cases of cancer of the female sexual organs which have fallen under the author's own observation. The three principal sections are on "Cancer of the Breast," "Cancer of the Labia, Vagina, and Ovaries," and "Cancer of the Uterus;" and the whole is preceded by some introductory remarks on the prevalence of cancer in the population, and relatively to the two sexes, on its constitutional and hereditary nature and on its curability by operations. Dr. Tanner quotes approvingly the researches and opinions of Van der Kolk, as given in this Review (vol. xv. 1855), on the formation and extension of cancer, and from this circumstance and some other remarks, observes that though he looks upon cancer as being sooner or later a constitutional malady, he is ready to admit its origin as a local disease.

So far as the Registrar-General's returns are of value in determining the question of the prevalence of cancer and its relative frequency in the two sexes, they represent the deaths from cancer in 1860 to have been 6827, or 2100 males and 4727 females, and therein indicate a very much greater proclivity to the malady on the part of the latter. Dr. Tanner has compiled a table to show the age at which cancer is most fatal, from which one prominent fact is deducible:—"that up to the age of fifteen the deaths from cancer are pretty equal in both sexes. But when the female sexual organs get into full activity, and this activity has had time to tell, then the mortality from cancer in women begins to rise, until it becomes greatly in excess of that in men." On the question of surgical operation, he remarks, "If any good is to be looked for from the excision of a cancerous growth, it can only be expected when the disease is in an early stage, when it is merely of a few months' duration. The importance of removing every trace of the morbid tissue, as well as the difficulty of doing so, are points which have already been dwelt upon." (p. 9.) To carry out Van der Kolk's deductions into practice, Dr. Tanner advocates it as "essentially necessary after the operation (by extirpation) is over, to examine microscopically the edges of the portion removed, in order to ascertain whether granular matter, nuclei, or cells exist in any part of the tissue. Should this be found to be the case, we must conclude that the disease has not been wholly and completely removed; and the wound being still open, a further portion should be cut off in the situation where the cancer-cells and nuclei were seen." This practical deduction would be very valuable, were it a little more practicable in its nature; for as matters stand—as it is generally thought by the patient, if not by the operator, desirable to have an operation as speedily as possible brought to a close—and as cancer-cells are frequently not so well characterized as might be wished, few surgeons will, we fear, be prepared to adopt the above-mentioned procedure.

The author quotes a "Fell" case of removal by caustics of a mammary tumour, which he satisfactorily diagnosed to be not cancerous, but which the American adventurer pronounced to be so, and treated as such by his plan, the patient, however, succumbing under it, instead of living on as a standing advertisement for him of cancer cured without the use of the knife.

Dr. Tanner tabulates the history of seventeen cases of cancer of the breast which have fallen under his own care. One case of removal by the knife is entered as cured, the disease not having reappeared in any part for a period of twelve years after the operation. The woman was fifty-two years old, and the scirrhus was only of seven months' duration. Erysipelas followed after the operation, and Dr. Tanner is inclined to attribute the successful issue in this case in some measure to this incident. In support of this idea he quotes other cases, particularly those recorded by Velpeau, and remarks on the larger proportion of cures obtained by this eminent surgeon, that the only difference he can discover "between M. Velpeau's cases and those of other surgeons is this, that a much larger number of them suffered from erysipelas after the operation of excision than commonly happens." However, he does not purpose to go so far as M. Rigal, and advocate the introduction of the erysipelatous poison, but moots the question: "Is it as advisable to procure union by the first intention, after the excision of a cancerous breast, as is generally believed?" This question he answers in the negative by implying that it would be much more advantageous to have suppuration of the wound go on for many days after the operation.

Eleven cases of cancer of the labia, vagina, and ovaries are tabulated, and as many as ninety-two of the uterus. In these terrible affections he has found no good follow the various vaunted specific remedies, "and the same disappointment follows excision of the neck of the womb, whether this operation be performed with the écraseur, the knife, or the ligature."

Of the 92 cases of carcinoma uteri recorded, "80 were married, and 10 were widows or separated from their husbands." One single woman had seven years previously to the appearance of malignant disease had a child, and therefore only one of the 92 was unmarried and a virgin.

"Again, of the whole number, 12 had never been pregnant, one had had 'several' children, and 23 had aborted once or oftener. Then there were 79 women who had had 497 pregnancies amongst them, or an average of about 6½ to each. This is at least 2½ above the proportion in England. . . . . These results, which can scarcely depend upon accidental causes, seem to show that child-bearing predisposes in some measure to cancer of the uterus."

This brief notice of Dr. Tanner's essay will suffice to show that it embodies a large supply of facts bearing on the history of cancer in the sexual organs of the female, and reflects credit on the author's industry in rendering his experience available and useful to the extension of pathological science.

ART. X.—*The International Aspects of Quarantine Legislation.* By GAVIN MILROY, M.D., F.R.C.P., &c. From the 'Transactions of the National Association for the Promotion of Social Science, 1862.' pp. 12.

WE are much indebted to Dr. Gavin Milroy for his continued and able exertions to excite and keep alive an interest in the important subject of quarantine legislation. This his paper on its "international aspects" is his latest effort in the cause, we hope not his last. Our government, and *à fortiori* other governments, are very much, we fear, in that apathetic state about quarantine, that like the unjust judge in Scripture, there can be little hope indulged of moving them to give the existing system or systems (for there is no uniform one) consideration, except by importunity, iterating, and reiterating the manifold evils which quarantine inflicts.

In his present publication, Dr. Milroy presents a brief but clear summary of the manner in which quarantine is conducted in the different countries of Europe, taking his information chiefly from the documentary evidence contributed by her Majesty's Consuls and Governors of British Colonies at the suggestion of the Public Health Department of the National Association for the Promotion of Social Science in 1859. Having given some account of the results of the inquiry thus conducted in the number of our Review for April, 1862, we shall here only remark that, as in the instance of passports, to which Dr. Milroy aptly likens quarantine, in the same degree it would appear that governments have become enlightened and free, so nearly have they mitigated the rigours of quarantine, reducing it indeed in some instances almost to a negation. Holland, Denmark, Sweden, Belgium afford examples in point; whilst Spain, Portugal, Greece, resisting improvement, are standing examples of the contrary, as, indeed, were Italy and Sicily before the war of independence.

In considering quarantine, the purpose for which it was originally instituted should be kept in mind—viz., as a protection against the oriental plague, and how afterwards it was extended to yellow fever; next and last to cholera; its restrictions being confined to these three diseases, the contagious or infectious nature of two of which appear to be at least doubtful. Further, it should always be kept in mind that the working of the system has been conducted too often in the most arbitrary manner, and for the worst political purposes; and that generally, whilst it has been most vexatious and injurious in operation, it has afforded no security against the invasion of those very maladies it was intended to exclude.

Dr. Milroy suggests the appointment of one or two Commissioners by our Government, with instructions to visit the ports of those countries in the south of Europe where quarantine continues to be most rigorously enforced. No doubt, in the way of enlightenment, the results would be beneficial, for we can only expect, from an accumulation of evidence, that degree of conviction that will in a manner compel a reform of the abuses.

The period we have now entered—the era of the steam-boat, the railway, the electrical telegraph—seems specially to need definite and clear views, carried into practice, in this matter: the one, the rapid locomotion being so favourable to the transmission of disease, if communicable from person to person; the other, the momentary transmission of intelligence, securing, or capable of so doing, exact information respecting the health condition of every region within the range of its magical circle.

We are glad to find that a deputation from the Council of the National Association which has waited on Lord Palmerston, urging the institution of such a commission, has been favourably received. Were its duties to be performed honestly and ably, how great would be the good resulting compared with the cost; in the way of commerce it might be equivalent to the gain of millions, free transit being the life of commerce; in the way of humanity, the spread of knowledge, the removal of prejudices and aversions, founded on ignorance, the accompaniment of isolation, the benefits ought to be beyond all price.

ART. XI.—*Memoir of the Life and Writings of Robert Whytt, M.D., Professor of Medicine in the University of Edinburgh from* 1747 *to* 1766. By WILLIAM SELLAR, M.D., F.R.S.E., &c. (From the 'Transactions of the Royal Society of Edinburgh,' Vol. xxiii. pp. 33.)

FOR this able memoir of a distinguished man, its author has been awarded a medal by the Royal Society of Edinburgh, before which it was read.

It is a pleasure to see justice done, and that in the amplest manner, to deceased merit, to a man who, Dr. Sellar justly remarks, was in advance of his age, and who "stands amongst those whose labours most largely contributed to the rapid strides of medicine in the latter part of the eighteenth century."

This memoir is not only interesting as affording an excellent account of these labours, but also as containing a sketch of the rise and progress of the medical school of Edinburgh, of which Dr. Whytt was one of its earliest ornaments.

His chief discovery was connected with the nervous system, anticipating as he did to a great degree that which now bears the name of reflex nervous action, and using the term sentient principle to signify what a *vis nervosa* is now employed to express. And more, he made the happy conjecture that the above-named nervous action is conveyed to and from the sensorium commune by isolated nervous fibrils; this is a conjecture in relation to its verification not unlike that of Watt regarding the composition of water established experimentally by Cavendish.

In reading this memoir, we have been reminded of the curious and very interesting fact that Dr. Whytt's writings on the use of lime-water in calculous complaints, and the controversy which he had with a brother professor on the qualities of lime-water, led to the great discovery of Dr. Black—that of fixed air (the germ, may it not be called, of modern chemistry), now as a science so vast in its amplitude and relations. And this again reminds us of the short space of historic time which has intervened between then and now—barely a century. Dr. Whytt died in 1766, the second Monro was his colleague and the colleague of Black, and there are some still alive, Dr. Sellar assures us, who attended the prelections of the former.

ART. XII.—*Practical Remarks on Laryngeal Disease, as Illustrated by the Laryngoscope.* By E. H. SIEVEKING, M.D., Physician to St. Mary's Hospital. 1862. (Pamphlet.)

THE observations embodied in this pamphlet were read before the Harveian Society of London, and subsequently appeared in the 'British Medical Journal' (December, 1862). Their object is to set forth the advantages accruing to us in our diagnosis from the use of the laryngoscope, and to explain the mode of using the instrument. We have read the pamphlet with much satisfaction, and would highly recommend it as being most clearly and pleasantly written, and as giving in very short compass much valuable aid to those who have not yet tried laryngoscopy, or who have hitherto found themselves unable satisfactorily to practise it.

ART. XIII.—*A Practical Treatise on the Ætiology, Pathology, and Treatment of the Congenital Malformations of the Rectum and Anus.* By WILLIAM BODENHAMER, M.D.—*New York* and *London,* 1860. pp. 368.

THIS work constitutes one of the most complete monographs with which we are acquainted. In it we have collected together and apparently with accuracy, no less than 287 cases from every available source, and also a review of both the "medical and surgical treatment in full from the earliest times, with all the improvements down to the present time." The date of the volume will explain why allusion to the more recent observations of our countryman, Curling, has not been made.* After an introduction, in which the ætiology, anatomical and pathological character, classification, symptoms, and prognosis of these congenital malformations are considered, our author devotes separate chapters to the nine species of malformation which he discriminates, and this classification will be seen by our readers to differ from that adopted by Mr. Curling. He divides them as follows into—I. Malformation characterized by an abnormal narrowing of the anal orifice. II. Atresia orificii ani, in which we have a closure of the orifice by a thin transparent membrane, somewhat resembling a hymen. III. Atresia ani et intestini recti, a species of arrested development, in which the rectum terminates abruptly in a "cul de sac" at a variable distance above its outlet. IV. Malformation, in which the anus is usually quite normal, whilst the rectum, at a variable distance above it, is either obliterated, partially or wholly absent, or occluded by a thin or thick annular membranous septum. V. In this, the rectum near its lower end sends off a *pipe-like* prolongation, which terminates externally in a preternatural orifice at some point in the perinæum, or at various points beneath the urethra, at the labia pudendi, or at some points in the sacral region. VI. In this form, the rectum terminates by an abnormal anus, either in the bladder, urethra, vagina, uterus, or in a cloaca in the perinæum with the urethra and the vagina. VII. In these, the ureters, vagina, or the uterus may terminate in the rectum by an abnormal orifice. VIII. The rectum in this species is entirely absent, and the colon generally ends in a "cul-de-sac." IX. In this the rectum and the colon are both wanting, and some other portion of the intestinal canal terminates externally in an abnormal anus.

The volume closes with a chapter on Abdominal Artificial Anus, including its history and the various proceedings resorted to by different surgeons.

The materials collected and introduced into the work are well digested, with full and particular references to their sources; and in the relation of the various cases, the remarks of the original writers are given in connexion with them. The volume is well illustrated by sixteen plates, each containing several drawings. Altogether, the book is a full and useful compendium on the subject up to the time of its publication, and a monument of no little industry and patience.

ART. XIV.—*On Diseases of the Skin.* By ERASMUS WILSON, F.R.S. Fifth Edition.—*London,* 1863. pp. 784.

THIS work has attained, since its first edition near twenty years ago, very considerable proportions, and the present edition, as the author observes in his preface, contains "numerous emendations and additions, both in pictorial and typographical illustration." In our notice of the former edition, we took occasion to regret that Mr. Wilson confined himself too much to the local phenomena of skin diseases. We still think that

* See our number for April last, p. 366.

he does so, for whilst in his preface at one part he has some capital observations regarding the necessity of "devoting more attention than heretofore to local treatment, and not less to constitutional treatment," and again speaks very discreetly of the necessity of considering the "solids" as well as the humours in our study of skin diseases, yet when he comes specifically to deal with the diseases *seriatim*, we still fail to meet with that complete notice of them which is only obtained by regarding the local phenomena in full connexion with general symptoms and with the condition of the various organs of the body in this disturbance of their functional activity. High praise is yielded in the preface to Professor Hebra, of Vienna. Following most eminent authority, our author discards the term lepra as we are wont to use it in England, and restricts it to the "leprosy" or "elephantiasis." He strictly couples the so-called lepra with psoriasis, and calls it by that name. Moreover, with Hebra, he gives to "eczema" a wider range than is commonly done, refusing, as did Bateman and others, to give to that disease the term psoriasis in such cases as become chronic, and are accompanied by considerable thickening and extensive desquamation of the diseased patch.

ART. XV.—*A Compendium of Domestic Medicine, and Companion to the Medicine Chest; comprising Plain Directions for the Employment of Medicines, &c. With an Appendix on Cod-liver Oil: its Purity, Mode of Preparation and Administration.* By J. SAVORY, Member of the Society of Apothecaries, London. Sixth Edition.—*London*, 1862. pp. 378 and 35.

THE author tells us that in this edition "he has made such corrections as may be considered fairly established by the progress of medical knowledge." The work evidently contains a large amount of medical information which in every situation of life, provided that no regular practitioner is at hand, may be found most useful. The portions devoted to "cookery for the sick," and to "medicinal articles," will no doubt, under many circumstances, be especially valuable; and several practical hints may be gained from the chapter on Cod-liver oil.

ART. XVI.—*A Practical Handbook of Medical Chemistry.* By JOHN E. BOWMAN, F.C.S. Edited by CHARLES L. BLOXAM. Fourth Edition.—*London*, 1862. pp. 303.

FEW students of medicine, we suppose, are without a copy of one or other editions of this valuable and handy work, and possibly there are but few of our younger fellow-practitioners who do not find it still a useful book for reference. On this supposition it can hardly be necessary for us to offer any criticism on its merits, and we would only remark, in reference to alterations and additions, which may be observed in the present edition, that processes have been introduced for the quantitative determination of kreatinine and ammonia in the urine; that the application of the volumetric principle to the analysis of the urine has been extended; that short practical directions for the examination of the solid excrements, bile, and the liquids of muscular flesh, have been added; that the electrolytic method of the detection of metallic poisons has been fully described; that short chapters on the detection of strychnia, nicotia, phosphorus, and alcohol, in organic mixtures, have been subjoined; and that concise directions for the application of Professor Graham's process of dialysis to the separation of poisons from organic mixtures, have been included.

ART. XVII.—*A Practical Treatise on the Medical Properties of the Aix-la-Chapelle Hot Sulphureous Waters, and the Mode of their Employment.* By L. WETZLAR, M.D.—*Aix-la-Chapelle,* 1862. pp. 100.

DR. WETZLAR, in his preface, informs us that this brochure has been produced in compliance with the wishes of English patients and medical friends, who desired to have in English a work like the author's 'Traité Pratique des Propriétés Curatives des Eaux Thermales d'Aix-la-Chapelle,' &c., which our readers may remember that we noticed with much satisfaction in our number for October, 1862. The chief interests attaching to the French work was no doubt owing to the '*cases*' and their results, by which the author's views as to treatment, &c., were established. The present work contained double the number of cases which its predecessor did, and several of the new cases are of considerable importance, especially those of progressive muscular atrophy, syphilitic cerebral affection, waxy degeneration of the liver, &c. We observe that the author, whilst instructing us as to the chemical and physical qualities of the St. George's waters, the effects of the springs, and the mode of employing them, and describing the disease, &c., which indicate their use and those which contra-indicate it, acknowledges that he "has not tried to explain their mode of action," as he did not wish "to enter into forced theories." On this score he thinks that our knowledge of the mode of action of mineral waters might be advanced by the analysis of the urine if carried out on a large scale in a hospital "for poor bathers," who would be under constant and attentive control. We gladly recommend Dr. Wetzlar's English work, as we did the French one, embodying as it does the chief observations resulting from an intelligent experience of not less than twenty-nine years.

PART THIRD.

Original Communications.

Art. I.

On the Influence of Sex in Hereditary Disease.
By William Sedgwick.

(*Concluded from our last.*)

With respect to many of the cases which remain to be considered, affecting organs situated in the chest and abdomen, there is an obvious difficulty in tracing their history, owing to the vague statements and the uncertain knowledge of the patients themselves concerning the family history of such diseases, and the limited opportunities afforded us for testing their accuracy. It is proposed, therefore, to pass over very briefly the consideration of such cases, as it is not from them that much information can be derived regarding either the extent or the limits of morbid inheritance; and consequently a few well-marked cases of pectoral and abdominal diseases, with such other cases as may appear to be useful for the purpose of exhibiting the influence of sex, will be sufficient to complete this division of the subject.

Among the illustrations of the influence of sex in hereditary diseases of the respiratory organs may be mentioned the frequency with which consumption has been observed to be so restricted in its development. Reference was made in a former paper on the subject* to the statistics obtained from the Hospital for Consumption at Brompton, which, to a certain extent, confirmed this point; but, as statistics have merely a limited value in a subject of this kind, it will be desirable to quote some individual cases of the disease, in order to show the extent to which this influence of sex prevails. In a case at present under my observation of advanced phthisis in a woman, aged forty-two years, the mother died of cancer of the womb at the age of fifty-three years, and the father died of asthma at the age of fifty-seven years, whilst the paternal grandmother died of phthisis; this patient is the seventh child of a family consisting of five sons and six daughters. Of the sons, one aged forty-seven years, and another aged forty years, are in good health; of the daughters, one died of phthisis at the age of twenty-four years, and another at the age of thirty-one years; the other children died in infancy, or soon afterwards. In this case it is

* British and Foreign Medico-Chirurgical Review, July, 1861, p. 208.

to be noticed that the disease was transmitted by atavic descent from the paternal grandmother to three grand-daughters, and that although there were five grandsons in the family, none of them inherited the disease. In another case which has come under my observation, a labourer, aged forty-four years, suffers from phthisis affecting the right lung; he is the third child of a family of seven children, consisting of six sons and one daughter. The eldest son, at the age of twenty-four years, and the fourth son at the same age, both died of phthisis; the father died of dropsy, at the age of fifty-four years; and the mother, aged eighty-three years, is still living; the latter has had one brother and two sisters. The brother died of phthisis at the age of thirty-eight years; one sister died at the age of forty-four years "of some disease not affecting the chest;" and the other sister, between ninety and one hundred years of age, is still living. On the father's side, three first cousins, two male and one female (being the children of the father's sister), as well as a son of this female cousin, all died of phthisis. In this case it is to be noticed that although, with one exception, phthisis was limited to the males, yet it existed both on the father's and on the mother's side, and whilst it might be assumed that this patient and his two brothers derived the disease by atavic descent from a male ancestor of their mother, since a maternal uncle died of it, yet it is more reasonable to suppose that it was, by atavic descent, chiefly derived from a male ancestor of their father, since four collateral relations on the father's side were also affected. Gintrac* records the case of a woman, dead of phthisis, who was twice married, both husbands being robust and dying of accidental maladies. By the first husband there were two children, a son and a daughter, who enjoyed the best health; by the second marriage she had four daughters and one son. The four daughters all died of phthisis, whilst the son, aged forty years at the date referred to, was quite healthy. Dr. Cotton has informed me of the case of a family which consisted of a father, mother, five daughters, and a son. The mother and four daughters died of phthisis; the father, son, and one daughter are now living; the father and son are quite free from the disease, but the daughter is affected with scrofula in a marked degree. In the case of Mrs. Pryor, suffering from phthisis, observed by Dr. Campbell† at the St. Marylebone Dispensary, the mother and two maternal aunts died of phthisis. In this case it will be noticed that as three sisters died of the disease, the inheritance may be referred further back. Girou‡ records the case of Madame C., threatened with phthisis, who was the mother of six daughters, all of whom died of the disease; and in another similar case, that of Madame D., two daughters out of four died of phthisis. In the following cases, observed by Dr. Greenhow,§ the unisexual development of the disease may be referred

* Mémoire sur l'influence de l'Hérédité sur la production de la surexcitation nerveuse, &c.: L'Académie Royale de Méd., Mémoires, tom. ii. 1845, pp. 199–200.

† Observations on Tuberculous Consumption, p. 379. 1841.

‡ De la Génération, p. 293. 1828.

§ Medico-Chirurgical Society, March 25th, 1862 (Medical Times and Gazette, April 5th, 1862, p. 362.)

to atavic inheritance; and at the same time it is to be observed that in all of them asthma affected the immediate parent from whom the inheritance was probably derived, for in the first case of phthisis cited, in which three daughters inherited the disease from their grandmother through a parent affected with asthma, and also in other cases of disease which have come under my observation, there seems to be an antagonistic relation between phthisis and asthma, which assists in maintaining the influence of sex by limiting the development of the disease.

"An old couple," observes Dr. Greenhow, "who both survived to the age of more than eighty years, lost all their sons by phthisis, whilst their daughters escaped. In a second case, in which also both parents survived to an advanced age, the five daughters all died of phthisis, while the sons were exempt. In a third case, six out of seven daughters died of phthisis under thirty years of age; the seventh went to India, where she also died about the age of forty years; but the only two sons are alive and well. In none of these cases was either parent phthisical; but it is worthy of note that all three mothers were asthmatical, the fathers being in each case healthy."

With respect to other affections of the respiratory regions, Gintrac* observed a case of spasmodic constriction of the larynx in a father and son. Lefebre† informs us that he derived the complaint from his father. Alibert‡ relates the case of a boy, aged seven years, who experienced difficulty of breathing at the beginning of winter, and whose father had died of asthma; and he knew a family in which the brothers became asthmatic at the age of forty years, indicative of the atavic inheritance of the disease; for, as Sir John Forbes justly observes,§ "In most of the cases the history of which we have investigated, while we have rarely failed to find some one of the near relations or progenitors of the patient subject to asthma, we have very often found the immediate parents to have been free from it;" and Floyer|| relates an interesting case of a man who was attacked with asthma at the age of twenty-five years, and whose "grandfather had the asthma, and died of it about the fortieth year of his age; but his father and mother were very healthful, and never had any asthma." With regard to the female sex, I have at present under observation the case of a woman, aged forty years, who has suffered severely from asthma since the age of thirty years. Her mother, still living, has also suffered from the same disease since the same age. Duchamp¶ observed a case in which a mother and her daughter were similarly affected. Piorry** records a case of hereditary asthma affecting a mother, daughter, granddaughter, and great-granddaughter; and another case, in which a woman suffering from asthma had lost her father, mother, brother, and two sisters, showing that, as both parents had suffered from the dis-

* Op. cit., pp. 324–5. † Journal Hébdomadaire, tom. iii. p. 97. 1835.
‡ Nosologie Naturelle, tom. i. p. 243. 1817.
§ Cyclopædia of Practical Medicine, art. "Asthma," vol. i. p. 196.
|| A Treatise of the Asthma, second edition, p. 107. 1717.
¶ Maladies de la Croissance, p. 108.
** It appears from the statistical researches of this writer, that of thirty-two cases of asthma admitted into the female wards of the Salpêtrière, in twenty-two it had been transmitted by the parents (De l'Hérédité dans les Maladies, p. 101. 1840).

ease, the children of both sexes had alike shared in the inheritance. Whilst M. Cazeau* relates a case in which three children (sex not stated) of a physician are subject to frightful and unaccountable attacks of suffocation, which are recovered from spontaneously; their father had suffered in the same way during his infancy.

As illustrations of the influence of sex in hereditary diseases of the circulatory system, Lancisi's† cases may be cited of aneurysm of the heart affecting in succession the great-grandfather, grandfather, father, and son. Fodéré‡ relates a case of aneurysm of the heart in a father and son; the daughter was exempt. For the following case I am indebted to Mr. Ernest Hart:—The father suffered for several years from shortness of breathing, believed to be of an asthmatic character, but probably secondary to mitral disease of the heart, of which he died suddenly at the age of forty years; he left three sons and two daughters; the second son died suddenly of mitral disease, at the age of fifty-three years; five days after his death, the youngest son died suddenly on the doorstep of the house in which his brother's widow resided, having for a long time suffered from pulmonary congestion with narrowing of the mitral aperture; the eldest son died at the age of sixty-three years, of softening of the brain; the two daughters lived to an advanced age, and were both free from heart disease. Mr. Chapman, of Hounslow, has lately informed me that he rejected a man for a benefit society on account of great hypertrophy of the heart, two of whose brothers had previously died of heart disease. Otto§ refers to a case of rupture of the heart in two brothers. Sir Henry Holland|| records the case of four brothers who died, between the age of sixty and sixty-five years, of organic disease of the heart, with prior cases of the same kind in their family; and it may be observed in connexion with this latter case, that the influence of sex is often combined with that of age in hereditary affections of this organ, and notably so in cases of angina pectoris, which disease appears to have been formerly known by the less scientific name of "rising the lights"—a name retained in the bills of mortality until a comparatively late date. Mr. Joseph Adams¶ knew three brothers attacked with angina pectoris, each as he arrived at the age of about eighteen years, and to each of whom it proved fatal in a few months; such of the children as grew up to manhood escaped the disease. Mr. Adams** was also personally acquainted with another family "in which three brothers were each in succession attacked with symptoms of angina pectoris between the age of forty-seven and fifty. . . . . Those who escaped to the age of fifty remained free for the remainder of their lives, and none were attacked earlier." The females escaped,†† "whether," Mr. Adams observes, "by a greater attention to

* Gazette des Hôpitaux, tom. viii. p. 27. 1846.

† Observations on Aneurysm, translated by John E. Erichsen, pp. 55-6. 1844. Sydenham Society.

‡ Traité de Médecine Légale et d'Hygiène Publique, tom. v. pp. 378-9. 1813.

§ Pathological Anatomy, translated by South, p. 280. 1831.

|| Medical Notes and Reflections, third edition, p. 33. 1855.

¶ Hereditary Peculiarities of the Human Race, second edition, p. 20. 1815.

** Ibid., pp. 28-9.

†† It is necessary to notice that disease of the heart is more common in males than

the changes of that age, it is not in my power to determine." Prosper Lucas* relates the case of a woman, aged fifty-five years, who suffered from angina pectoris, which commenced in her at the change of life; her mother suffered from the same affection from the age of thirty-five years, being about the same period of life in each. And as I have before had occasion to observe in other forms of hereditary disease, in the same way that either limitation may occur without the other, so also in angina pectoris, the influence of age may be independent of that of sex, as in the case recorded by Dr. Robert Hamilton,† of a private in the 10th Regiment, aged twenty-three years, who suffered from angina pectoris, which began in him at the age of twelve years. "His father, two of his brothers, and a sister, were all attacked with it about the same age with himself, and to all of them it proved fatal." Otto‡ informs us that he knew a woman who died of disease of the heart, in the prime of life, having had two daughters affected with disease of the heart, one grandchild the subject of blue disease, and another which from youth had a violently pulsating heart; but the sex of the grandchildren in this case is not stated. So likewise in the case observed by Dr. Baird,§ of irregular distribution of the radial artery, interesting with reference to the pulse, in which three persons belonging to the same family presented the same anomaly, the sex is not given. Dupuytren‖ observed a well-marked case of hereditary hæmorrhoïds in a broker aged forty-five years (in which the bleeding occurred at monthly intervals, lasting three or four days, and ushered in by premonitory symptoms), whose grandfather and brother suffered from the same disease, which had shown itself also in his son, illustrating the association of atavism with direct descent in the same case. As hæmorrhoïds, however, like phthisis, are very liable to occur hereditarily in both sexes, and consequently to be transmitted as a double heritage, the influence of sex cannot in general be well defined; and this observation applies not only to hæmorrhoïdal, but sometimes to less localized affections of the circulatory system, as in the following case observed by Dr. Henry Stewart, in which the circulatory system of both parents was affected, resulting in the transmission of a double heritage:—A woman, aged thirty-two years, suffered from violent epistaxis, which continued almost incessantly for nearly a week, notwithstanding the efforts to arrest it made by several surgeons of eminence; almost every member of her family had died from some kind of hæmorrhagic disease; her son, aged nine years, died suddenly

in females, and that sexual preference may consequently aid in the hereditary limitation of the disease to the male sex; and with regard more particularly to angina pectoris, it appears from the statistical researches of Sir John Forbes (Cyclopædia of Practical Medicine, art. "Angina Pectoris," vol. i. p. 88), that of eighty-eight cases of the disease, eight only, or one in eleven, occurred in females.

* Traité Philosophique et Physiologique de l'Hérédité naturelle, &c., tom. ii. p. 691. 1850.

† Duncan's Medical Commentaries, vol. ix. p. 307. 1783-4.

‡ Op. cit., p. 262.

§ Observations on some of the most frequent and important Diseases of the Heart, by Allan Burns, p. 300. Edinburgh, 1809.

‖ On Lesions of the Vascular System, &c., by Baron Dupuytren, translated by F. Le Gros Clark, pp. 133-6. Sydenham Society, 1854.

from hæmoptysis; her mother, when she died at the age of seventy-three years, was covered with spots of purpura hæmorrhagica; her father died from rupture of the heart, and two brothers died from affections of the circulatory system: one of them from rupture of one of the circumflex arteries in the right arm, producing diffused aneurysm, and the other brother died from rupture of a varicose vein in the right leg.

There are many other well-marked and interesting illustrations of the influence of sex in hereditary disease not grouped with the preceding. For example, some peculiar affections of the stomach are often observed to be hereditarily limited to one sex. Linnæus* relates that Zimmerman suffered from violent gastric colic, which affected him from his earliest youth, and which suddenly ceased on his becoming a father; but his son inherited this evil, which afflicted him all his life. Morgagni† records the history of a woman who died of vomiting, as her mother had done, and whose children (without specifying sex) had continued to suffer from the same affection. In a case of this kind, observed by Prosper Lucas,‡ a father and son suffered from involuntary vomiting at meals; whilst the opposite condition of inability to vomit has also been observed to occur hereditarily, as in the case related by Dr. Osborne,§ of a gentleman who presented this peculiarity, which extended to several members of his family. With the foregoing may be classed those cases in which certain individuals chew the cud. This occurred in a case of hereditary rumination referred to by Prosper Lucas,‖ of a family residing at Bristol; the same author cites also the case of a man (native of Sweden) and his son; and another case of a tailor and his son, in which the same hereditary peculiarity occurred.

The hereditary peculiarities connected with diet are often subject to the same influence of sex, as in the case related by Prosper Lucas,¶ in which the grandmother, mother, and daughter eat without drinking, the aversion to fluids being so great as to be observed in fever. Special cases sometimes present themselves in which an hereditary aversion to particular articles of food is strongly marked, or in which certain articles of food produce abnormal and alarming effects. Venette** relates the case of two brothers who had an hereditary aversion to cheese; their mother had a decided taste for cheese, but the repugnance of the father was such, that at only the smell of it he was ready to faint. So also with regard to the hereditary inability to feed on certain kinds of food, in consequence of the alarming symptoms which result from all attempts to do so, as in those cases in which mutton or shell-fish act as though they were poisonous. Cases, how-

* Pujol: Œuvres de Médecine Pratique, tom. ii. p. 309. Paris, 1823.

† Epist. xxx. art. 7. ‡ Op. cit., tom ii. p. 695.

§ Edinburgh Medical and Surgical Journal, p. 255. 1832.

‖ Op. cit., tom. ii. p. 696. ¶ Op. cit., tom. i. p. 388. 1847.

** Ibid., p. 389. See also the case of Schook, author of a treatise 'De Aversione Casei,' who belonged to a family nearly all of whose members were unable to bear the smell of cheese. (Osphrésiologie, ou Traité des odeurs, du sens et des organes de l'Olfaction, par Hippol. Cloquet, seconde édition, p. 135. 1821.)

ever, of a more general character are commoner, in which the principles of vegetarianism, for example, find a natural response in certain members of a family born with an hereditary aversion to all animal food. A well-marked case of this kind presented itself lately in the French army, of a soldier who inherited this peculiarity from his father, and the aversion was so strong, that after eighteen months, during which no efforts availed to overcome it, he had to leave the regiment.*

In many cases of hereditary hernia the same influence of sex prevails. Valentin Polychrest† speaks of a family in which all the males had a hernia. The 'Journal d'Allemagne'‡ contains the case of an infant who had umbilical hernia, and whose grandfather and great-grandfather also had umbilical hernia; an interesting example of atavism associated with direct descent in the same case. Prosper Lucas§ observed a case in which the father, aged sixty years, had been suddenly affected with double inguinal hernia, and one of the sons, at the age of thirty years, had been attacked with the same infirmity on the right side, and menaced with its occurrence on the left. A case of congenital (scrotal) hernia of enormous size has been lately recorded,|| in which two brothers, one aged seven years and the other four years and a half, were both operated on successfully last year by Mr. Wood, at King's College Hospital. And Dr. Greenhalgh has informed me of the following case occurring in his practice, in which hernia was hereditary in the female line for four generations: A lady, aged twenty-eight years, consulted him, in 1845, respecting a femoral hernia of the right side; her mother had suffered for many years from femoral hernia also of the right side, which became strangulated in 1847; her maternal grandmother had been operated on, at the age of sixty-five years, by Mr. Lloyd (of St. Bartholomew's Hospital), and her maternal great-grandmother had been operated on, at the age of eighty years, by Sir T. Blizard, for hernias, in like manner affecting, it is said, the right side.

The frequency with which diseases affecting the urinary organs occur hereditarily has been very generally observed, and in many of them the influence of sex is well-marked. In the case of a gentleman, aged twenty-eight years, who had been subject to repeated attacks of inflammation of the bladder and strangury for some years, for which I had occasionally been consulted, his father, paternal uncle, and paternal grandfather had all died from disease of the urinary organs. The biographers of the late Professor Edward Forbes inform us that "nephritic ailments had been hereditary in his family, and it was found that with him also the same organs were unsound." So likewise stone in the bladder and urinary sediments in general are often hereditarily limited by sex. In a case lately under my observation, two brothers suffered from repeated attacks of gravel, which commenced imme-

* Gazette des Tribunaux, Mai 21, 1842.

† Prosper Lucas: op. cit., tom. i. p. 223.

‡ Ibid. § Ibid.

|| Medical Times and Gazette, Sept. 27th, 1862, p. 326.

diately after birth. F. Hoffmann* has related the case of a German Princess who was afflicted with renal calculus, and whose daughter, from the hour of her birth, suffered excruciating pain when passing water; the child died at the end of three days, and on examining the body, a calculus as large as a peach-kernel was found in the bladder. As this case might be considered an instance of communication of a maternal disease to the fœtus in utero, independent of what may be admitted to be hereditary influence, it will be convenient to notice a corresponding case observed by Mr. Squire,† in which the sex of the parties affected prevents the possibility of such an objection being raised: the case is that "of a still-born male child that he had the opportunity of examining, where the calices and the pelvis of the kidneys were filled with numerous uric acid calculi, some of the size of small peas; the father had been operated on for stone, and was then passing uric-acid calculi by the urethra, and he was a continual sufferer from marked symptoms of the uric acid diathesis." In the case of Montaigne, the essayist,‡ who inherited stone in the bladder from his father, he was the only one of a family of sons and daughters who did so. In a case of stone in the bladder, recorded by Morgagni,§ a man and his paternal uncle were similarly affected, indicating the presence of atavism in the descent; and this is still more forcibly illustrated in a case which occurred lately at the West London Hospital,‖ in which a boy, aged six years, suffered from stone in the bladder, which in his case appeared to have been both an hereditary and also a congenital disease; lithotrity had been performed on his maternal uncle, and a maternal cousin suffered likewise from stone.

Gout and rheumatism may also be mentioned as liable both to occur hereditarily and also to be subject in like manner to the influence of sex. With respect to the former disease, Dr. Garrod¶ states that in his hospital practice there was hereditary influence in fifty per cent. of the cases observed, and that if to these his private cases be added, a still higher percentage would be obtained; whilst Scudamore has recorded that of 522 patients affected with gout, it was ascertained to be hereditary in 332;** and even this large proportion of hereditary cases would be still further increased if we add to the list those cases in which two or more members of the same family were affected without any recognised occurrence of the disease in a preceding generation: for in one of the cases recorded by Scudamore, a butcher and his three brothers all suffered from gout "in a very severe degree;" their two sisters were free from it. It may be useful also to cite, as illustrations of selection in the direct inheritance of gout, the case of a family of nine children, six sons and three daughters, in which only one son was affected with gout, inherited direct from his

* Dissert. inaug. de morbis fœtus in utero materno, Halæ Magdel., 1702: quoted in Cyclop. Anat. and Phys., art. "Fœtus," vol. i. p. 336.

† Lancet, Jan. 10th, 1863, p. 60.

‡ Essais de Michel Montaigne, liv. ii. chap. 7.

§ De Sed. et Caus. Morbor. Epist. Anat. Med. iv. Art. 4.

‖ Lancet, April 4th, 1863, p. 386.

¶ The Nature and Treatment of Gout and Rheumatic Gout, p. 251. 1859.

** A Treatise on the Nature and Cure of Gout and Gravel, 4th edition, p. 55. 1823.

father; and another family of ten children, in which likewise only one son was affected, like his father, with gout. As an instance of the prolonged inheritance of gout, Scudamore has recorded the case of a gentleman affected with gout, whose father, grandfather, and great-grandfather all had gout: and Dr. Garrod* states the following case:—"A gentleman labouring under a severe form of gout, with the development of chalk-stones, and, although not more than fifty years old, he had suffered from the disease for a long period. On inquiry, I ascertained that for upwards of four centuries the eldest son of the family had invariably been afflicted with gout when he came into possession of the family estate." Dr. Bullar observed the case of a gentleman who had gout at the age of fourteen years, and whose father and grandfather also had gout. In a case which I have lately had under observation, a mechanic, aged twenty-four years, married, with one child, has been suffering from a first attack of gout in both great toes; he has one brother and three sisters who have never had the disease; his father, the only surviving member of a family of five sons, suffered from gout, which began in him also at the age of twenty-four years; and his paternal grandfather had gout, but at what age it commenced in him is not known. This tendency to limitation by age as well as by sex is not unfrequent in gout, and it is well illustrated in the following case, occurring in my own practice, in which four brothers began to suffer from gout at the age of eighteen or nineteen years, and they all died in succession from it; the eldest being forty-two, the second thirty-two, the third thirty-four, and the fourth thirty years of age, at the time of death; a younger brother, now aged thirty-six, who has been living in Australia for the last seven years, has up to the present time enjoyed good health; their father died from gout at the age of fifty-two years, having suffered from it since the age of thirty-eight years.

Rheumatism, especially when it attacks young individuals, has commonly been regarded as hereditary, and is often also limited by sex. In the case of a carpenter, aged thirty-three years, lately suffering from rheumatic fever, I ascertained that his father and one brother suffered from rheumatism, and also a maternal uncle, showing the double inheritance of the disease. This was noticed also in a case which has come under my own observation, of a girl aged eleven years, suffering from a first attack of rheumatism, whose paternal aunt at the same age, and maternal aunt at the age of twenty-seven years, had both suffered from rheumatism. In one of my cases of acute rheumatism with disease of the heart, affecting a lad, aged thirteen years, the father and the paternal uncle had also suffered from rheumatism, from which it may be inferred that the disease had probably been derived from a previous generation. The same occurred in the case of a journeyman mechanic, aged twenty-two years, who lately consulted me for a first attack of acute rheumatism, and who has five sisters, aged respectively thirty, twenty-six, twenty-four, eighteen, and fourteen years, all free from the disease; his father and paternal uncle have both suffered from rheumatism, which began early in life. In

* Op. cit., p. 253.

another case occurring in my own practice, two brothers, who were the second and fifth children in a family consisting of three brothers and two sisters, suffered from rheumatism. In the Spanish Bourbons, it is well-known that rheumatic gout was hereditary for several generations, and Fodéré,* who was consulted by Charles IV. of Spain, during an attack of the disease, states that this monarch's brothers and ancestors "had all been attacked by the same malady." So likewise in cases of chronic rheumatic gout the same hereditary limitation occurs, as in a case I am acquainted with in which this affection came on gradually in a woman at the age of thirty years, and the joints of whose hands are now, at the age of forty-three years, much crippled and deformed; her mother, who died at the age of forty-six years, suffered greatly from chronic rheumatic gout. which had commenced thirteen years previously, and which had thoroughly crippled and deformed the joints of both hands; there were three brothers and four sisters, the eldest of whom was a brother aged forty-five years, all of whom have been free from any similar affection: and in another case, which is at present under my observation, in which a girl, aged eighteen years, has the hands and feet dreadfully crippled by the same affection, which began at the age of fourteen years, her mother, maternal aunt, and maternal grandmother have all suffered in the same way, whilst the males in the family have been exempt.

To the foregoing illustrations of the influence of sex in hereditary peculiarities and disease may be added the following cases of hereditary obesity, for which I am indebted to Dr. Russell, of Birmingham. The first is the case of a very stout and flabby man, with copious deposit of fat, and symptoms of fatty heart; he has four brothers and one sister; the sister is thin, whilst one of his brothers is as large as himself, and the three others are larger; his father, paternal uncle, and paternal grandfather were large and fat men; his mother was of medium size, and his maternal grandmother was tall and thin: the second case is that of a very stout man, aged twenty years, with a very large amount of subcutaneous fat, and symptoms of fatty heart; he has had ten brothers and sisters, of whom only two brothers and two sisters are living; the two brothers are even fatter and heavier than he is, whilst the two sisters are of only medium size; his father was, as a young man, always very fat, and other male relations in the family are also large-made and fat.

In like manner, both the fatality and immunity from certain diseases are not unfrequently the result of the hereditary influence of sex; and so likewise may be the occasional failure of vaccination as a protection against small-pox, of which the following case, observed by myself, affords a good illustration, in which a father and two sons by different marriages had small-pox after vaccination, one of the sons having died from the disease: and as regards the other sex, Dr. Gillette† relates the following case: A little girl, named Lefèvre, had small-pox at the age of six months, and again severely at the age of three years; her mother, vaccinated with success at the age of six

* Op. cit., p. 375.

† Sur les Anomalies de la Vaccine: Journal de Médecine, Novembre, 1843.

months, had a very intense attack of small-pox at the age of twenty-seven years.

In those cases also which illustrate the correlations of hereditary disease, the influence of sex and of age, either conjoined or not, has been often observed. Prosper Lucas* refers to cases in which the hæmorrhagic diathesis was correlated with rheumatism and gout, the one disease being substituted at a certain age for the other in such members of the family as survived. Gout in like manner may be hereditarily correlated with hæmorrhoïds, as in the case reported by Stahl,† of a man attacked with gout in his youth, one of whose sons, at the age of twenty years, had gout and hæmorrhoïds, whilst another son had only hæmorrhoïds. Similar correlations are sometimes observed in congenital defects, as in the case related by Mr. Cooper Foster,‡ of a man who had hare-lip, and was the father of a large family, four of whom were the subjects of congenital deformity; the first and second children had hare-lip and imperforate rectum, the third child had imperforate rectum and defective palate, but no hare-lip; whilst the ninth child had only imperforate rectum. From my own observations on this point, I have reason to believe that the principle of correlation may often be traced in other hereditary diseases and defects, such, for example, as in cases of phthisis correlated with insanity, and that limitation by sex and sometimes by age may be associated with it; as in the case of a family I am acquainted with, which consisted of nine brothers and five sisters, in which two brothers died of phthisis at the age of twenty-five years, and a third brother, who was threatened at the same age with the same disease, but escaped by removal to another climate, subsequently became, like some preceding members of his family, insane; § and from what has been already observed on this subject, it may be inferred that this occasionally happens in other cases in which the medical history of families in this strait between the Scylla and Charybdis of disease has been either imperfectly sought after or withheld.

The hereditary influence of sex, not unfrequently combined with that of age, is often well marked in cases of cancer,‖ affecting structures common to both sexes, in rickets, and in other diseases; but as it is necessary to shorten this division of our subject, it will only be possible to notice the special character of those hereditary cases which

* Op. cit., tom. ii. pp. 798–824. † Ibid., p. 822.

‡ The Surgical Diseases of Children, p. 30. 1860.

§ This relation between the hereditary transmission of phthisis and insanity is doubted, if not altogether denied, by some physicians; as, for example, Dr. Cerise (Ann. Medico-Psychol., tom. iii. p. 627, 1857) and Dr. Morel (Traité des Maladies Mentales, pp. 117–18. 1860).

‖ Dr. Norris has recorded a case of fungoïd disease, interesting in connexion with the preceding remarks, in a man aged fifty-nine years, which originated in a mole situated between the umbilicus and pubes, and in which he remarked that "not only my patient and his children had many moles on various parts of their bodies, but also his own father and brothers had many of them: the youngest son has one of these marks exactly in the same place where the disease in the father first manifested itself. These facts, together with a case which has come under my notice, rather similar, would incline me to believe that this disease is hereditary." (Edinburgh Medical and Surgical Journal, vol. xvi. pp. 562–5. 1820.)

are either popularly supposed to be sexually limited or are necessarily so restricted from their connexion with organs peculiar to one sex. It has, for example, been supposed that the hereditary production of twins was due to some peculiarity in the female rather than in the male sex, and in one of my cases already cited,* it will be observed that the females only in the family became the parents of twin children, and that however numerous were the offspring of the males, they were all born singly; and it is well known that this prolific production is common to the females of certain families. Osiander† relates the case of a woman who in eleven accouchements had given birth to thirty-two children, was herself born with three other twins, and her mother had had thirty-eight children; another woman, delivered of five children at a birth, had a sister who was delivered of three; and lately, at Rouen, twin sisters gave birth to twins on the same day.‡ Mr. J. Lewis Brittain related last year at the Edinburgh Obstetrical Society,§ the case of a woman who had twins eleven times, and whose mother had had twins twice; and the report states that "several of the members mentioned that they knew of some analogous cases." Dr. Mitchell, in a paper on "Plural Births in connexion with Idiocy,"|| cites the following cases: The mother of an idiot, twin-born, bore twins twice, the maternal grandmother once, one maternal aunt twice, another once, and a sister once; in a second case the mother was herself one of twins, and she bore twins once; and in a third case of twin-born idiot, the aunt had borne twins; whilst among the cases in which the idiot was not twin-born, in one the mother and the maternal grandmother each bore twins twice; in a second case the mother and the maternal grandmother each bore twins once, and a maternal aunt twice; in a third case the mother and three maternal aunts each bore twins once; in a fourth case the mother bore twins once, and a maternal aunt bore twins four times running; and in a fifth case the mother and two sisters of the idiot each bore twins once. It is also well known that the hereditary production of twins in sheep is encouraged by saving the ewe-lambs that are twins.¶ Notwithstanding these facts, there are some cases which show that twins occasionally owe their descent as such to the male line, of which the following case affords a good illustration; two brothers (twins) both had twins by their wives many times in succession; the wife of one of them having died, the second wife produced, like the first, twins;** and in the case recorded by Mr. Stocks,†† of Salford, twin brothers also produced twins; one of them having a family of ten children, eight daughters and two sons, all of whom were twin-born; and the other a family of eleven

* British and Foreign Medico-Chirurgical Review, July, 1861, p. 209.

† Handbuch der Entbindungskunst, Band i. pp. 316–17.

‡ British Medical Journal, Nov. 30th, 1861, p. 598.

§ Edinburgh Medical Journal, 1862, p. 468.

|| Medical Times and Gazette, Nov. 15th, 1862, p. 513.

¶ Notes on Fields and Cattle, by the Rev. W. Holt Beever, M.A., p. 144. 1862.

** Nouveau Dict. d'Histoire Naturelle, tom. xii. p. 566, art. "Génération." Virey, 1817.

†† Lancet, July 20th, 1861, p. 78.

children, of whom eight were twin-born: it is, moreover, to be noticed that in this last case, whilst five of the female twins in the succeeding generation produced twins at their first birth, the three children of the only one of the male twins of whom any account is given were all born singly—leading us to infer that the hereditary predisposition to twins was probably derived from a female ancestor, and that each of the twin brothers referred to, in addition to being the medium of transmission, also shared in the inheritance. In connexion also with the influence of sex in the production of twins, it is necessary to notice the popular error respecting the alleged barrenness of females who have themselves been born as twins with male children, for it is still customary among nurses and midwives in some places to talk somewhat disrespectfully of such females as disqualified for the marriage state in consequence of their supposed inability to have children. This error, which probably arose from the well-established fact of the barrenness of the free-martin (the imperfect cow-calf twin with a bull-calf), was refuted by Mr. Cribb, in a paper published in 1823,* which contains six cases of such females becoming mothers. Dr. Sieveking has informed me of a case in which a woman, twin with a male, subsequently gave birth to twins; and any remaining doubt on the subject is removed by the fact that such females have on some occasions become even more than usually prolific, as in the case which occurred near Maidenhead, of quadruplets, consisting of three boys and one girl, who were all reared, and the only female in this quartet subsequently became the mother of triplets, consisting of two boys and one girl.†

There are, however, many cases of hereditary disease or peculiarity in which sexual limitation necessarily occurs, owing to the organ or function affected being limited to one or the other sex. Such, for example, as the total absence of the uterus in three out of five daughters in the same family,‡ an instance of the collateral inheritance of a defect which necessarily could not have been directly transmitted. A fatal tendency to puerperal hæmorrhage is sometimes hereditary in the women of certain families, as in the following well-marked case observed by Dr. Hart Vinen: A woman, subject to hæmorrhage of a very dangerous character after the birth of each child, and which was ultimately fatal, left one son and three daughters. The first married daughter died from hæmorrhage after her first confinement; the second married daughter had two children, whose birth was in each case followed by almost fatal hæmorrhage; whilst the third daughter is at present unmarried. Cancer is well known to be often restricted to the female sex when it affects the uterus or the breast.§ I am

* An Inquiry into certain opinions which exist relative to the Procreative Powers of Women who are Twins, the Socius in Utero, or Co-twin, being a Male: London Medical Repository, vol. xx. pp. 213–216. 1823.

† London Medical Repository, vol. xxviii. (new series, vol. v.) p. 350. 1827.

‡ British Medical Journal, Oct. 5th, 1861, p. 359.

§ Hereditary cancer of the breast is usually, but not necessarily, restricted to the female sex; for in the following case, observed by Mr. Zachariah Laurence, a man who was attacked with epithelial cancer of the left eyelid, and whose brother had epithelial cancer of the lower lip, lost his father from cancer of the breast, whilst his mother, who survived to the age of eighty-six years, died of natural decay.

acquainted with several cases of this kind, such as the case of a mother and daughter affected with cancer of the breast; and another case in which two sisters are so affected. Mr. Zachariah Laurence has informed me that in a case of cancer of the female breast, at present under his care, a cousin (her father's sister's daughter) died of the same disease affecting the same part. In another of Mr. Laurence's cases, two sisters suffered from scirrhus of the breast, whilst their father and mother lived to a very advanced age, and were both free from the disease. In another case, a woman, aged forty-six years, with scirrhus of both breasts, had lost her mother from scirrhus of one breast. And in another case, a woman, aged forty-five years, with scirrhus of the left breast, had lost her mother from scirrhus also affecting the left breast. Portal* states that the Duchess de la Vallière and her daughter, the Duchess de Châtillon; Madame Deshoulières and her daughter, Antoinette Thérèse; the celebrated French actress, Madlle. Contat, and her sister Emilie, all died of cancer of the breast. He was physician also to a family in which three married sisters died of cancer—two of cancer of the breast, and the third of cancer of the womb; and he was acquainted with another family in which three sisters out of five died of cancer of the womb. In a case related by M. Levi,† a mother and two daughters died from mammary cancer, and a third daughter was menaced with the disease; the sons were quite well. Not unfrequently, however, the disease, as in one of Portal's cases above referred to, may be almost necessarily limited to the same sex without being limited to the same organ; and a case of this kind was lately under my observation, in which a female suffered from cancer of the breast, whose mother died from cancer of the womb. So likewise in cases of supernumerary nipples which sometimes occur hereditarily in females, they are not always situated even on the same part of the body; for in a case of this kind published by Adrien de Jessieu,‡ the additional nipple was placed in the groin, and served ordinarily for suckling, whilst in the mother of this woman, who was born also with three nipples, they were all placed on the anterior region of the thorax.

With reference to cases necessarily limited in a corresponding manner to the male sex—as, for example, cases of supernumerary testicles, which sometimes occur hereditarily—Fernel§ mentions a family in which the males presented this peculiarity; and Sinabaldi|| refers to another family in which nearly all the male members were triorchid. Varicocele, or varicose condition of the veins of the spermatic cord, is sometimes also hereditary, as in the case observed by Breschet,¶ in which a father and several sons were all similarly affected with varicocele. In like manner hypospadias is occasionally hereditary, as in

* Op. cit., p. 90, note, and p. 91.

† Traité d'Hygiène Publique et Privée, troisième édition, tom. i. p. 147. 1857.

‡ Globe, tom. v. p. 128. 1827. § Oper. Pathol., lib. i. cap. 8.

|| Geneanthropæiæ, lib. ii. tract. 2, p. 204.

¶ Dupuytren on Lesions of the Vascular System, translated by F. Le Gros Clark, p. 100. Sydenham Society, 1854.

the case of a grandfather, father, and son, who were so affected;* and also in the case published in Rust's Magazine† of a degree of hypospadias in a father and son; whilst in Kauw Boerhaave's‡ case of four hypospadiac brothers; Lepechin's§ case of three brothers; the two instances cited by Baum‖ of the existence of hypospadias in brothers of the same family, the first mentioned by Walrecht, and the second by Gockel; Naegele's¶ case of twin brothers; similar instances of twin hypospadiacs observed by Katsky** and Saviard;†† and in Sir Everard Home's‡‡ case of a family of three children residing near Modbury in Devonshire, the second of whom was a well-formed female, and the eldest and youngest badly formed males: although it can only be inferred that in such cases the defect was hereditary, yet the general correctness of the inference is fully established by the fact that in this defect, as in the case of congenital hydrocele previously referred to,§§ as well as other affections of the male sex, atavism may occur through a female intervening to transmit the inheritance; for in a case observed by Meckel,‖‖ it appears that a woman, born of a family which presented many examples of hypospadias, gave birth to two boys affected with the same deformity.

It cannot at present be satisfactorily decided whether such interruptions in the transmission of peculiarities thus necessarily limited to the male sex are equally liable to occur in those peculiar to females by transmission through males. It is well known that the power of giving a copious supply of milk may be transmitted by the bull as well as by the cow;¶¶ but the result of my own observations on this subject leads me to infer that in all cases of interrupted descent in hereditary disease, the transmission by females is more common than by males, and that as regards the class of cases now under consideration, it may be asserted in general terms that whilst there is little difficulty on the part of females to transmit diseases and defects of exclusively male organs, there are very few, or rather scarcely any, corresponding cases in which the reverse occurs. And in conclusion it may be remarked, that with regard to all peculiarities of development in which certain characteristics of the two sexes are combined in the same individual, they are not only of frequent occurrence, but are sometimes very strikingly exhibited in many of the lower animals; and that with regard to their transmission it may be inferred, from the comparatively greater frequency of their occurrence among the

* Gazette Méd., troisième série, tom. i. p. 350. 1846.

† Magazin für die Gesammte Heilkunde, Band xviii. s. 113.

‡ Nov. Com. Acad. Sc. Petropol., tom. i. p. 61, tab. xi.

§ Ibid., tom. xvi. p. 525.

‖ Dr. James Y. Simpson: Cyclop. Anat. and Phys., art. "Hermaphrodite," p. 735.

¶ Meckel's Archiv, Band v. s. 136.

** Acta M. Berol., Dec. 1, tom. ix. p. 61.

†† Observ. Chirurg., p. 284.

‡‡ Lectures on Comparative Anatomy, vol. iii. p. 321. 1823.

§§ British and Foreign Medico-Chirurgical Review, July, 1861, p. 210.

‖‖ Handbuch der pathologischen Anatomie, Band i. p. 20.

¶¶ Burdach: Traité de Physiologie, tom. ii. p. 117; and Girou, op. cit., pp. 127–8.

invertebratæ, that they retrogressively become more and more often hereditary, until at last in some of the lowest tribes we find the apparent union of the two sexes in one individual is hereditarily constant.

Having now considered generally and in detail the influence of sex in hereditary disease, which in some forms, such as colour-blindness, and the hæmorrhagic diathesis, seems to be the prevailing law in their development, and which is also to be recognised to a greater or less extent in all forms of morbid inheritance, it remains for me to notice the principal conditions which may either really or apparently modify this restrictive influence by disturbing or superseding its action in those diseases which are truly hereditary, or which, through the development of diseases altogether independent of any hereditary influence, may yet so closely simulate the character of the preceding as to be liable on many occasions to be mistaken for them.

Among the causes which may disturb the influence of sex in the hereditary transmission of disease, are all endemic influences, which are not only in themselves capable of developing disease, but which may also modify the course of those diseases which have been the result of other causes. It is well known that defective sanitary arrangements in the dwellings of the poor may, by primarily affecting the parents, impair the physical development of their offspring, and that congenital deformities are, for example, sometimes the result of the continued deprivation of light, which thus indirectly induces an arrest of development, such as can be produced directly and at will in the case of tadpoles, which in the absence of light fail to become frogs.* It is not to be expected that when a congenital deformity has been so produced and repeated in two or more members of the same family any restriction to one sex could occur, although it may occasionally happen that peculiar conditions of the air, the water, or the soil may affect one sex in preference to the other; as in the case of bronchocele or goître, which is essentially of endemic origin, although very liable, when so developed, to be transmitted hereditarily, and yet in both cases is far more common in the female than in the male sex. Dr. Mitchell† informs us that, "in Scotland, eighty or ninety per cent. of all cases (of bronchocele) will be found to be women." In the case of cretinism, which like bronchocele is usually of local origin, no corresponding preference for one sex prevails; and it is important to observe that when, instead of being combined in the same individual, bronchocele and cretinism are disassociated, no hereditary transmission of the latter affection can occur, for owing to the arrest of sexual

* "The effect of darkness in producing deformities is well illustrated in the case of the French historical painter, Ducornet, who used to paint with his feet, having been born without arms, of poor parents living in one of the dark caverns under the fortifications of Lille. It appears that several of the deformed beggars in Paris had also been born at Lille, and that the effect of the absence of light in these underground places in producing malformed births was so notorious, that the magistrates of Lille issued strict orders to prohibit the poor from taking up their abode in them." (Medical Gazette, vol. x. p. 848. 1832.)

† On the Nithsdale Neck, or Goître, in Scotland: British and Foreign Medico-Chirurgical Review, April, 1862, p. 514.

development ungoîtrous cretins are sexually infants, and, like those tadpoles which have been reared in the dark, are unable to reproduce their kind.*

The peculiar influence of climate in modifying the character and course of disease, the effects of intemperance in drink in cold countries, such as in Scotland, Norway, Sweden, &c., as compared with those which follow in temperate or in warm countries; and many analogous facts show how necessary it is to consider well the causes which may modify the development and progress of disease generally, before we admit or reject the evidence adduced either for or against the influence of sex in hereditary disease.

Cases of congenital disease or defect will occasionally present themselves in two or more members of the same family which are undoubtedly of endemic origin, although the exact nature of the influence which produced them cannot be explained—such, for example, as the case† of a family which dwelt alternately at Paris and Bordeaux. The children engendered at Bordeaux were all born deaf-mutes; the children engendered at Paris were all born endowed as their parents with perfect integrity of hearing. And this endemic influence is still more clearly shown in the case recorded by Puybonnieux‡ of a married couple with eight children, of whom five were deaf-mutes; four of these last, and two children who could speak, were born at Rebrechien, at a house called *Le Jeu de Paume*, situated near the forest of Orleans, in a place elevated and apparently healthy; nevertheless, the people who had dwelt there before the married couple referred to had had three children, of whom two were deaf-mutes. Cases of this description occur as a rule independently of any hereditary influence, and they are not usually liable to reappear in succeeding generations, if the influence of locality has ceased.

The origin of the hereditary distinctions of race, which alike characterize both sexes, is also to be ascribed chiefly to endemic influence, and although it is customary to suppose that some centuries must elapse before the perpetuation of such influence can be fully established, yet a familiar illustration is offered in the present day by the American federal race, of a people who, without intermarriage with the Red Indian, have in a few generations so far succumbed to local influences as to have lost many characteristics of the race, or races, from which they are descended, and to have acquired in their stead those peculiar to the original proprietors of the soil. The same effect is already discernible, though in a less degree, in the European population of Australia; whilst even the supposed immutability of the Jews yields in like manner to the same influence, for Sir Charles Nicholson, at the annual meeting of the British Association for the Advancement of Science, held at Cambridge in October, 1862, stated that there is at present a race of Jews in India perfectly black, and

* Clinical Lectures on the Physiognomical Diagnosis of Disease, by Thomas Laycock, M.D., &c., Lecture VIII.: Medical Times and Gazette, May 3rd, 1862, pp. 449-50.

† Anecdotes de Médecine, tom. ii. p. 241.

‡ Mutisme et Surdité, p. 30. 1846.

that the physiognomy of the Jews in China had long since become the same as that of the Chinese, although the Jews marry only among themselves. This is parallel to what often occurs when plants are removed from one soil and climate to another, as in the well-known case of the Siberian naked barley (*Hordæum cœleste*), which when grown on the Rhine frequently degenerates into common barley.* In the same way also is to be explained the hereditary varieties of colour, &c., which occur under similar conditions in the lower animals. Darwin, in his 'Naturalist's Voyage,' (ch. ix. p. 192,) mentions the following instance of the localized propagation of colours among the cattle which range the pasturage of East Falkland Island: "Round Mount Osborne, about half of some of the herds were mouse-coloured, a tint not common anywhere else; near Mount Pleasant, dark-brown prevailed; whereas south of Choiseul Sound, white beasts with black heads and feet were common." So likewise in the case of insects, the variations of colour resulting from locality are often hereditarily constant under similar conditions of existence, however trifling may be the effect of such influence on any particular insect, as in the case of the Willow butterfly (*Vanessa antiopa*), a rare and beautiful species, which is characterized in this country by wings of a purplish colour, with a whitish or straw-coloured margin; but in French and other foreign specimens this margin is buff or orange-coloured, and the variation of colour dependent on locality is hereditarily so constant that entomologists can always tell whether a specimen of this insect is native or foreign, and it is prized accordingly. But the tendency of particular localities to reproduce certain specialities of colour, as well as of form and other variations, is still more strikingly exhibited sometimes in the case of shells. Sir Emerson Tennent† observed that "In the gardens which line the suburbs of Galle, in the direction of Matura, the stems of the cocoa-nut and jak trees are profusely covered with the shells of the beautiful striped *Helix hæmastoma*. Stopping frequently to observe them, I was led to observe that each separate garden seemed to possess a variety almost peculiar to itself. In one, the mouth of almost every individual shell was *red;* in another, separated from the first only by a wall, *black;* and in others, but less frequently, *pure white;* whilst the varieties of external colouring were equally local. In one enclosure they were nearly all *red*, and in an adjoining one *brown*."

Mussels, oysters, &c., change the colour of their shells to that of the bodies on which they are fastened. In like manner the ticks which Dr. Hooker,‡ in the Himalayan mountains, observed on the body of a parti-coloured lizard agreed so completely in colour with the three or four scales which each tick was of a size to cover, that whilst a tick lodged on the yellow scales of the animal's belly was yellow, and another on the brown scales of its head brown, a third tick which was clinging to the parti-coloured scales of the neck had

* Müller's Physiology, by Baly, vol. ii. p. 1665. 1842.

† Sketches of the Natural History of Ceylon, p. 372. 1861.

‡ Himalayan Journals, vol. i. p. 37.

its body parti-coloured, the hues corresponding with the individual scales which were covered. Even where such acquired peculiarities appear to be hereditarily constant, it has been observed that change of locality will often very quickly restore what may be termed the distinctive colour of the species. In the case of the lately announced discovery by Mr. Lewes* of a new species of fresh-water polyp (*Hydra rubra*) in the ponds of Wimbledon Common, it has since been found to be merely a local variety of the *Hydra fusca*; and the red colour, although hereditarily constant in this locality, gradually disappears when the polyp is supplied with water and weed from a different pond, and it then becomes undistinguishable from the *Hydra fusca*. So also in the case of the orange-disked and orange-tentacled anemones, named by Mr. Gosse *Venusta* and *Aurora*, the distinction is not specific, but due simply to endemic influence.† There may also often be noticed an adaptation of structure as well as of colour. In the case of many mussels, it is well known that they alter the form of their shell to that of the body on which they fasten themselves, and this power of accommodating themselves to their position is very strongly developed in many of the lower animals, and may account for the fact observed by Sir J. E. Tennent‡ in Ceylon, that each of the flat ticks which adhered to the fleshy neck of a tortoise (*Testudo stellata, Schweig.*) in such a position as to be exposed to constant danger of being crushed against the plastron during the protrusion and retraction of the head, was covered with a horny case almost as resistent as the carapace of the tortoise itself. It may, moreover, be remarked that as change of soil, climate, and other local conditions, have been experimentally proved to produce hereditary variations in animals as well as plants, so also may parasites vary with their position on the bodies of animals which they infest; and as removal from one part of the body to another is often in their case equivalent to leaving a native for a foreign land, the consequent alteration of colour and structure so developed would be uninfluenced by sex in its hereditary transmission.

It is not, however, to be inferred from the preceding remarks that variability in the colour or structure of animals can, unless in exceptional cases, be always permanently transmitted, for in the case more particularly of domestic animals, its origin seems to be partly connected with excess of food; and this, in accordance with the principle already pointed out, that all excess tends to reverse the natural order of things, may in extreme cases lead to sterility; and it is well-known that in the human subject excess of food is an occasional cause of abortion. It is in consequence also of this, that plants grown on a poor soil show a stronger tendency to produce flowers than leaves; whilst, on the other hand, varieties of colour in flowers, produced artificially through the influence of soil, are perpetuated by slips but not by seeds. It is not necessary to explain more fully the nature of these

* Studies in Animal Life, p. 73. 1862.
† Lewes: Seaside Studies, second edition, p. 150.
‡ Op. cit., vol. i. p. 189.

hereditary variations, and perhaps all attempts to do so would be useless, from the fact that it is only in certain cases that such variations occur; for many of the lower animals are capable of resisting the effect of all endemic influence, and consequently they retain their form and colour unaltered and unimpaired; such, for example, as the common house-fly, the hive-bee, and many others, which although almost everywhere distributed, remain permanently unchanged. But although the information to be derived from such sources may, at first sight, appear to be very limited, yet all such illustrations are useful in showing how uncertain may be the effect of the same causes in producing similar results, and that consequently, when any hereditary defect has been traced or referred to local or endemic influence, the subsequent failure of the same cause to produce a corresponding result does not show that our previous reasoning has been founded on error, for in further illustration of the same subject, it may be stated that in the case more particularly of insects, many variations of colour which naturally characterize the two sexes are hereditarily constant and hereditarily distinct in each. This influence in development is, however, of considerable importance, as will be subsequently shown, in connexion with the influence of sex on disease; for if it can be established that hereditary distinctions of race among the higher and of species among the lower animals can thus occur as the result of endemic influence, so may disease in like manner be developed and transmitted as the result also of endemic influence, and independent of the restrictive influence of sex. For except in special cases, such as bronchocele, in which the structure affected seems to be connected in some peculiar way with the female sex, it may be assumed that where the parents have been born and bred in the same place, the effect of any endemic influence would be the same, or nearly so, upon both, and that the offspring, as regards sex, would share equally in whatever disease might be so developed, as in the case referred to by Fodéré,* of a village of club-feet; whilst on the other side, where any acclimatized individual subsequently moves to another locality, marries and has children, the effect of any previous endemic influence would be liable to be transmitted chiefly to the offspring of the same sex.

Another of the causes which are capable of disturbing the influence of sex in hereditary disease is consanguinity in the parents, which to some, though perhaps very limited extent, leads to abnormal development in the offspring. During the last few years this subject has attracted considerable attention, and there seems to be a general willingness among well-informed persons in the present day to conclude that consanguineous marriages are occasionally followed by evil results, but it has not yet been proved that consanguinity alone is able to produce the effects which are usually ascribed to it, for there are other disturbing influences with which it is generally associated, and to which some at least of the bad consequences can often be referred. Hence the consideration of consanguinity has been preceded in this paper by that

* Op. cit., tom. v. p. 262.

of endemic influences, which include a great variety of disturbing causes, and it is probable that a large proportion of the cases of disease or defect commonly supposed to depend on consanguinity in the parents is chiefly due to one or more of these local influences, and that consanguinity itself occupies merely a subordinate position, in aiding to produce rather than by itself causing abnormal development.

In a very able paper "On Marriages of Consanguinity and Deaf Dumbness,"* which is generally supposed to be one of the most constant defects resulting from such marriages, M. Boudin informs us, "that deaf-mutes are the issue of consanguineous marriages in the proportion of 28 per cent. at the Paris Imperial Institution, 25 per cent. at Lyons, and 30 per cent. at Bordeaux;" and that as regards the Jews in Berlin, 27 in 10,000 are deaf-mutes, whilst the proportion is only 6 in 10,000 among the Christian population in that city; and apparently therefore with great justice, he concludes that "the hypothesis of the pretended harmlessness of consanguineous marriages is contradicted by the most evident and well-verified facts." Now, at first sight, it might appear that these figures, which have been confirmed by other observers, furnish an overwhelming proof in favour of consanguinity as a cause of deaf-mutism, but on closer inquiry it will be found that, in consequence of the fallacy to which all such numerical researches are liable, they possess only a limited value, and that the question, as usual, is not one which can be settled by a statistical *coup-de-main.* For, in the first place, with regard to the supposed frequency of consanguineous marriages among the Jews, M. Isidor, the Grand Rabbi of Paris, states that such marriages are far less frequent than is generally believed; and moreover, if the inference drawn from the great prevalence of deaf-mutism among the Jews of Berlin were correct, the statistics would be found to coincide with those of deaf-mutism among the Jews elsewhere, but such is not the case, for although the number of Jews in Paris is estimated at 25,000, only four of them are deaf-mutes.† Again, with respect to the greater frequency of deaf-mutism in other races among the offspring of those who are allied, compared with those who are aliens, by blood, although the facts adduced by M. Boudin and other writers are undoubtedly correct, yet the inference that has been drawn from them is in like manner probably erroneous, for all, or nearly all, the illustrations of deaf-mutism in these cases of consanguinity have occurred in circumscribed localities, where deaf-mutism, independent of consanguinity, is more common than elsewhere. Mr. W. R. Scott, of the Deaf and Dumb Institution at Exeter, has lately called attention to the fact that deaf-mutism occurs in much larger proportion in secluded and rural populations than in urban and manufacturing districts; in the union of Crediton, in Devonshire, 1 in 1143 of the population is a deaf-mute, and in the Scilly Islands, this is still more remarkably shown by the fact that there are no less than six deaf-mutes in a population of 2677, or 1 in 446.‡ But perhaps the strongest argument against the un-

* Recueil de Mém. de Méd. Militaire, Mars, 1862.

† Medical Times and Gazette, August 9th, 1862, p. 150.

‡ Ibid., Aug. 23rd, 1862, p. 211.

qualified admission in these cases of consanguinity as the *fons et origo mali*, is the fact that deaf-mutism cannot as a rule be directly transmitted to the offspring, even in those cases in which both the parents are deaf-mutes; for it is chiefly by means of breeding-in that peculiarities of structure among the lower animals are perpetuated, and their hereditary transmission is effected with so much certainty and facility, that it would be difficult in the present day to say what amount of abnormal development may not by this system be established as a permanent variety. It is therefore evident that consanguinity alone cannot be accepted as the cause of deaf-mutism, nor consequently as the sole cause of any other diseases or defects which have from time to time been ascribed to it.

The evils which result from consanguineous marriages are of a deeper importance than the occasional production of deaf-mutism or any other local or structural defect, for they are exemplified in the progressive degeneration and ultimate failure of race, which among the lower animals are now well known to result from too closely pursuing the system of "breeding-in;" and although the sexes, under these circumstances, remain distinct as regards individual existence, yet, as regards reproduction, the continued reunion of the immediate descendants of those who have through successive generations become as it were one flesh, must physiologically tend to develop a condition very similar to that in which the two sexes are combined in the same individual. It has been observed amongst the Arabs, who pride themselves on the purity of their breed of horses, that they are not only aware of the necessity of occasionally crossing their breed, but that they readily avail themselves of opportunities for doing so; and in like manner amongst the breeders of sheep and cattle in this and other countries, the importance of an occasional cross with rams and bulls obtained often at a great price, and aliens in blood to their own flocks and herds, is generally acknowledged, and when not attended to, the race becomes less fertile, and deteriorates. This may be said to illustrate practically what might theoretically be expected to occur, for as the life of the individual depends on a proper renewal of the blood, so does that of the race in like manner seem to need from time to time to be renewed also; and hence probably it is that among the lower animals the system of breeding-in cannot, as a rule, naturally occur, for individuals of the same race become scattered soon after their birth, and the home-ties of their early life are broken some time before they could form a consanguineous alliance. It may moreover be inferred from the late researches of Mr. Darwin* on the fertilization of orchids, that self-fertilization in plants, like consanguineous alliance in animals, is generally opposed to nature, for as a rule, each orchidaceous flower is fertilized by the pollen of another flower, conveyed to it by insects, which in some instances is rendered conspicuously necessary by the separation of the sexes, and also by the fact that when their cultivation is carried on in countries foreign to their own,

* On the various Contrivances by which British and Foreign Orchids are fertilized by Insects, and on the good Effects of Intercrossing. 1862.

mechanical aid is requisite for their fertilization; as in the case of the vanilla, which "in Tahiti, Bourbon, and the East Indies, does not fruit without artificial aid. . . . . This fact shows (says Mr. Darwin) that some insect in its own American home is specially adapted for its fertilization, and that the insects of the above-named tropical regions, where the vanilla flourishes, either do not visit the flowers, though they secrete an abundance of nectar, or do not visit them in the proper method." As all flowers have their appropriate insects, with structural peculiarities fitting them for this office, it is probable, as Mr. Darwin suggests, that not in orchids only, but throughout the vegetable kingdom, the insects which frequent flowers perform the duties of "marriage-priests," by effecting the requisite union of individual flowers, which either by continued self-fertilization, or by complete separation of the sexes, would otherwise have been infertile. All recent inquiry tends, indeed, to prove that this principle in development almost everywhere prevails, and that even amongst the lowly-organized animals and plants which have been hitherto regarded as asexual, male and female elements in some very primitive form exist, and that self-reproduction, which in their case is the analogue of breeding-in among the higher races, is continued only as it were for a season, and that even the humblest organisms, which seem to reproduce their kind by gemmation and segmentation, necessarily revert at periodic intervals to what appears in their case to correspond with sexual admixture, through the agency of two separate organisms, for their perpetuation; for the researches of Dr. Carpenter have led him to state that "the life-history of no organism can be considered as complete unless it includes an act of 'conjugation,' or some other form of the true generative process;"* and the late observations of Dr. Balbiani on the reproduction of infusoria, prove that in those cases in which the two sexes are combined in the same individual, and in which by far the most frequent mode of propagation is by segmentation, a true generative operation periodically occurs, when "two individuals are always needed for fecundation, and that they act at the same time and reciprocally the part of male and female."†

It is therefore probable that although consanguinity occupies an important position in reference to the influence of sex on disease, by lowering the development of the offspring and rendering it more liable to be affected by other morbid influence, yet it is chiefly when associated with endemic causes of disease that evil consequences ensue, and it is doubtful whether consanguineous marriage alone can, except by frequent repetition, produce such an amount of degradation, general or local, as will show itself in organic disease, although there is no doubt that it will sometimes tend to perpetuate disease previously developed by other means. Hence also it may be inferred that in earlier periods of the world's history, consanguineous marriage was less objectionable than in the present day, for when the endemic

* The Microscope and its Revelations, third edition, p. 271. 1862.

† Recherches sur les Phénomènes sexuels des Infusoires : Journal de la Physiologie de l'Homme et des Animaux, p. 515. 1861.

existence of our crowded populations, which breathe the same air, dwell on the same soil, drink the same water, and feed on the same food, which itself is produced under like conditions, is contrasted with the nomadic habits of scattered and half-savage tribes, there is reason for concluding that nations, like individuals, need renewal of their blood, and although other causes probably co-operate to bring about the evil result, yet when the conditions requisite for that renewal fail, their existence as a nation begins to cease.

The extent to which the influence of sex may be affected by consanguinity appears, however, to be less than by the opposite condition of hybridism, for it has been customary to suppose that no true hybrids are fertile, and therefore whatever sexual peculiarities might prevail in them would be liable to be still more quickly lost by extinction of race. Dr. Lindley,* it is true, informs us that in the vegetable kingdom "the power of creating mule plants fertile for two or three generations incontestibly exists;" and the late researches of M. Broca† have established that the mule offspring resulting from the crossing of different species of animals—such as from the union of the dog and wolf, of goats and sheep, of camels and dromedaries, of hares and rabbits, &c.—are "perfectly and indefinitely fruitful among themselves;" whilst M. Rouy, of Angoulême, has succeeded in obtaining hybrids from the hare (*Lepus timidus*) and the rabbit (*L. cuniculus*) which have not only been fertile for thirteen generations, but whose fertility seems likely to continue; and these hybrids, under the name of *leporides*, are now supplied in thousands to the French markets.‡ But notwithstanding these facts, it has been observed, especially in the animal kingdom, that "the sexual union of hybrids is generally unfruitful, and when fruitful, as in the case of the union of a hybrid with an individual of one of the species which had co-operated to form the hybrid, the product relapses into the type of one of the original species." Even in those cases of spurious hybridism which result from the crossing of varieties of the same species, a similar tendency to relapse occurs, when peculiarities which have been suppressed for several generations will return and show us that, by means of atavism, a lost inheritance in development may at any time be restored.§ This transmission of peculiarities in interrupted descent is well known to

* An Introduction to Botany, fourth edition, vol. i. p. 243. 1848.

† Journal de la Physiologie de l'Homme et des Animaux, tom. iii. p. 433.

‡ Lewes: Studies in Animal Life, p. 162.

§ It has been noticed by several observers, that, within certain limits, the influence of sex is well-marked in vegetable hybrids: for example, Dr. Herbert (Journ. Hort. Soc., vol. ii.) established it as a law with respect to Amaryllaceous hybrids, "that they resembled their mother in foliage and stem, or the organs of vegetation; and their father in flower, or the organs of reproduction;" in like manner Fries-Morel (Ann. de la Soc. d'Hortic. de Paris, p. 112, 1828) states that in carnations the hybrids resemble the mother in form and the father in colour. See also, Sagaret, "Considérations sur la production des Hybrides," &c. (Annales des Sciences Naturelles, tom. viii., 1826); and De Candolle, Physiologie Végétale, tome ii. 1832. Messrs. Giertner, Knight, and Wiegman (De Candolle, op. cit., tom. ii. p. 714) have further remarked that many vegetable hybrids have a tendency to return, after a time, to the maternal, but never, as a rule, to the paternal type.

the breeders of domestic animals, who term it "*breeding back*," and like the German term "*Ruckschlag*," and our own "*atavism*," refers to a condition which is now familiarly known to be of frequent occurrence in disease, and as such will require special notice.

Closely allied with this subject is the curious and important influence which a previous marriage has on the succeeding offspring. It is now well ascertained that when the female of any animal has had fertile intercourse with a male, the effect on her system is to a certain extent permanent, in consequence of which any succeeding offspring by other males may show evidence more or less conclusive of having partaken of the nature of the first male, for it is the first union which chiefly impresses itself on the succeeding offspring. In the well-known case of Lord Morton's mare covered by a quagga, and subsequently by horses of pure breed, the succeeding offspring continued to show traces of the quagga; and the same occurred in the case of Sir Gore Ouseley's mare covered by a zebra, and subsequently by a thoroughbred horse; so also when a mare has had a *mule* by an ass, and afterwards a *foal* by a horse, the foal exhibits traces of the ass. Mr. W. Bullock Webster* informs us that an Arab mare that has once had a colt by a half-bred horse can never again breed a pure Arab; the colts, even after a number of years, always taking, to some extent, after the horse she first bred by. And similar facts have been noticed with regard to other animals, as of the bitch with different kinds of dogs, the sow with different kinds of boars, the cow with different kinds of bulls, &c. With respect to the human race, in which corresponding results have been observed, the effect of such intercourse has to be considered under two heads—first, as between individuals of the same race; and second, as between individuals of different races. Respecting the effect produced in the first class of cases, it seems to be well-established that the peculiarities of a male that has once had fruitful intercourse with a female are more or less liable to be transmitted to the succeeding male offspring; and as regards the effect produced in the second class of cases, it is apparently still greater, for if we can admit the evidence of Strzelecki† on this subject, it appears that whenever a fruitful intercourse takes place between an aboriginal female, as, for example, of Australia and a European male, "the native female is found to lose the power of conception on a renewal of intercourse with the male of her own race, retaining only that of procreating with the white man." It is, moreover, probable that the unfruitfulness which attends the promiscuous intercourse of those who live by the unlawful pleasures of others is intimately associated with the above physiological facts.

In considering, therefore, the effects of hybridism and this allied condition, it is necessary to determine not only the purity of the race on each side, but also the purity of the individual, before we can accept any conclusions drawn from this kind of evidence; for where the blood is thus permanently affected, and the system is liable to undergo

* The Evils of Consanguinity, Algiers, Aug. 29th, 1862 (Times, Sept. 3rd, 1862.)

† Physical Description of New South Wales and Van Diemen's Land, pp. 345–7.

a change, never as it were to be washed out, it is not possible to tell how far hereditary defects may be thus indirectly transmitted, and the offspring, for example, of a healthy couple be influenced by the marriage of a widow whose previous husband had been subject to some disease or peculiarity which the mother has conveyed to her offspring by a second or a subsequent marriage. It is well known that the children resulting from such marriages sometimes bear more resemblance to the first husband than to the second; and as the same results which follow in the case of natural inheritance may, as a rule, be expected to occur quite as readily, if not more so, in the case of morbid inheritance, it is probable that this may occasionally act as a disturbing influence in the inheritance of diseases common to both sexes; so that whilst the daughters inherit direct from their mother, the sons by a second marriage may, to some extent, inherit from their mother's first husband, through the agency of what may be termed *indirect atavism.* It is moreover to be noticed that the more closely the condition of true hybridism is approached, the more difficult and uncertain will be the transmission of hereditary peculiarities or defects in either sex, so that its influence will ultimately be found to exceed greatly that of consanguinity, for at the same time that it is liable to give rise to anomalies in development, it has the effect also of more completely arresting their inheritance.

Great inequality in the age and condition of the parents has been considered by some writers on hereditary disease to influence very strongly the offspring, but except so far as it may affect the general question of sex independent of disease, and there is some evidence to show that it does so, it could not be expected to limit the inheritance; but, on the contrary, it might, by superseding the controlling influence of the other parent, in some cases check any tendency in the disease to be confined to what would otherwise be its peculiar sex.

Finally, among the disturbing causes in development may be classed the influence of maternal emotion, which seems to be capable of affecting the offspring by causing a partial arrest of development, and when this occurs, the resulting abnormal condition may be repeated in other members of the same family either by a recurrence during pregnancy of the same emotional state, or in consequence of the impression on the maternal system, as may occur also in the case of a previous marriage, not being effaced. Admitting, therefore, that the mental condition of the mother can be thus phrenographed in the physical development of her offspring, it is evident that where two or more members of the same family have been so affected during intra-uterine life, the result must be independent of the influence of sexual limitation, and it is not impossible that in some cases its occurrence might disturb the normal course of a sexually limited and hereditary disease by influencing the development of a child belonging to the contrary sex.

From the preceding remarks it will obviously be impossible to form a clear estimate of the influence of sex, or to attempt any explanation of many other phenomena in hereditary disease, without having previously considered the nature of atavism, to which frequent allusion has been made; from its forming the central point round which all observations on hereditary disease seem, as it were, to converge, and which, as regards its relative importance, may be termed the key to morbid inheritance. In consequence, moreover, of the intimate union which exists between sexual limitation and atavism, any notice of the one condition to the exclusion of the other would be necessarily incomplete. It is therefore proposed, before concluding this paper on the influence of sex in hereditary disease, to offer a few concluding remarks on the nature of atavism; but previously to doing so, it may be convenient to notice, that when diseases have affected several members of the same family, but not, so far as can be ascertained, their ancestors, it has been the custom to assume that they are the result of some disturbing influence in development, such as has been already considered; and, accordingly they have been placed by many writers on the subject in a separate class, under the title of family diseases, as distinguished from those commonly called hereditary. There does not appear to be any need for continuing this distinction, which is apt to lead to some confusion, by inducing us to separate parallel cases of the same disease, when, from some omission in the family history, the required proof of the condition of the previous generations in each case cannot always be obtained. It is not, however, necessary to conclude that all these so-called family diseases result from the hereditary transmission of the disease itself, for external and endemic causes may have had something to do with their production;* and although, strictly speaking, even such cases are inherited or derived from parents who, having themselves suffered from the morbid influence, have transmitted its effects to their offspring in the form of a family disease, which members of the same family are consequently liable to share in common, even to the extent sometimes of living free from it for the same length of time, and of dying from it at the same age; still, after deducting these, there remains a large number of cases to which even this explanation cannot apply, and in which we are forced to conclude that the simultaneous occurrence of the disease in two or more members of the same family is either the result of some previous

* The distinction which some have endeavoured to establish between the transmission of hereditary and acquired diseases or defects has not only less foundation than it might at first sight be supposed to have, but it is moreover probable that if the causes on which morbid states of the system generally depend could be fully analyzed, all hereditary diseases would be referred to the class of acquired defects—acquired through the agency chiefly of external and disturbing causes, and differing therefore only in the influence through which they have been developed; consequently, all hereditable disease may be looked upon as primarily an acquired state of the system, and it is apparently in consequence of this that it usually admits of being transmitted for only a limited number of generations, and then ceases, by a recurrence on the part of the organ or tissue affected to its normal state: just as artificially produced varieties in animals and plants show a frequent tendency to leave off the acquired peculiarity of their race, and to change back to the original type, unless the disturbing influence be continued, in which case the acquired peculiarity becomes hereditarily constant, as, for example, the flat crown of teeth in dogs which are accustomed to feed on vegetable diet.

generation having been affected, or that it is an accidental coincidence. As it is never desirable, in the study of natural phenomena, to fall back on the latter conclusion, it is necessary to consider closely whatever evidence can be adduced in favour of the former view, which simply involves an extension of the usual limits of atavism, and which may therefore be appropriately considered in connexion with it. For although it is not possible to form an idea of the primary cause of any of the operations of nature, yet as regards the inheritance of disease, in common with all other events in nature, two very distinct ideas result from the study of its phenomena—namely, the idea of force as producing such phenomena, and the idea of time as determining their succession and duration.* It is to the consideration of the latter idea that we now approach, and it is hoped that the brief notice contained in the following remarks will not only suffice to prepare the way for further inquiry in this direction, but will also suggest a reasonable explanation of many of those difficulties which have hitherto obscured the inheritance of disease.

Atavism, or the principle of latent inheritance, is a normal phenomenon in certain forms of disease, as in colour-blindness and the hæmorrhagic diathesis, in which it is associated with sexual limitation, and pursues for the most part a very regular course. It is well known that in these two forms of disease the order observed, both in their development and transmission, is usually complete, so that whilst the disease in each case shows itself only in the males of the first, third, and fifth generations, its transmission is effected only by the females of the second and fourth generations; and as this occurred also in my case of ichthyosis and in other diseases already noticed, the question naturally presents itself whether in those cases in which two or more members of the same family are affected, without any evidence of the occurrence of the same disease in preceding generations, it may, notwithstanding, have occurred without being recorded. I have at present under observation two well-marked illustrations of the occurrence of disease usually hereditary in the common meaning of the term, but both of them presenting the form of a family disease. One of them is a case of colour-blindness affecting two brothers, who are the youngest children in a family consisting of five brothers and two sisters; no individual belonging to any previous generation in this family is known to be affected, although diligent inquiry on the subject has been made by one of the two brothers referred to, who is a member of the medical profession. The other is a case of the hæmorrhagic diathesis in three brothers, who are the fifth, sixth, and eighth children in a family consisting of six brothers and four sisters. The mother belonged to a family of four brothers and eight sisters, who were all unaffected, as were also the maternal grandfather, who lived to the age of ninety years, and the male and female relations generally on both sides of the family. In judging of such cases, it is necessary to call to mind that these diseases, being as a rule atavic in descent,

* Life on the Earth; its Origin and Succession, by John Phillips, LL.D., p. 3. 1860.

pass over the second and fourth generations; and also that in both of the two cases referred to, some of the brothers, as well as all of the sisters, have been passed over in each family affected. It might consequently be inferred that as the disease in each case could thus pass over part of a generation, so also it could pass over a whole generation liable to it, and that instead of following its usual course, and affecting the first, third, and fifth generations, it might affect only the first and fifth, and be altogether suppressed in the third generation. This opinion respecting the extension of the customary limits of atavism, inferential only as regards these two cases, is supported by some direct and conclusive evidence, for in addition to such cases as those of albinism already cited, affecting cousins so remotely related that the common ancestor belonged to the fifth generation, but in which no proof of the previous occurrence of the same affection could be obtained, there are some cases in which the evidence is happily complete, although it must be admitted that their number is small, for it is necessarily difficult to trace the course of hereditary disease when the interruptions extend over three or more generations, as no family records are usually kept of any of those diseases which our flesh is heir to; and in the absence of such records it must always remain an open question whether a disease has been inherited or not.

The following three cases bearing on this subject have been selected because the evidence is of a very trustworthy character, and they suggest that, as long interruptions have occurred in their hereditary development, many similar cases may have passed away unrecorded. The first illustration occurs in the curious case, previously referred to, in which the only record of family blindness, affecting four out of five children about the age of twelve years, was an ancient tombstone, the figures and inscription on which showed that a mother and her children, members of two former and remote generations of the same family, had also been blind.* The second illustration occurs in the case of our late sovereign, George III., in which the disease passed in the direct male line from the first to the eighth generation, without showing itself in the intermediate generations. This monarch's paternal ancestor, Duke William of Lüneburgh, surnamed the Pious, the eighth in direct male ascent was, like his descendant, blind and insane, was like him also in his tastes, and was like him in the number of his offspring.† The concluding illustration is derived from the practice of Dr.

* Sir Henry Holland, op. cit., p. 33.

† "Among the German princes who sate under Luther, at Wittenberg, was Duke Ernest of Celle, whose younger son, William of Lüneburg, was the progenitor of the illustrious Hanoverian house at present reigning in Great Britain. Duke William . . . . was a very religious lord, and called William the Pious by his small circle of subjects, over whom he ruled till fate deprived him both of sight and reason. Sometimes, in his latter days, the good duke had glimpses of mental light, when he would bid his musicians play the psalm tunes which he loved. One thinks of a descendant of his, two hundred years afterwards, blind, old, and lost of wits, singing Handel in Windsor Tower." (The Four Georges, by W. M. Thackeray, pp. 5–6. 1861.)

The number of offspring in each case was fifteen, but as two of the latter family were born prematurely, they are not recorded in history, and the statement in the text may therefore appear open to objection.

Newman, of Stamford, who has favoured me with the following history of the case, which is instructive, although the interruption in descent is of shorter duration than in either of the preceding cases, and the restrictive influence of sex, as is often noticeable in hereditary malformations of the fingers and toes, is imperfectly defined. On Feb. 1st, 1863, a man came under observation who had a supernumerary finger, composed of two well-proportioned phalanges and nail, attached to the base of the first phalanx of each little finger. This peculiarity was inherited from his paternal great-grandfather, who had the same malformation. Neither his paternal grandfather, his father, his four paternal uncles, or his two paternal aunts, had it; but one of his sons, being the second child in a family of six sons and four daughters, had a supernumerary finger on the right hand agreeing exactly with his father's both in situation and form, and an elevation on the left hand at the corresponding site. His sister had a similar deformity in both hands, and two daughters of one of his paternal aunts have corresponding malformations, one of them having a supernumerary finger on one hand only, whilst the other daughter has supernumerary fingers on both hands and supernumerary toes on both feet. In reviewing the preceding case, it may be observed that the defect was transmitted from a common male ancestor, through the male line, to a great-grandson and a great-great-grandson; whilst in another line it was transmitted through a grand-daughter, not herself malformed, to two great-grand-daughters who were so, two complete generations being passed over in each case. This indicates that when distantly related cousins, or other relations, are similarly affected, a failure in tracing back the defect to a common ancestor may negatively weaken the evidence in favour of its being a common inheritance, but cannot be recognised as an argument against the correctness of our inference that it is so; for, as a common ancestor is often observed to be merely a medium for transmission to descendants more closely related than in the preceding case, it may reasonably be assumed that a common ancestor may in like manner occasionally transmit without sharing the defect, in some of those cases in which the relationship is more remote.

If, notwithstanding the force of the foregoing evidence, it should be urged that these so called family diseases, limited as they often are by sex, must still as a rule be considered to appear in families independent of any hereditary influence, they might in like manner be expected to disappear from families without leaving any trace of their occurrence; but so far is this from being the rule, that we occasionally meet with cases in which, for example, several brothers have been similarly affected without one corresponding case of the disease being discoverable in any preceding member of the family, and yet in succeeding generations the disease has not disappeared. If, in such cases, the reappearance of the disease was limited even to a son of one of the individuals affected, it might be accounted for on the principle of the inheritance of acquired defects, for any abnormal condition of the body, whether resulting from accident or any unknown cause, may be occasionally transmitted to the immediate descendants; but such an explanation would not apply in those cases in which the disease reappeared in nephews instead of sons—as, for example, in the case of hæmorrhagic diathesis, recorded by Dr. Clay,* affecting three brothers in a family, without any previous generation, so far as could be ascertained, having been in like manner affected; but a sister who did not herself share in the disease had three sons affected in the same way as their three maternal uncles. In such a case it would be unreasonable to suppose that the disease in the three uncles could have been otherwise than hereditary, for its reappearance in their nephews, who being the sons of a sister, necessarily did not inherit direct from the mother, but by atavic descent from a previous generation, was an almost conclusive proof of its transmission from a common ancestor. When, therefore, cases are met with in which, like the above, three or any other number of brothers or sisters share in the same disease, which is not obviously the result of external causes, it may be assumed that the disease is hereditary; and if, in addition to this, any of the collateral relations are similarly affected, and the disease itself be moreover limited to one sex, and when, not being congenital, it is developed also at the same age in each, the presumptive proof in favour of inheritance is too strong to be explained away by any theory of coincidence. Consequently, if the facts themselves in the above case of hæmorrhagic diathesis cannot be gainsaid, the inference drawn from it cannot be disputed. Nor can it be urged that such illustrations as the one just referred to are in any degree exceptional, as many corresponding cases might be cited. For example, Mr. Wardrop has recorded a case in which five brothers afflicted with the hæmorrhagic diathesis had three sisters free from it; but each of the sisters had, besides other children, two sons similarly affected, whilst no female in the family shared in the disease. In this case it may justly be inferred that the disease from which the five brothers and their three pair of nephews suffered was derived from an ancestor more remote than usual; and the extent to which the inheritance may be referred back in these cases is sometimes greatly increased by the fact that owing to a suppression of the disease in the ancestor to whom, as already stated, it might be referred,† whilst either the disease itself, or a tendency to it, is exhibited in that ancestor's brother, or some more distant relation, the circumstantial evidence in favour of remote atavism is rendered still more

* Medical Times, vol. xiii. pp. 293–4. 1846.

† This reference to the probable suppression of hereditary disease in a member of some previous generation is connected with a question of great importance in atavism, but which the assigned limits of this paper will not allow of being fully discussed; for there is only space to state, that not only has it occasionally been noticed that the development of disease in the parent or ancestor from whom it has been transmitted has occurred subsequent to its development in the offspring, but it has, on the other side, been observed, that ancestral disease has failed to appear in the offspring either until an advanced period of life or until some other morbid change had led to its development. (Morel, op. cit., p. 116, note; and Prosper Lucas, op. cit., tom. ii. pp. 851–3.)

complete, as in the following case observed by Dr. Krimer, of a woman, M. K., "whose paternal uncle had been subject to epistaxis, and who had lost all her five brothers in early youth by hæmorrhagy, from trifling injuries, or without ascertained cause. She herself had five sons and two daughters; of the sons, four had died of hæmorrhage, but one, notwithstanding many dangerous attacks, was still alive when the history was written, and had two sons and a daughter, in whom the tendency had not appeared. A sister of M. K. had two sons and two daughters; one of the sons died in a few days after birth, of cramp, asphyxia, and blueness of the whole body; the other son was liable to bleedings and ecchymosis. None of the females of the family had any hæmorrhagic disposition."* And this point is perhaps even more forcibly illustrated in Mr. Murray's case of hereditary "Hæmorrhœa Petechialis,"† in which four brothers, belonging to a family of six brothers and four sisters, were similarly affected; no other relation was known to have suffered from the disease except a boy, who bore the mother's maiden name, but who was only distantly related to the family: for the inheritance which these four brothers shared in common was transmitted, as is usual in these cases, through their mother, from some far off ancestor common to them and to their maternal but distantly related cousin.

In reference to the connexion of atavism with disease, it is necessary to state, in opposition somewhat to M. Girou's opinion on this subject,‡ that although the principle of atavism is intimately associated with that of sexual limitation both in natural as well as in morbid development, yet it is far more common in the latter than in the former state. M. Girou endeavoured to show that children naturally resembled their grand-parents of the same sex, and that the transmission of this family likeness was effected through parents of the opposite sex. It is, however, quite evident that although such atavic resemblances may often be noticed, yet contrary facts can be so readily adduced as to disprove the general application of the above principle in the normal development of the human race; and this is perfectly consistent with the statement I have previously made, that the laws which regulate the development of hereditary disease have been adapted to a scale so low as to agree in principle with those which govern the development not of the higher but of the lower animal tribes. This accords also with Moreau's observations on hereditary insanity already referred to,§ which indicate that natural and morbid inheritance are, as regards the influence of sex, in some degree opposed to each other. In connexion also with this subject, it may be observed that in the offspring of two dissimilar parents there is never, as a rule, complete fusion of the two parents, but a distribution of the characters peculiar to each; and although this is less strongly remarked in the offspring of the human race than it is in that of the lower animals—as, for example, in the case of some hermaphrodite insects, in which family quarterings may

* Edinburgh Medical and Surgical Journal, vol. xxv. p. 455. 1826.
† Ibid., vol. xxvi. pp. 33–5. 1826. ‡ Op. cit.
§ British and Foreign Medico-Chirurgical Review, p. 522, 1863.

result from specific distinctions of sex being associated without fusion in the same specimen,* yet as regards the inheritance of disease, it will be found that the morbid characteristics of one or the other parent are either completely repeated or completely absent, but not fused together in the offspring. This is what is meant in inheritance by the doctrine of "election," which is based on the observation that certain attributes of organization peculiar to one parent are repeated in the offspring; and it offers a reasonable explanation of the fact that children often inherit the defects of one parent, whilst in many other respects they resemble the other; and the inheritance in these cases, both natural and morbid, may sometimes be conveyed to them by atavic descent.

Of late years the attention of naturalists has been specially directed to the principle of alternation of generation, or metagenesis, and the attention more particularly of the medical profession has been called to the same subject by Dr. Sharpey in his able address on physiology, at the late meeting (1862) of the British Medical Association in London. But even so far back as the year 1819, Chamisso† made the curious observation respecting the development of salpa (one of the tunicata), that the solitary salpa did not produce solitary but associated salpæ, and *vice versâ*, the solitary salpa being now recognised as asexual (or imperfectly female), and the associated salpæ as hermaphrodite; and subsequent observations have shown that corresponding alternations occur in the reproduction of the cestoïd entozoa, of zoophytes, the oviparous trematoda, and many other lowly organized animals.‡ This alternation of generation, which appears to be confined to the invertebratæ, and which, prevailing chiefly in the lowest conditions of animal life, becomes less frequent in the higher invertebratæ, is a form of reproduction in which the grandparents resemble the grandchildren in atavic succession; and it corresponds very closely in principle with what has been observed in the atavic development of colour-blindness, the hæmorrhagic diathesis, and other hereditary diseases. As a further illustration of the extent to which the principle of analogy may be applied in the investigation of all natural phenomena, it may be observed that, in the same way that more remote forms of atavism characterize some cases of disease in which imperfect limitation by sex occurs, so among the lower animals there are to be found corresponding illustrations of the same principle in

* As in the specimen of the scarce egger moth observed by Mr. Westwood (Entomologist's Text Book, p. 397, 1838), at Berlin, in which the front part of the body and front wings were half male and half female, and the hind part and hind wings half female and half male; the characters of the male and female insect being exhibited on opposite quarters of this specimen.

† De animalibus quibusdam e classe vermium Linnæanâ. Fasc. I. De Salpa. Berolini, 1819. 4to.

‡ On the Alternations of Generations; or, the Propagation and Development of Animals through alternate Generations, by Joh. Japetus Sin. Steenstrup, translated by George Busk. Ray Society, 1845.—On Tape and Cystic Worms, by Carl Theodor von Siebold, translated by T. H. Huxley, appended to Küchenmeister's Manual of Parasites. Sydenham Society, 1857.—Dr. Allen Thomson: Cyclopædia of Anatomy and Physiology, vol. v. Supplement, 1859.

their development. Such, for example, as occur in the fully developed descendants of the aphis, or plant-louse, in which sexual limitation characterizes the whole of several successive generations, which are composed only of females; but the power of hereditary transmission in the case of these as well as of other animals low down in the scale is, as often occurs in the hereditary transmission of disease, limited to a given number of generations, and when that limit is reached, the progress of development is to some extent reversed, and a recurrence to another type in development occurs, no longer characterized by limitation as regards their sexual character, for the restricted production of females is for a time interrupted by a generation composed partly of males*—the atavism in the one case being associated with natural, and in the other with morbid development. The tendency of all recent inquiry in this direction seems to indicate that few if any of the phenomena in the development of hereditary disease are so strictly limited to the human race, or so exceptional in their occurrence, as not to be more or less closely allied with the development of some of those humble beings which have for the most part, until lately, escaped our notice. For it appears to be a rule in reproduction, that the lower the position of organized beings the more constant is the occurrence of this principle of atavic or interrupted inheritance, the associated principle of sexual limitation being gradually merged in that of alternation of form. So that whilst in the higher invertebratæ there are alternations between the sexual and the virgin forms of reproduction, and in many of the lower invertebratæ between the hermaphrodite and the gemmiparous, in the corresponding alternations which occur in some of the lowest conditions of animal life the sexual element, if not altogether lost, exists in too rudimentary a form for us to recognise its presence.

It may also be remarked, that although the occurrence of atavism in disease is usually effected through the female, yet it occurs also, though less frequently, through the male sex. Illustrations of this are given in many cases of disease already cited, and among the lower animals it may often be noticed as a natural phenomenon. It is well known, for example, that the supply of milk by cows is hereditarily influenced by the bulls rather than by the cows from which they are directly descended, and that the character of the secretion, as regards both the quantity and the quality of the milk, is chiefly derived from the paternal grandmother by atavic descent;† and as we descend still lower in the scale, we find, for example, in the case of insects, evidence

* The above fact has been well described in the following passage from a recent writer on zoology: "A remarkable peculiarity has been observed in plant-lice (*Aphides*), where a single impregnation suffices for many families in succession; the males are not observed until the end of summer or in autumn; they impregnate the last family, consisting of wingless females, which, without copulation, would be barren. Their eggs remain during the winter on branches of trees, and in spring produce only female plant-lice, which, without copulation, are prolific and viviparous." (Handbook of Zoology. By J. Van der Hoeven. Vol. i. Translated from the second edition by the Rev. William Clark, M.D., F.R.S., &c. pp. 263–4. Cambridge, 1856.)

† Burdach: Traité de Physiologie, tom. ii. p. 117; and Girou, op. cit. p. 127.

more or less decisive in favour of the transmission by either sex of the distinctive peculiarities of the other; whilst the capability of both sexes in the human race to transmit disease by atavic descent is occasionally illustrated by the occurrence of cases in which the transmission is effected by a male and a female branch of the same family, as in the following case related by F. Meckel,* in which the modified influence of sex is associated with atavism of unequal remoteness: "A man whose palate was entire, but uneven, as if cicatrized, had by a perfectly healthy wife seven children, of whom the four boys were well-formed, but the three girls had hare-lip and divided palate. His mother's sister had also seven children, five sons and two daughters, of whom the former were all similarly deformed."

There is some uncertainty respecting the extent to which the influence of sex may be modified in those cases in which, through the double inheritance of the disease, atavism is associated with direct descent. For although it might, as a rule, be expected that when the parents are similarly affected, the children of both sexes would share equally in the inheritance, yet when, on the contrary, the inheritance, although double, is derived, as sometimes happens, from ancestry of the same sex, it might be expected that it would as a rule be in like manner restricted to descendants of the same sex; and although exceptions are occasionally noticed, still there is satisfactory evidence in favour of such restriction. In addition to the cases already cited, the following case of hereditary cataract, observed by Dr. Hart Vinen (who has favoured me with his notes of the case), affords a good illustration of this subject. The patient was a gentleman who came to this country from Mauritius to be operated on for cataract in both eyes: his father had double cataract, which had been operated on in Mauritius with partial success; his maternal grandfather had double cataract, which was operated on in Isle of Bourbon; and two maternal uncles, sons of the above grandfather, had each of them double cataract, whilst their four sisters, one of whom was the patient's mother, were quite free from the defect.

Lest it should be supposed that undue stress has been laid on the hereditariness of sexual limitation in disease, and that its occurrence, both in direct and atavic descent, is too exceptional to justify some of the remarks which have been made on the subject, it may be convenient again to direct attention to the fact that phenomena which seem to occur only exceptionally in one class of organized beings, may be and not unfrequently are the result of established rules in the development of others; for there is a general agreement in all the operations of nature, so that even the successive stages of development in our own race to some extent resemble the completed development of the lower animals. This analogy, however, becomes still more noticeable in abnormal development, the reproduction of which constitutes hereditary disease; and it is probable that the frequency with which limitation by age is conjoined with that of sex, in many of the preceding cases,

* Lancet, 1829-30. Vol. i. p. 792.

is to some extent dependent upon this lowering principle. Hence, therefore, it may be assumed that the study of the comparative physiology of reproduction is capable of leading us to the knowledge of fresh truths in development, one of which is the hereditariness of sexual limitation, hitherto considered chiefly in connexion with disease in the human race, but which is also a recognised law in the reproduction of many insects and other invertebrate animals, in which the limitation by sex occurs naturally for a certain number of generations, and then usually ceases abruptly through the operation of some unknown cause. In addition to the illustrations already adduced, it may be stated that the wingless females of lepidoptera have been observed to go on producing (parthenogenically) unimpregnated eggs and wingless females from them for many times; whilst the queen or perfect female bee, before impregnation, lays eggs which produce males only.* It may be useful also to notice that in addition to the reproduction of sex in cycles, in which, as for example in the aphis, one generation composed partly of males alternates with several complete generations of females, there is another form of parthenogenesis in which a still more exclusive limitation by sex prevails, for the race is constituted solely of females, which produce perfect eggs, to which no males have been found to belong; this appears to be the case in all true gall flies (*cynips*),† in many of the entomostraca,‡ and it has been observed also in animalcules, such as the rotifera.§ It would perhaps be digressing too far from our subject to enter more fully into this interesting inquiry, but it is important before leaving it to notice that the phenomenon of parthenogenesis, which is intimately associated with limitation by sex, does not suddenly cease here, but occurs also in vertebrate animals, and in like manner independent of disease: for the virgin ovulation of birds, the virgin lactation of mammals, and also the development of corpora lutea in unimpregnated females of the human race, show that the traces of its occurrence are to be met with in the most highly-organized animals; || the tendency of the non-union of the sexes at the proper season being in many cases simply to delay rather than to prevent reproductive development. It may therefore be inferred that as sexual limitation is associated with this low form of reproduction in many of the lower animals, and as its vestiges can be traced throughout the animal series, so also it would be likely to reappear in connexion with the lowering influence of disease; for what-

* On a True Parthenogenesis in Moths and Bees: a Contribution to the History of Reproduction in Animals. By C. Th. E. von Siebold. Translated by W. S. Dallas, F.L.S., &c. 1857.

† Ibid. p. 65. ‡ Ibid. p. 66.

§ The absence of male rotifers is supposed by Dr. Carpenter (The Microscope and its Revelations, third edition, pp. 490–1, 1862) to be due to their being produced only at certain times; a supposition which is rendered probable by the fact that the production of female rotifers is viviparous.

|| M. Delafond: On certain Physiological Phenomena connected with Parturition and Lactation in Bitches that have not been Fecundated when in Heat (L'Union Médicale, tom. ii. No. 61, 1857). Sir Everard Home (Lectures on Comparative Anatomy, vol. iii. p. 306, 1823) states that on examining the ovaria of a virgin forty-seven years of age, he found seven corpora lutea in one, and five in the other.

ever tends to develop a lower organization must tend also to develop a lower class of phenomena,* and as it would be unreasonable to suppose that the one could ever be altogether disassociated from the other, we are led by the comparative physiology of reproduction to expect the occurrence of sexual limitation in all forms of hereditary disease, whether direct, atavic, or collateral.

The amount of information which has already been derived from the above source, leads us to anticipate that further cultivation of this, until lately, almost uncared for field of inquiry, will yield so rich a harvest of facts, that much of what appears to be now obscure and irregular in hereditary disease will hereafter admit of being satisfactorily accounted for and explained; as it is probable that the same fundamental principle in organic development everywhere prevails. For even in the lowest forms of protophytes, which multiply by duplicate segmentation, and are commonly supposed to be independent of any sexual influence, reproduction, as distinct from numerical increase, is effected by a process of conjugation, consisting of the reunion or fusion together of a pair of cells, which becomes the spore or primordial cell of a new generation subsequently evolved by successive repetitions of the segmentary process; whilst in the higher forms of protophytes, reproduction is effected by the union of distinctly sexual structures evolved from what appears to be an unsexual parent. And

* This principle in development is well illustrated in the case of insects which undergo metamorphosis, for the power of reproducing—or as it may more conveniently be termed, of reconstructing (or restoring)—lost parts, which they possess only in the larval condition, is replaced in their perfect state by the reproduction of the species. (On the Reproduction of Lost Parts in Myriapoda and Insecta. By George Newport, Esq., F.R.C.S. Phil. Trans., pp. 283–294, 1844.)—And in the same way also that the development of flowers arrests the growth of annual plants, so the development of offspring in insects which have attained their "full perfection" is substituted for the power of self-increase, which exists only in the larva.

It is necessary, however, to state that some difference of opinion seems to prevail in the mind of one of our most distinguished writers on this subject, who remarks, with reference to the development of the concluding generation of Aphides, that "we have, in fact, at length male and female individuals, preceded by reproductive individuals of a lower or arrested grade of organization, analogous to the gemmiparous polypes of the zoophyte and the leaves of the plant."—(Owen's Parthenogenesis, p. 60, 1849.) Notwithstanding the weight of so great an authority, it may be allowable to observe, that although the plant-lice of preceding generations are here rightly distinguished as belonging to "a lower or arrested grade of organization," in accordance with what appears to be a fundamental principle in parthenogenesis, there is an essential difference to be noticed between their development and that of leaves; for whilst the imperfect aphis exists as a distinct individual, and as such can reproduce its kind, the leaves of the plant remain attached to the parent stem, and they cannot manifest any independent power of a like character, since it is not from the leaves individually that a fresh crop of leaves is produced; and although John Hunter's statement that "every part of a vegetable is a whole," might apparently lead to this conclusion, yet the part in this case does not form an independent whole. Hence this so-called reproduction of leaves, which bears the same relation to the plant as the so-called reproduction of a lobster's claws does to the animal, of which they form simply a part, cannot be regarded in any way as analogous to, but on the contrary as altogether distinct from, the phenomena of alternate generation and parthenogenesis; just as successive crops of pustules in a skin disease are the result of a process altogether distinct from that by which a skin disease is reproduced in succeeding generations in direct or in interrupted descent.

this form of reproduction, which is continued in algæ, lichens, and mosses, superseding in them altogether the process of numerical increase by segmentation, attains its highest development in ferns, which, although unsexual in their mature condition, exhibit the restrictive influence of sex in their reproductive development.* With respect to the reproduction of these latter plants, which takes place from spores, Dr. Hofmeister† has observed that it is effected by the intermediate development of a structure called the prothallium, from which the germinating organs are produced; the male organ (*antheridium*) giving birth to a spermatozoon, whilst in the female organ (*archegonium*) is formed the embryo-sac; "when a quantity of fern-spores are sown (Dr. Hofmeister states), the germinating *prothallia* are developed at very different periods; the earliest *prothallia* produce in the first instance only *antheridia*, afterwards *antheridia* and *archegonia* together, and when advanced in age, only *archegonia*." It will be here observed, that the development of the male and female reproductive organs, which apparently occurs independent of the unsexual parent itself, takes place in distinct and periodic groups, although the ultimate union of the male and female elements is essential for the production of another generation of ferns; and it will, moreover, be noticed, that whilst development in ferns proceeds from a perfect but unsexual plant to the formation of imperfect but sexually distinct structures needed for reproduction, in insects and other invertebrate animals, it proceeds from perfect and sexual parents to that of imperfect and unsexual offspring (larvæ) incapable of reproduction.‡ Finally, it may be remarked, that in the same way that phenomena which have been shown to prevail among the invertebrata may be traced throughout the animal series, so likewise in the case of plants may corresponding analogies be perceived; for if we pass from the reproductive phenomena of protophytes and the higher cryptogamia to those of phanerogamous plants, it will be found that the lower and more exceptional forms of reproduction in the latter, illustrating the restrictive influence of sex, agree in principle with what has been shown to be the normal condition in the development of the former plants. This is well illustrated in a fact observed by Mr. Knight, that "cucumber and melon plants will produce none but male or staminiferous flowers if their

* Dr. Allen Thomson: op. cit.—Dr. Carpenter: op. cit., and Comparative Physiology, fourth edition.

† On the Germination, Development, and Fructification of the higher Cryptogamia; and on the Fructification of the Coniferæ, translated by Frederick Currey, M.A., F.R.S. Ray Society, 1862.

‡ This inverted relation is further illustrated in the case of ferns by the phenomenon of variation, technically called "sporting," for not only do fern varieties exhibit a strong tendency to perpetuate themselves by spores, but even stranger forms still are liable to be developed from them; and their tendency to revert to the original type is so slight that Mr. Bridgman informs us that "out of some thousands of *Filix-mas-cristata* seedlings, only one reverted to the normal form, and two others closely approach the *angustata* of Sim, all the remainder being identical with the parent." (On the Influence of Variation in the Reproduction of Monstrosities among Ferns: Annals and Magazine of Natural History, December, 1861.)

vegetation be accelerated by heat; and all female or pistilline if its progress be retarded by cold."

Thus guided by the light which the comparative physiology of reproduction casts on hereditary disease, we are enabled to explain how many cases which have hitherto seemed to contradict those principles in the development of disease which have been contended for in the preceding pages may be found to essentially accord more or less fully with them—as, for example, the case observed by Girou,* of a sporting bitch with a cleft nose, the issue of a father with a cleft nose and of a mother with a common nose, who had eight pups at one litter, four of which were males with a cleft nose, and four females with a common nose; the grandfather in this case transmitting through the daughter his own defect to all his grand-sons, whilst all his grand-daughters are free from it. The exceptional fact here is, that the daughter should both transmit and also inherit the defect, for it must be conceded that the grandsons probably derived the defect from their grandfather rather than from their mother, otherwise it would be difficult to explain its absence in the four grand-daughters; and the case, with this explanation, agrees with one of the most common forms of hereditary disease, and finds a parallel in the ordinary development of some of the invertebrate animals. It is, moreover, evident from such cases as the preceding, and many corresponding cases might be quoted, that as regards atavism in disease, whether associated or not with limitation by sex, no fixed boundaries, recognisable by us, can be expected to limit its operation, for, like other general laws in nature, unity in principle co-exists with variety in results; and it is chiefly because we are less familiar with the results of atavism in disease than we are with many other reproductive phenomena, as for the sake of illustration, with memory, that we hesitate to accept them, although they are not in themselves more exceptional or peculiar than some of those are which we not only never hesitate to accept, but with which this phenomenon in morbid development seems to be closely allied. For atavism in disease appears to be but an instance of memory in reproduction, as imitation is expressed in direct descent; and in the same way that memory never, as it were, dies out, but in some state always exists, so the previous existence of some peculiarity in organization may likewise be regarded as never absolutely lost in succeeding generations, except by extinction of race. Consequently it may be inferred that in some distant descendant hereditable disease will again be revealed, and exhibit so complete and exact a copy of its prototype, that were it possible to compare notes, the resemblance in many cases of atavic inheritance would be not less startling than in the few which have been already cited; for in the illustrations of remote atavism which have been presented to our notice, there is not only shown to be a more or less perfect recollection of the disease itself, but also of the sex which it had endowed.

* Op. cit., p. 121.

Art. II.

On Mycetoma. By H. V. Carter, M.D. Lond., Assistant-Surgeon, Bombay Army.

In the following remarks it is proposed to give some account of a very serious disease widely prevailing in India, and in its nature and pathological characters well worthy the attention of the surgeon and naturalist. "Mycetoma" stands for a form of swelling which is caused by the growth of a fungus. The term is sufficiently expressive, and briefer than, if not otherwise preferable to, that of "Fungus Disease," under which I first described the affection. Since those observations were made (March, 1860) many facts have come to light which almost complete the natural history of mycetoma, so that it may not be premature to offer the following as at least a basis for subsequent research.

A condensed description of the pathological characters of the disease, and a short account of its natural history, will be presented in succession; the facts upon which both are founded being entirely derived from personal observation.

I. The feet and hands are the only parts attacked; but this feature of the affection, as also its local or endemic character (which must still be called Indian), may require to be modified in the course of time and after more extended observation. Patients present themselves with a foot or hand (generally the former) much swollen, of a dark colour, and studded with numerous sinuses; the form of the swelling is more or less globular, and as to its extent, the whole of the member, or one side or part only, may be implicated. In the former case the projecting fingers appear to be imbedded, being themselves generally free; and the sole or palm is flat or even convex. Seldom does the disease extend much beyond the ankle or wrist, and its whole appearance, at first sight, somewhat resembles a long-standing scrofulous affection. The sinuses are considerable in number, and often clustered together about the sole, ankles, or dorsum of the foot; some are simple openings, others are raised upon soft elevations or present a pouting edge. The appearance of the more recent, especially in preserved specimens, is characteristic, being circular in form, from one-third to one-half inch in diameter, and gradually deepening towards the central aperture, from the removal of successive layers of cuticle; white patches are frequently seen around. The size to which the swelling may attain varies: in advanced cases its circumference may be eighteen inches, or upwards, and the form is then hugely misshapen.

Any one who is acquainted with the fungus-disease could not mistake it, when tolerably advanced, for ordinary caries; the size of the foot, its globular form, and the number and appearance of the sinuses, being the chief diagnostic characters; to which may be added, the absence of a corresponding degree of constitutional disturbance, pain, or hectic fever, and the patient is generally of a scrofulous or syphilitic taint. But there is one test which is applicable in almost all cases, and that is the character of the discharge. Sometimes the fungus-particles are so abundant as to block up the apertures of the sinuses, or float away in numbers in the thin serous or sero-purulent fluid, and when less numerous they may generally be detected with the aid of a lens. In the black variety a single glance will be sufficient, and in the pale and soft (which have been well compared to mustard or poppy seeds), their appearance is hardly less characteristic. The presence of these particles in the discharge from the sinuses is an infallible test of the nature of the disease; and by the use of the microscope I was very early enabled to make a correct diagnosis in a rather obscure case, but generally this aid is not required. The external appearances of mycetoma appear to be the same whatever the form of fungus-growth. The sinuses are the terminations of canals, more or less lengthy and tortuous, which occasionally lead to bone; but the latter will not usually yield to pressure of the probe, for it is not really in a carious condition, although partly absorbed.

A section of a foot thus affected presents on first view much confusion of parts. The skin is greatly thickened, and the bony, muscular, and fibrous tissues seem blended and intermixed with a glairy or tenacious slough-like material, of reddish or greyish tint; globular masses of fungi, too, are seen scattered about, which are either yellowish and of cheesy consistence (the so-called tubercles?), or deep brown or black, and much firmer. With a little attention, however, the following arrangement becomes evident: the collections of fungi are lodged in spherical cavities hollowed out in the osseous cancellous tissue, or in the soft parts, from which "loculi" branching tubular canals pass off, frequently inosculating, and terminating either in closed expanded extremities or on the surface at the sinus-apertures. These canals, like the loculi, are lined throughout by a membrane, easily separated from the bone or blended with the softer tissues, and they also contain fungus-particles imbedded in the soft or glairy material above-mentioned; it is evident that their office is to conduct the fruits of the vegetable parasite to the external surface, where they are expelled in the serous discharge. A varying amount of inflammation, with its results, attends the growth of these foreign bodies, and the bones of the foot and leg, or of the hand, are affected in a striking manner, which, however, need not here be described; the spherical cavities which they contain are the most peculiar feature, and caries, or ulceration of the articulations, is seldom present, absorption from pressure being the only agent at work.

Mycetoma makes its appearance by a small, flattened, indolent tumour or "lump," firm to the touch, little painful, and of slow growth. In the course of a few months raised soft spots, or blebs, or vesicles, arise, which soon burst and let out the fungus-particles; sinuses thus are formed, and persist until all are expelled; meantime the swelling enlarges, or fresh ones appear, and so the disease progresses. The commencement is often on the sole of the foot; or, in the case of the hand, one of the fingers may be first attacked, as a most interesting specimen in my possession shows.

The natural duration of the disease is prolonged, the cases ordinarily seen being of from four to ten years' standing, and sometimes longer; its termination seems only coeval with exhaustion of the vital powers. A spontaneous cure must, I think, be exceedingly rare, though doubtless within the range of possibility. Some idea of the frequency of this unique affection may be gained from the fact that individual observers in this country have reckoned their cases by the score; one gentleman sent me particulars of seventy-five cases he had treated, and even in Bombay a year seldom passes without three or four cases being seen at the Jamsetjee Jejeebhoy Hospital, although the disease is not endemic here. It has only been seen in natives hitherto, the explanation of which is obvious, as they alone go barefooted, and seldom wash the feet thoroughly. Other noteworthy features are the following: it has mostly a single local manifestation; it is much most frequent in men, and during the middle periods of life, and commonest amongst the agricultural classes; it is not hereditary, or peculiar to any diathesis. In all these particulars, as well as in its endemic character, the fungus-disease resembles the guinea-worm disease, and is unlike scrofulous affections, leprosy, elephantiasis, &c.; it is indeed a much more serious affection than the Dracunculoïd, and merits far more the attention of medical officers in India.

As to treatment, amputation is a certain cure, so long as every part invaded by the growth is removed, and it is necessary to mark this, as partial amputations have failed. Were it possible (of which I have strong doubts) to destroy the vegetable growth by local applications or injections, this plan might be adopted previous to the more serious procedure; a very intelligent graduate of the Grant College sent me word that he thought he had eradicated the disease by the free use of strong nitric acid, but time must tell how far such means are really available; the wonderful fecundity of the parasite and its deep penetration into the tissues, seem to me almost insuperable obstacles.

II. *Description and Natural History of the Fungi.*—These foreign bodies—the sole cause of disease—are not of a uniform appearance; but as the history, course, and appearance of the disease seem to be in all cases the same, so it may be inferred, is its exciting cause; and it is well-known that these low-organized growths are susceptible of great modifications according to external circumstances; some experiments, to be presently mentioned, indicate, moreover, a common origin of the two most frequent forms of fungi. Of the three varieties distinguished below, two were first noticed by myself, and I also was enabled to detect the real nature of the second or most common, which had been previously described by my colleague, Dr. G. R. Ballingall, and by him submitted to the late lamented Professor Quekett,* without, however, a definite opinion being elicited of its character.

* I would here beg permission to add my testimony of respect and regard to the memory of that amiable and talented man, in whose society I passed many profitable hours during the time I held the Studentship in Anatomy of the Royal College of Surgeons.

1. The black fungus occurs in more or less spherical masses, attaining the size of half an inch in diameter; outer surface of a jet-black colour, and minutely tuberculated; section of a rich deep brown, and radiated in aspect; consistence very firm, friable, and readily yielding along the radii, sometimes tearing like decayed wood. Structure of closely aggregated fasciculi (diam. $\frac{1}{200}$th to $\frac{1}{400}$th in.), cylindrical, beaded, branching and blending, and radiating from a common centre; they are composed of pale, homogeneous fibres (diam. $\frac{1}{1000}$ to $\frac{1}{3000}$ in.), and at their peripheral extremities expand into firm, rounded "heads" of a deep black colour, to the varying projection of which the tuberculated character of the exterior is due. These globular expansions (diam. $\frac{1}{100}$ to $\frac{1}{120}$ in.) are also found at the ends of the shorter branches, and are composed of closely packed cells (beaded cellular filaments?) of an orange tint, interspersed amongst which are larger, thick-walled cells (abortive sporangia?). These larger masses occupy the "loculi" before mentioned, and seem to break up into smaller fragments, each of which corresponds to one or more of the globular "heads" thus become detached; and these black particles, incalculable in number, crowd the canals or sinuses on their way to the outer part of the body; they are somewhat larger than a pin's head in size, and may alone be present in the foot. Containing the reproductive elements, these black particles will, under favourable circumstances, germinate, and we then find a red mould-like fungus spring up; this is probably the parent, so to speak, or normal form of the black fungus of mycetoma. The latter also occurs in another condition, having undergone degeneration (fatty) in the foot, leaving lighter coloured masses, crystalline in consistence (stearine or margarine?) and devoid of structure; this change is an approximation to the next variety.

2. Small masses of cheesy consistence and light-brown tint, formed of an aggregation of granular particles, and occupying the same "loculi" as the above. The granules or particles are quite visible to the unaided eye, and resemble poppy seeds; their number is immense, and they are freely discharged by the sinuses. Each consists of numerous minute rounded or angular bodies (diam. about $\frac{1}{60}$th in.), which are enveloped on all sides by a deep crystalline fringe (stearic?), thus presenting a curious appearance, enough to perplex both Dr. Ballingall and Professor Quekett, more especially as the rounded bodies appear to be structureless, or only finely granular. It was not until I met with a specimen of this variety of mycetoma, in which the fungus particles were free from the crystalline fringe, and still showed a cellular structure, that I learned the true nature of these bodies; they are degenerated fungi. In their interior may sometimes be seen clear nucleus-like forms, which somewhat resemble spores, but which are probably oil globules.

3. Once I found countless minute pink-coloured particles, visible to the eye as reddish grains (like Cayenne pepper), and when magnified exhibiting a bi-, or multi-partite aspect of regular arrangement; when single the particles were oval, and resembled, more or less, in size and structure, the bodies just described, but they possessed the property of

multiplying themselves by subdivision, and their colour was different. The cellular structure was not apparent, but I conclude that it once existed. The crystalline envelope was absent, though, as in other cases, much free fat (also of a pink tinge) was seen.

On the present occasion it is not necessary to enter into further details respecting these parasitic growths; I hope one day to record the results of closer examination, and now proceed to offer a few remarks on the natural history of mycetoma.

It may be regarded as certain, that the hand or foot becomes accidentally inoculated with the spores of some mould or mould-like cryptogam, which at certain periods of the year—most likely previous to or during the wet season, when all kinds of fungi abound—makes its appearance on the soil of particular localities : the naked unwashed feet of the agricultural labourer must be peculiarly liable to such contingency, and it is not necessary to infer the pre-existence of an artificial abrasion of the cutaneous surface, as the spores are quite capable of passing into natural apertures—e.g., the sweat ducts. In many specimens I have noticed pinkish streaks in the substance of the skin and subjacent tissue, on the sole of the foot, &c.; and on further examination, finding these streaks to contain numerous spore-like cells in various states of growth, I conclude that they constitute the first stage of development of the disease.

As to the specific character of the parasite, I was at first strongly inclined to compare the fungus of mycetoma with the "rusts" and mildews which attack so many cereals and grasses, and to ask if it is not possible that the species infesting common Indian grasses, &c. (e.g., sorghum, maize), if transplanted into the human foot, might not give rise to the disease; but more recently, as the result of further inquiry, and in deference to the opinion of the Rev. M. J. Berkeley, our great British authority, with whom I have had the advantage of corresponding on this subject, I am inclined to surmise that the human fungi correspond to those imperfect states of ordinary moulds, &c., which have been distinguished by the term "sclerotia." Under certain unfavourable circumstances, the mycelium of the mould ceases to put forth the organs of fructification, and assumes the form of a firm, compact, cellular substance, capable of resisting adverse influences, but also susceptible, under more favourable circumstances, of again developing into the normal or fundamental species—a phenomenon essentially analogous to what occurs in the lowest forms of animal life, and, in a far less degree, to the hybernation of some of the higher.

Now it happens, that on several specimens of mycetoma, placed in spirit or water shortly before the monsoon season, a red mould has appeared on the exposed surfaces, whilst other preparations similarly placed have not shown any such appearance; again, fungus-particles from the foot, set in moistened rice-paste, have also given rise to the same mould, whilst plain rice-paste, placed side by side, has been either unaffected or only yielded common forms. I have recently ascertained this fact, with respect to both the chief varieties of the disease, and it throws clear light upon the origin and nature of this destructive parasite.

Mr. Berkeley's opinion is to the same effect, and he informs me that he should name this red mould *Chionyphe* (mucor ?) *Carteri;* I had not ventured, from want of practical knowledge, to suggest a name, although the fungus was fully referred to by me last year (1861).

It cannot be denied that this mould has not been seen in its natural locality, and also that cotton soil from the affected districts has failed to yield it when moistened and exposed to air; but the observations and experiments that have been made are as yet too few and incomplete to enable us to speak positively on this part of the subject.

Did space permit, I should gladly point out the numerous analogies that exist between this unique parasitic affection and other entophytic and entozoic diseases, all of which, it seems to me, to transcend, in both interest and importance; but I trust enough has been said to afford some idea of its appearance and characters.

Note.—Further information may be found in the last three volumes of the 'Transactions of the Medical and Physical Society of Bombay,' especially No. 6, and Mr. Berkeley has just published, in the 'Intellectual Observer,' a short description of the fungi, based chiefly on my memoirs, and illustrated by figures which, to a certain extent, agree with my own, but partly differ; the characters of *Chionyphe Carteri* are laid down as follows—"hyphasmate ex albo flavo-rubroque; sporangiis demum coccineis; sporis breviter fusiformibus."

With regard to another subject, that of leprosy, a short account of which lately appeared (Jan. 1863) in this Review, I would add that additional information, necessarily omitted here, may be found in the eighth volume of the same 'Transactions.'

Art. III.

Medical Results of the Recent Chinese Wars. By T. Nelson, M.D., Staff-Surgeon R.N.

By the recent publication of the 'Medical Returns of the Navy for the Year 1859,'* not only is much valuable information given of the general state of health amongst the seamen and marines all over the world, but very important data are furnished for enabling a comparison to be drawn between the medical results of the first Chinese War, and the second, which was brought to a final close shortly after the period embraced by the present Report.

So favourable an opportunity of comparing the influence of two separate but similar expeditions upon the health of those exposed to them very rarely occurs, and it has been accordingly thought worthy of a more special attention and inquiry, in order, as far as possible, to observe what progress has been made in the general treatment of diseases in the East, and in the development of those measures which are considered best fitted either to ward off their presence, or at all events,

* See our number for April last, p. 441.

to mitigate their destructiveness. In order, however, to draw the comparison correctly, and to extract from it such lessons as it may be able to convey, a few briefly expressed historical facts must be recalled to memory, along with certain collateral circumstances, so as to place the whole subject in a well-defined and perspicuous light.

It will be recollected that the first Chinese expedition began operations in the month of June, 1840, and was brought to a conclusion by the treaty of Nankin, in August, 1842. So that the period of actual hostilities may be said to have lasted two years and two months. The second series of warlike operations broke out towards the close of the year 1856, and terminated for a time with the treaty of Tientsin in July, 1858. They recommenced in June of the following year, and ended finally in 1860 under the walls of Pekin. For the purpose at present in view, each series of operations may be regarded as embracing a period of three years.

The auspices under which the two expeditions were initiated can scarcely, however, be deemed alike. When the necessity of having recourse to arms against the Chinese became obvious in 1840, our state of preparation was far from favourable. It is true that the same year had been illustrated by a great naval achievement—the siege and capture of Acre. But this was the first gleam of splendour shed upon the naval service after a long and dreary night. The brilliant victory of Navarino had taken place long before, and during the interval the navy had been subjected to a series of economic experiments which cannot now be recalled without amazement at its impolicy. The old experiment related by the Greek humourist, wherein an economist tried to maintain his horse on the least possible quantity of food, was attempted by the Government of that day with respect to the fleet; and it is to other agencies than their political foresight that the catastrophe which befel the horse did not overtake the British navy.

The opposite views of England and France in 1840 on the politics of the East, and the behaviour of the Chinese towards our merchants and traders, aroused this country to a sense of the peril to which an unwise, because niggardly administration of naval affairs, exposed its dignity and safety. It then became again obvious that to maintain our political influence in Europe, and to vindicate our honour, as well as to protect our commercial interests in the remote but lucrative regions of China, the huckstering spirit relative to the navy must be abandoned. The commencement of hostilities with China, accordingly, found the British navy just recovering from the severe regimen it had been for a time subjected to. The half-manned ships had been completed with their proper crews; the guns which had been removed were again replaced on their decks; and numbers of officers who had been for years consigned to a painful inactivity were recalled to active service.

It might reasonably be assumed that the naval service had acquired something of that inaptitude which the benumbing shade of economy and a prolonged quiescence was likely to engender. For many years no opportunity had been given of equipping and conducting extensive naval expeditions. Until the Syrian operations, a perfect calm had prevailed, sufficient to enervate men less thoroughly imbued with professional skill and energy than our officers and seamen happily proved to possess. These operations, culminating in the capture of Acre, were evidence enough how rapidly and effectively hostilities could be carried out at a *short* distance from England, in the old and classic arena of British naval triumphs.

But the task which the first Chinese expedition imposed was of a different character. There were difficulties here to be met and overcome that had never before been presented to the planners and the conductors of a naval expedition. The country which it was proposed to act against was utterly unknown beyond the beaten track of a single river, and the narrow suburb of a provincial city. The military resources of the government and the warlike spirit of the population were enveloped in mystery. The climate of the country, and what influence it would exert upon crowds of Europeans, were questions which had yet to be answered by a painfully-acquired experience. The only sources of information we could boast of, were the memoirs of two abortive embassies, whose opportunities of observation had been jealously confined to a beaten highway and the banks of a canal. The sole facts, indeed, of which there could be no doubt were, that the country to be attacked was seventeen thousand miles distant from England, and twelve times as populous. Curiously enough, the first was considered as the most formidable of the two. It was felt to be no ordinary exertion of administrative effort, to equip and conduct an extensive expedition, which had to traverse the greater portion of the globe's surface before reaching its destination, and whose nearest source of supply for food and stores when there, were the distant British settlements of Trincomalee and Singapore. To find a true parallel to this adventurous undertaking, we should have perhaps to go back to the very infancy of the nautical art.

In the face of these difficulties the Chinese expedition of 1840 was undertaken, and was conducted, after a series of arduous exertions, to a successful close. Those who are familiar with the events which marked it will readily admit, that the efforts it demanded were heavy and harassing; that the hardships and exposure it entailed were very great; and that its achievements were both brilliant and substantial. Like other expeditions before and since, it had to pass through its phases of mismanagement and vacillation. By sad experience, its leaders had to learn how obnoxious certain seasons of the year in China are to European constitutions, and how much this baleful influence can be aggravated by exposure to certain localities. Let it be remembered, that there were no records in existence of warlike operations against such a people, and in such a region. Where to go, what to do, and how to go about it, had to be struck out on the spot. The errors of to-day were to serve as the beacons of to-morrow, and all reliable information had yet to be gathered together, whether it related to the nature of the country, the character of the people, and, what most concerns the present remarks, the climate and the endemic diseases which prevailed.

Without entering into details, it will be sufficient for our purpose to glance rapidly at the more prominent achievements which were effected under those circumstances. First, there was in the south, the blockade of the Canton river, and the capture of the Bogue forts with 459 guns; then the occupation of the heights of Canton, and the subsequent ransoming of that city. In the north, Amoy was captured, with 296 guns; Tinhae, with 136 guns; Chinhae, with 157 guns; 12,000 Chinese were repulsed with great loss at Ningpoo; 8000 Chinese routed near Tze-kee. Subsequently, the great river of Yang-tze-Kiang being entered, Woosung with 230 guns, and immediately after, Shanghai were captured. Finally, the whole British force having been massed at Golden Island, Chin-Keang fell, after a vigorous defence by the Tartar troops; and shortly after, the treaty of Nankin was signed, which brought hostilities to a permanent close. The time occupied during these operations was rather more than two years, the blockade of the river Canton having commenced in June, 1840, and the treaty of peace was signed in August, 1842.

Bearing these facts in memory, we must now fix our attention on the sanitary history of this first expedition. Under this point of view, nothing could be more complete than the information we possess in the admirable reports drawn up by Dr. Bryson, of the Admiralty, as was previously observed in this Journal. But for the accurate and unimpeachable details so copiously furnished by them, the present task of comparison would be utterly impracticable, and, indeed, could not be thought of at all. In these elaborate statistics are registered everything relating to the state of health and sickness prevalent in our vessels in the East, both under a peaceful and hostile state of affairs. It is, accordingly, entirely by their assistance and guidance that the subsequent details are collected together, and the proposed comparison made possible. But, in order to form a correct idea of the actual increase of sickness occasioned by a state of war, it will be necessary first of all to observe what was the average amount of sickness during peace. For this purpose, we have only to refer to the reports from the East for the year immediately preceding the outbreak of hostilities—viz., 1839.

The mean number of officers and men employed for that year was 2050, and the cases of sickness of all kinds which occurred amounted to 3066. Exhibited in another form, this gives the ratio of 1495·6 for each thousand. Of these cases, 45 were invalided, and 25 died. From this last number have purposely been excluded accidents by drowning, in which climate had no concern.

If we place these numbers in juxtaposition with those furnished by the two subsequent years, it will be seen at a glance how rapidly, by a state of war, disease and death assume formidable proportions.

Year.	Mean strength.	Diseases of all kinds.	Ratio per 1000.	No. Invalided.	Ratio per 1000.	Dead.	Ratio per 1000.
1839	2,050	3,066	1495·6	45	22	43	21
1840	3,330	5,871	1763·1	60	18	111	33·3
1841	4,840	8,816	1821·5	159	32·8	290	59·9
1842	7,300	16,375	2243·2	170	23·3	376	51·5

The amount of sickness which prevailed from 1840 till 1843 on the Chinese station was indeed formidable, and produced an effect on the minds of Englishmen at home which can yet be readily traced in their estimate of the Chinese climate. As is justly remarked in the official report by Dr. Bryson, "The mischief inflicted by it is by no means comprised in the number of deaths recorded. Many who apparently became convalescent, or who returned home invalided, carried about with them the smouldering remains of serious organic disease, which broke out afresh on their return to their native land, and ultimately consigned them to premature graves."

The contrast between the proportionate number of deaths from disease alone in 1839 and 1841—namely, 12 per 1000 in the former, and 57 per 1000 in the latter, is sufficient to impress with sadness the least thoughtful mind on the inevitable consequences of a state of war.

It would be but reasonable to assume that not a little of this serious increase of disease was due to those incidents connected with the first expedition which have already claimed a passing notice—namely, to the novelty of the undertaking, the dearth of information upon many essential points, and the difficulty of procuring wholesome supplies of food. It is well known that for months together salt provisions, and those of a most indifferent quality, were the principal sustenance of the many ships' crews. Vessels which had made the voyage from England, and had paid but hurried visits to one or two intermediate ports, were sent, on arrival in China, to blockade some spot where fresh meat and vegetables were not to be had, and so remained for many weeks.

The commissariat arrangements, which were so well conducted at a subsequent epoch, were not practicable, at the period we now speak of, from our want of settlements and our inevitable ignorance of Chinese ways and customs. The climate also was but imperfectly understood, and no doubt many things were done, productive of mischievous results, which, with a better knowledge, might possibly have been avoided.

Accordingly, although shocked and distressed, we are not greatly surprised when in his comments upon the condition at one time of the line-of-battle ship *Cornwallis*, Dr. Bryson forcibly declares: "It is, in fact, hardly possible to conceive a more deplorable spectacle than that which the crew of this ship presented. The crew of the *Centurion* when she passed round Cape Horn upwards of a hundred years ago, could scarcely have been in a worse condition, with the exception of their being buffeted about in an unknown sea in a crazy, miserable vessel."

A state of affairs very little better is described to have existed in the *Driver* and the *Hazard*, "wherein dysenteric complaints continued to prevail so long and with such a degree of violence, that it might well have been supposed that the cause of the disease existed in the ship herself."

To ascribe, therefore, a considerable share of the sickness of the first expedition to these causes, would appear no more than fair and proper; and as a natural consequence, we would be led to expect that with their removal or modification a great amelioration would result.

To the second expedition, then, we ought to look for furnishing this most desirable issue. In it, advantages were enjoyed which were unknown to its forerunner. By a series of hostile operations we had already probed the depth of Chinese valour and warlike ability; and in the course of many quiet intervening years of close intercourse, we had learned much both concerning the country and its climate. In the north, as in the south, we had formed important settlements, and were enabled to collect information and to procure supplies, the latter to overflowing, when required. Besides all this, our naval organization was at the highest state of efficiency which familiarity with extensive operations and the stimulus of a recent war could bring about. Under these favourable circumstances, to say nothing of the assumed progress of medical science, there existed something like a right to infer, that the medical statistics of the second expedition would exhibit a marked diminution under every head of the table already quoted, and thus show the value of all the advantages we had enjoyed by the decided progress made in maintaining a much higher standard of health amongst the men, and in neutralizing more effectually the consequences of those diseases which it was beyond human means to prevent.

Unhappily, a conclusion so flattering, and seemingly so reasonable, like many of the hypothetical class to which it belongs, bears but indifferently the test of facts. On referring to the Medical Reports, so accurately and so ably drawn up, for the three years during which the second series of hostilities lasted, of which we have returns—namely, 1857, '58, and '59—we extract from them the following tables:

Year.	Mean strength.	Diseases of all kinds.	Ratio per 1000.	Invalided.	Ratio per 1000.	Dead.	Ratio per 1000.
1857 ...	7,080 ...	17,595 ...	2,485·2 ...	378 ...	53·4 ...	328 ...	46·2
1858 ...	11,300 ...	29,990 ...	2,653·9 ...	915 ...	80·9 ...	706 ...	62·5
1859 ...	6,600 ...	15,756 ...	2,387·9 ...	555 ...	84·1 ...	322 ...	48·8

The preceding simple figures at once dispel the pleasing illusion in which we were so much inclined, and with such apparent reason, to indulge. A difference does indeed exist between these and the former figures quoted, but the difference unfortunately happens to be in the wrong direction. Instead of even holding by previous precedents, they show a marked increase. The lowest ratio of all diseases in the second considerably exceeds, it will be noticed, the highest point reached in the worst year of the first expedition; while the ratio of invalided cases is more than doubled, and the ratio of deaths touches a higher average than in any former year whatever.

The first desire which a revelation so unexpected creates, is to discover, if possible, the causes which have led to so unlooked-for a result; and the question at once forces itself upon us, was there anything unusual in the character of the operations which aggravated the baleful influence of climate upon the force employed? On a cursory recollection of the work done, there is nothing that especially suggests itself as being more arduous or onerous than what had been achieved on a former occasion. No doubt the duties which preceded the bombardment of Canton were very harassing, and entailed much exposure and exhaustion upon the men, but it cannot be said that they were more so than what attended upon the blockade of Canton in 1840, and the subsequent occupation of the heights behind that city. Again, the movements in the Gulf of Pecheli, brilliant and effective as they were on the first occasion, although disastrous in the following year, were not accompanied in either case by more, if indeed with nearly so much, privation and exposure to climate, as marked those in the Yang-tze-Kiang in the year 1842, when Chin-Kiang was captured, and a treaty of peace exacted from the astonished and dismayed Chinese. It so happened, throughout the second series of hostilities, that all the remarkable and decisive achievements took place during the most salubrious months of the year. The Fatshan Creek affair occurred in May, the capture of Canton in December, and the two attacks on the Taku forts in June respectively. If we add to this advantage the supply of wholesome food from our well-organized settlements, which never failed throughout the whole period of the war, it becomes plain that we must look elsewhere than to greater hardships and exposure, for the cause of that increase of sickness which distinguishes the second over the first Chinese expedition.

A solution here presents itself of this variance, which at first view looks very feasible—namely, the prevalence of some exceptional epidemic amongst the men, from which their compeers on a former occasion were comparatively exempt. A careful inspection, however, of the different diseases in the "Returns" of the two periods respectively, does not bear out this supposition. It is found, on making research, that the same category of diseases illustrates alike the medical histories of the two expeditions, and that no new morbific agent was introduced into the one that was not known to the other. The only difference traceable is simply in degree. To show this clearly and satisfactorily, we have only to extract the principal diseases from the Returns of the two periods, and collate them together.

To begin with the most numerous class of diseases, although by no means the most fatal—namely, fevers, the following tables will show their comparative prevalence and subsequent effects. First, of the *continued and remittent type :*

Year.	Mean strength in men.	Cases of continued and remittent fever.	Ratio per 1000.	Invalided.	Ratio per 1000.	Dead.	Ratio per 1000.
1840	3,330	269	80·8	—	—	11	3·6
1841	4,840	866	178·9	4	0·8	43	8·9
1842	7,300	1309	179·3	4	0·5	32	4·4
1857	7,080	1442	203·	3	0·4	36	5·1
1858	11,300	4203	371·9	62	5·4	88	7·9
1859	6,600	864	130·9	14	2·2	45	6·8

In the larger number of men employed, a proportionate increase was to be expected; but the second expedition, it will be observed, goes far beyond the former averages, whether as regards the ratio attacked, invalided, or dead. The ratio of attacks is, in fever of the first as compared with the second triennial period, as 439·0 to 704·6; of invalided, as 1·3 to 8; and of deaths, as 16·9 to 19·8 in the thousand men employed.

Proceeding to the next type of fevers—namely, the *intermittent*—the most common, although happily the least formidable of the endemic diseases of the East, we again collate from the Official Returns of the two periods:

Year.	Mean strength.	Cases of intermittent fever.	Ratio per 1000.	Invalided.	Ratio per 1000.	Dead.	Ratio per 1000.
1840	3,330	117	35·1	—	—	2	—
1841	4,840	1582	326·9	12	2·5	24	5
1842	7,300	3843	526·4	18	2·5	22	3
1857	7,080	2875	406·1	11	1·6	3	0·4
1858	11,300	4358	385·7	18	1·5	5	0·4
1859	6,600	2609	395·4	7	1·1	—	—

Here, again, the average rate of attacks in the later expedition exceeds the former in the proportion of 1187·2 to 888·4 per thousand men employed. But it is gratifying to notice that, both in deaths and invaliding, the scale is decidedly inclined in favour of the more recent period. Indeed, when we come to consider the fact, that out of an aggregate of cases of continued and remittent fever, amounting to 2444 in the first expedition, the number of deaths was only 86, and the number of invalids 8; and that in like manner, in the last expedition, of 6509 cases treated medically, no more than 169 died, and 79 were invalided, there is great cause for congratulation on the general success of the measures adopted by the medical officers of both expeditions to counteract the lethal influence of two forms of intertropical fevers which, from the violence of their symptoms, and the prostration they occasion, must always be regarded with great anxiety.

With respect to the milder type of intermittents, the same remarks apply even more forcibly, inasmuch as out of 5542 cases which occurred in the first triennial period the loss by death was only 48, and 30 by invaliding. And even this small proportion is still more strikingly diminished by the gross results of the second epoch, when it is observed that out of 9842 cases treated, only 8 died and 36 were rendered unfit for further service.

It could be wished that the decided decrease in the number of deaths and of men invalided, as above shown with reference to intermittent fever, had been extended to the more serious form first tabulated, and that we had been called upon to notice a general instead of a partial success in our more recent experience of intertropical fevers. But, in fairness, the balance against the second series of results ought to be ascribed to the existence of a more aggravated form of continued and remittent fevers which, to a certain extent, is countenanced by their being more prevalent throughout the squadrons. After all, the difference, especially as regards mortality, is not great, and may fairly be placed to the account of that oscillation of effects which appertains to all endemic and epidemic diseases at different periods.

We next come to what may be regarded as the severest of the scourges of the Chinese climate—namely, affections of the bowels. These, under the respective heads of diarrhœa and dysentery, we shall at once proceed to collate as given in the Official Returns:

Year.	Mean strength.	Cases of diarrhœa.	Ratio per 1000.	Invalided.	Ratio per 1000.	Dead.	Ratio per 1000.
1840	3,330	925	277·8	9	2·7	3	0·9
1841	4,848	778	160·7	9	1·9	9	1·9
1842	7,300	2254	308·8	10	1·4	21	2·9
1857	7,080	2920	412·4	—	—	—	—
1858	11,300	5070	448·6	—	—	—	—
1859	6,600	2260	342·4	24	3·6	1	·2

Year.	Mean strength.	Cases of dysentery.	Ratio per 1000.	Invalided.	Ratio per 1000.	Dead.	Ratio per 1000.
1840	3,330	508	152·6	7	2·1	27	8·1
1841	4,840	878	181·4	40	8·3	95	19·6
1842	7,300	716	98·1	13	1·8	66	9·0
1857	7,080	470	66·4	121	17·1	123	17·3
1858	11,300	990	87·6	324	28·6	252	22·4
1859	6,600	546	82·7	161	24·4	90	13·6

A careful comparison of the foregoing table unfolds a state of matters far more calculated to awaken anxiety than to call forth expressions of satisfaction. In the aspect now presented to us of the two expeditions, the later one appears in a much worse light than its predecessor. If we look at the rate per thousand of attacks from the milder forms of diarrhœa, we shall find that the second period greatly exceeds the former. The aggregate rate numbers for each triennial period respectively is as 1203·8, to 747·3. Under the graver form of bowel complaints—dysentery, the later epoch, it is true, shows in its rate-column a marked decrease in point of numbers attacked, being in correct figures as 236·7 to 432·1 per thousand. But the promise which this agreeable fact at first sight holds out is immediately after dispelled, when the rates of invaliding and of death come to be compared. Here the difference between the respective numbers is so marked as to give rise to a feeling of painful astonishment. During the first period, from 1840 till 1843 inclusive, the gross number attacked was 2102, of whom 60 were invalided, and 188 died. During the second period, from 1857 till 1859 inclusive, the number attacked was 2006; of these, 606 were invalided, and no less than 465 died. It must be confessed that the great discrepancy between these respective results (for the disparity does not extend to the respective numbers attacked, less than one hundred being the difference, and even this is in favour of the more recent period), furnishes matter for the gravest reflection and most anxious inquiry.

Alluding to the mortality as quoted above, Dr. Bryson says—" By far the greatest number of these deaths occurred in vessels stationed in the Canton river, and in the Marine battalion stationed at Canton.

Amongst the latter (out of a force of 1500), there were 39 deaths, and amongst the crews of the gunboats there were 24. The great mortality among these two bodies of men was undoubtedly owing to their greater exposure to climatic agencies, and to the irregular, dissipated lives they led."

The question here suggests itself, Was the exposure to climatic agencies and irregularity of living greater in this than in the first expedition? Recollecting the obstacles which the first had to encounter and overcome, and which have been briefly alluded to at the commencement of these remarks, it is difficult to persuade ourselves that such was really the case—at all events, so far as climatic agency is concerned. The sufferings of the expedition up the Yang-tze-Kiang in 1841, as described so fully and clearly in the Report of Dr. Bryson for that year, surpass anything which was experienced by any of the expeditions of the more recent period—the two, for example, in the years 1858–9 to the Peiho. And, furthermore, it is worthy of remark that while in the first series of operations, the whole seaboard of China was successively visited, and almost every seaboard town hostilely dealt with, Canton and the Peiho rivers were the only scenes of active hostilities during the more recent period.

It must be admitted that the intercourse between our people and the shore appears to have been much greater lately than on the former occasion. The early capture and subsequent occupation of Canton necessitated an exposure of this kind, and the disproportionate mortality in the marine battalion and in the gun-boats on the Canton river, has been most justly ascribed by Dr. Bryson to this cause. But after all due allowance is made for this sinister agency; when, for instance, we deduct the sixty-three deaths which took place in the battalion and among the gun-boats, there still remains upwards of four hundred deaths to be accounted for in the squadron from dysentery alone: a number greatly in excess—indeed, more than double of what stands on record as the aggregate of victims of this disease during the first expedition.

To estimate correctly the difficulty by which this question is surrounded, it will be necessary to keep in view the fact, that the squadron employed recently in China was amply furnished with all the requisites which were calculated to protect the men from disease, or to mitigate its effects: such as wholesome provisions, in the shape of an ample supply of fresh meat and vegetables; hospital ships carefully fitted and well adapted for their special purposes; and medical comforts and necessaries in unstinted supply to the different ships. At no time were the crews exposed to the privation of unwholesome food, as was the case during the first expedition. The water, which in the opinion of certain medical officers is looked upon as a morbific medium of most pernicious power in bowel complaints, was often "served out," in many steam-vessels, fresh from the distilling apparatus. Indeed, nothing that prudent foresight could guard against was omitted to preserve the men in health, or to mitigate the severity of their complaints when attacked.

It is precisely these precautions and appliances which rob us of most of our stock *reasons why*, to account for the mortifying disparity which the comparison we have drawn has brought to light, and we are consequently compelled to search deeper, in order, if possible, to reach a solution of the problem.

Is the character of dysentery in China grown more virulent within the last twenty years? Has its grasp become more fatally tenacious than when we first made its unwelcome acquaintance in Chinese waters? If we reason from analogy, this assumption is scarcely probable, as experience has taught us that old and well-known diseases do not, under ordinary circumstances, increase in violence. Under exceptional conditions, no doubt, they often reappear in an aggravated form—when, for example, they pass from a sporadic to an epidemical activity. But in the case before us, there is nothing to countenance this inference, for the numbers affected were absolutely fewer, although the invaliding and fatality were much greater amongst them. But if the character of the disease remained much the same in 1858 as in 1841, and, as already shown, the predisposing causes were, in point of fact, fewer during the latter than the former period, is there any other disturbing influence to which this increment of mortality and invaliding may be ascribed?

The only obvious source of explanation which yet remains, is that furnished by the remedial agents which happened at the two separate periods to be most in vogue. We select this mode of expression purposely, for the history of medical therapeutics for the last twenty years exhibits as much change—it might even be said fickleness, as the contemporaneous and ever-varying whims of the fashionable world in matters of dress. That constant desire to substitute fresh and more efficient remedies for the old ones, and the notoriety which waits even upon any semblance of success, tends to crowd upon this domain of medical art a more than desirable amount of change. Dissatisfied with present results—for, after all, the remedial powers of even the most approved medicines are attended by a large margin of failures, this yearning for and search after more perfect agents is not only common but praiseworthy, and when kept within the limits of sound induction, indispensable to the progress of medicine as a science and an art. But in following out this necessary routine, there are unhappily in existence proofs more than sufficient, that change has often been mistaken for progress, and that old measures have been discarded to give place to novelties, which, it were well if they even did not fall much short of, instead of surpassing, the efficacy of those they have been summoned to supersede.

The natural bias which an innovator or discoverer has for the offspring of his own mental research, disables him, more especially in medical art, from submitting to the world a dispassionate and impartial record of its actual success. Nor, unfortunately, is this confined to himself, for in proportion to his mental capacity, and the enthusiasm with which he prosecutes his discovery, he gathers around him a school of eager admirers and supporters, who accept with unquestioning faith his facts, and not unfrequently themselves fashion facts to their favourite's peculiar views.

Accordingly, the statistics of medicine, when called in to give support to the particular notion or opinion of the writer who uses them, must always be accepted with caution, if not suspicion. Nothing is more easy, as unhappily nothing is more common, in cases of this kind, than for men, unconsciously perhaps, to embrace within their definition of a disease, with the successful treatment of which they wish to be identified, any symptoms, however mild, which at all resemble it; and having thus accumulated an imposing number of cases, to proceed to invest the particular remedy or remedial courses they made use of for its treatment, with the special credit of effecting a striking amount of cures, which, however, would have equally happened in all likelihood, had the cases been left entirely to nature. Thus, in one of our foreign hospitals, years ago, it was announced that yellow fever, by the resuscitation of an old remedy, had been stripped of its terrors, that the antidote to this bane of our intertropical settlements had been found to reside in a few drops of turpentine frequently administered—a practice introduced by Rush in 1790. And more recently still, in the Transactions of one of our medical societies, it was shown by the answers of different contributors to the questions propounded to them by the society as to their individual success in the treatment of diphtheria, that the average rate of cure ranged from 0 to 100 per cent. *Tantane incertudo in animis medicis!*

It has pungently, and not without truth, been said by a brilliant and successful statesman, that "statistics may be made to prove anything:" meaning, doubtless, where a preconceived notion had to be sustained. And this brings us back to the statistics which have hitherto been occupying us. In these there cannot be a suspicion of bolstering up anything. They are compiled from year to year with the same dispassionate temper as the tables of mortality, or the census returns. What general facts they may eliminate when several series of them are compared together is as little foreseen as premeditated. They take their character and dimensions from the nature of the isolated details furnished by individual workmen, each of whom knows not what his quota will tend either to establish or to destroy. When the time comes for putting together these details, and exhibiting in a generalized form their results, then valuable and unimpeachable facts come to light. We might guess, but could never accurately ascertain, but for such help, what climates are most unfavourable to European seamen, and what seasons are most to be dreaded; what hygienic precautions are found to be most serviceable; what diseases are most prevalent in certain localities; and although last, not least in importance, what remedial agents were attended by the largest measure of success. If confidence, then, can be placed in any series of medical statistics, it must be in such as these; and in truth they are worthy of perfect reliance so far as they go.

We have now to return to the question of the therapeutic measures employed during the respective periods of the two expeditions, and endeavour to learn whether the unfavourable comparison already indicated may depend upon the changes which have taken place in the remedies themselves, or in the mode of their administration. Here, possibly to avoid what might be called invidious comparison, the statistical reports are less full than, for the solution of the present question, could be desired; their main object being less to describe medical treatment than to record other facts. Enough, however, is incidentally given in the earlier reports to enable us to judge of the treatment most in vogue twenty years ago; and in order to convey accurately what it was, it will be best to quote:

In the second and third forms (meaning dysentery proper, as elsewhere described) the surgeon of the *Sumarang* says, "recourse was had to bloodletting in ten of the worst cases; and calomel and opium, varying in proportions of from ten to twenty of the former and two of the latter, were given morning and evening for two days afterwards. Small doses of castor oil, the daily use of enemata, or of suppositories when the introduction of the pipe could not be borne, were then employed, and finally astringents. In several instances the patients treated with large doses of calomel after they have been bled were speedily relieved; but an unpleasant ptyalism supervening, the remedy proved worse than the disease, and the convalescence was exceedingly protracted." The same surgeon, however, subsequently states, that "he placed as much confidence in this valuable medicine for the cure of dysentery, as he did in the sheet anchor" (for securing the ship).

Elsewhere, the surgeon of the *Agincourt* observes, "that acute cases of dysentery required very active treatment—namely, bleeding both local and general, and the administration of calomel every third hour until ptyalism took place. Astringents of various kinds, together with blisters, sinapisms, and opium suppositories were also used, but the greatest dependence was placed in giving calomel until it affected the system."

Again, the surgeon of the *Cornwallis* remarks, "that bleeding, both locally and generally, has been attended with the happiest results. When this failed, however, blistering over the abdomen was resorted to, and calomel and opium and ipecacuanha were given internally, together with cretaceous mixtures as adjuvants. When all these measures failed, there was little left to be done but to smooth the patient's downward course to the grave."

In the *Driver*, in which the cases of bowel complaints were both numerous and severe, the treatment had recourse to is thus recorded: "In cases attended with considerable fever and constitutional derangement, abdominal tenderness, and a full and frequent pulse, venesection was employed. If the tongue indicated a foul state of the stomach, an emetic was given. Scruple doses of calomel combined with opium were given during the earlier stages of the disease, and subsequently smaller doses until the system became affected. When there was much abdominal tenderness, leeches were applied over the seat of the pain, and the bleeding promoted by fomentation and cataplasms."

Enough has been quoted above to indicate the general mode of treating the graver forms of bowel complaints which occurred during the first expedition.

When we turn from this treatment to look at the aggregate results, it must be confessed that the sentiment inspired is not that of unqualified satisfaction; indeed, it is far more akin to positive disappointment. Out of 2102 cases of dysentery, 188 succumb in death, and 60 are invalided from the station.

In presence of so modified a success, it is not to be wondered at that a feeling of dissatisfaction should have pervaded the minds of medical men, and that some change of treatment should have been made. Accordingly, of late years, the heroic or active treatment by depletion, and the exhibition of large doses of mercury with and without opium, has fallen into desuetude, and reliance has been placed on milder measures. Ipecacuanha, in the manner recommended by the Indian practitioners, has been largely tried, together with hydr., cum creta, opium, the mineral acids, topical bleeding, counter-irritants, enemata of ipecacuanha; in short, all the older remedial agents, excepting, as already stated, the so-called heroic, as regards bleeding generally, and the administration of calomel pushed to ptyalism.

On referring to the records in which the amount of success which followed upon this method of treatment is accurately reported, we find it to be as follows:—For the three years, 1857–58–59, the aggregate number of cases treated was 2006. Here we see the actual number less by nearly one hundred than what occurred during the first expedition, notwithstanding the men employed in the later proceedings exceeded those in the former by 9500. So far, this is a favourable circumstance, as it shows that the disease in question was not so prevalent as formerly, and that the hygienic measures adopted have acted most beneficially as a preventive. But when the inquiry is extended further, and the fate of those who unfortunately were attacked is traced, we find that of these 2006 no fewer than 606 were invalided, and 465 died; a result, compared with the preceding one, in every way disastrous. By presenting the products respectively in the form of figures, the mind will seize at a glance the painful disparity—

Years.	Number of men employed.	Cases of dysentery.	Number invalided.	Number dead.
1840–1–2	15,470	2102	60	188
1857–8–9	24,980	2006	606	465

Thus, in rough numbers, in the first Chinese war, one out of every eight and a half of the men attacked succumbed or was disabled from service; in the second Chinese war, one out of every two.

It may be urged by those anxious, and very pardonably so, to explain away this disheartening comparison, that probably many of the cases registered in the journals of the first expedition as dysentery, belonged in reality to the milder form of bowel complaints. But it cannot, in fairness, be supposed that the older class of medical officers were less able to draw a proper distinction between the two forms of the disease under notice than their successors. Indeed, the quotations made in the official returns, some of which have been already cited, leave no room for doubt what they understood by the term dysentery, how fully alive they were to its formidable character, and how anxiously they endeavoured to combat it with all the appliances of their art.

Taking a dispassionate and impartial view of the facts as they have just been disclosed, the conclusion to which they lead cannot be avoided, —namely, that the recent method of treatment of dysentery in China, so far from having become more successful from the additional experience which twenty years confer, has, on the contrary, very decidedly retroceded. In presence of this painful but inevitable inference, it becomes a subject for the serious consideration of those who are called upon, in the regions of the far East, to neutralize by medical means this dread scourge of Europeans, to decide whether the much-contemned practice of twenty years ago has not been superseded somewhat rashly, in consequence of its modified measure of success, for another which, conforming to the quietism of the day, has brought about a series of results that in these times has no parallel.

The only remaining aspect of the two expeditions to China which yet claims special notice before bringing these remarks to a close, concerns the casualties which took place while our officers and men were engaged in actual encounters with the enemy. By those not versed in the inner history of war, wherever carried on, this phase has always been regarded with peculiar sentiments of horror and sadness. Carried away by their excited imaginations, they picture to themselves the scenes of the battle-field and deck, and suppose that the gloomiest and most destructive proofs of the havoc of war are to be found there. And, undoubtedly, the concentration of human misery there displayed in the small heaps of dead strewn about, and in the mangled forms of those still living, appeals with awful force not only to the actual witnesses, but to those who simply listen to or read the harrowing description. A profound sentiment of horror and pity fills their breasts, and leads them to conclude that in the shock of actual conflict the heaviest disasters in the shape of sacrifice and suffering take place. But such, alas! is not the case. To ascertain fully what war entails, we must quit the battle-field and deck, and pass into the wards of the military hospitals and hospital-ships. There, in the subdued atmosphere of suffering, in the pale faces and wasted forms, lately admired for their manly vigour and comeliness, in the racked bodies, not only of the wounded, but of the fever-stricken—of the victims of what has been aptly termed the diseases of camp life, are to be seen, in their fullest and most appalling extent, the ravages of war. Compared with those who silently suffer, and who, almost unnoted, pass through this portal on their way to the grave, the numbers of those who die or who meet the cause of their death in actual strife are comparatively few. From the time of the Crusades and long before, down to the unnatural and gigantic struggle now enacting on the shores of the New World, this fact proclaims itself with no uncertain voice—that the bullet and bayonet, and their prototypes, the sword and spear, are but the inferior, the secondary agents of that fell instinct which impels men to compass each other's destruction. Although the contentions we have been engaged in with the Chinese are not to be compared for one moment in point of magnitude with many of our former wars, still the lesson we draw from them is the same, and bids us look to our hospitals, and not to the field of actual strife, for the true extent of that suffering and death which war never fails to carry in its train.

In the subjoined table, the illustration of what has just been urged will be made sufficiently evident :

Years.	Mean strength.	Cases of wounds and accidents.	Ratio per 1000.	Inva-lided.	Ratio per 1000.	Dead.	Ratio per 1000.
1840	3,330	701	210·5	8	2·4	8	2·4
1841	4,840	834	172·3	13	2·7	13	2·7
1842	7,300	1313	179·9	12	1·6	22	3·
1857	7,080	1495	210·7	36	5·1	51	7·2
1858	11,300	2496	220·8	59	5·2	52	4·6
1859	6,600	1756	266·1	131	19·8	97	14·6

In this last table it will be noticed, as in those which have preceded, that the figures of the more recent period continue to maintain their bad pre-eminence in point of magnitude, not only absolutely, but in a relative sense. The ratio, whether of actual loss or of those only disabled, is decidedly larger. But it must be explained here, that the numbers above exhibited represent casualties of every kind, including accidents, which with others, in the class of wounds, have no direct relation with hostile operations, but from which it is not now possible to separate them. It may, however, be taken for granted that a very large proportion of these casualties originated in the hostilities which prevailed at the time of their occurrence, and more especially of those which ended in death or removal from the station. It is worthy of note that this increase of the recent epoch indicates the presence of a more obstinate and skilful mode of defence on the part of the Chinese of late years. No doubt their freer and more extended intercourse with foreigners of the West since 1840 has taught them methods of making war which were known to them very imperfectly, if at all, before. Indeed, in each of the three defences they made of the forts at the mouth of the Peiho—the second with so much loss and disaster to us—they displayed a skill and obstinacy which went far to prove, if not the actual presence amongst them of Western heads and arms, at all events no contemptible amount of Western pluck and tactics.

Before bringing these remarks to a final close, it may not be unacceptable for the reader to have placed before him a grand summary, such as is here subjoined :

Year.	Mean strength employed.	No. of all cases of disease, &c.	Ratio per 1000.	No. inva-lided.	Ratio per 1000.	No. dead.	Ratio per 1000.
1840	3,330	5,871	1763·1	60	18	111	33·3
1841	4,840	8,816	1821·5	159	32·8	290	59·9
1842	7,300	16,375	2243·2	170	23·3	376	51·5
1857	7,080	17,595	2485·2	378	53·4	328	46·2
1858	11,300	29,990	2653·9	915	80·9	706	62·5
1859	6,600	15,756	2387·9	555	84·1	322	48·8

The heavy balance which is here shown against the total results of the second series of operations in China will not, after what has gone before, come upon the reader with surprise. The preceding details will fully have prepared him for this adverse balance-sheet. Amongst the different feelings it is calculated to provoke, those of pity and sympathy will doubtless be most prominent in the minds of the philanthropic at this summary of their countrymen's sufferings and sacrifices. To the medical inquirer alone there will be least cause for gratification; for although he may be able to trace from the effects of the hygienic measures pursued a decrease in the amount of certain forms of intertropical disease, he will nevertheless be constrained to admit that in the therapeutic phase no progress, to say the least, has been made. The lesson conveyed, however, if not a grateful one, will not be unprofitable if it leads to further investigation, and to the reconsideration of those remedies which have perhaps been too hastily condemned. The statesman and the economist will, however, find cause for pride and gratification in the fact that with a naval force so comparatively small, and yet the chief instrument in the drama, and at a sacrifice, although distressing, still so confined in its incidence, a vast empire, which had been sealed to foreigners since the dawn of civilization, was at length thrown open, not only to Englishmen, but to all the world; and the industry of three hundred and fifty millions of *workers* made available for the general benefit of mankind.

ART. IV.

Historical Researches on the Use of Forceps as a Means of Fixing the Eye in Extraction of Cataract. By THOMAS WINDSOR, Surgeon to the Manchester Eye Hospital, &c.

THE question of priority in any medical discovery appears at first sight to present few attractions, and its solution to be of equally little value. It is generally supposed, that the knowledge of the fact discovered is in itself amply sufficient; further consideration will, however, show that researches on such points are of real utility, and that they may be made interesting. For, on the one hand, it is on them that the history of medicine depends—the first necessity of any history being truth, history without truth being fiction; on the other hand, they are of service in showing the amount of valuable, and too often forgotten, information stored in the literature of former periods. They who so fondly call themselves practical men, and who with rare exceptions form the charlatans or dunces of the profession, may deride such knowledge—book-knowledge, as they term it; but the many eminent men of the present day who have combined profound learning with great practical skill, furnish us with a victorious reply.

We shall have occasion to show that the idea of employing forceps to steady the eye in extraction, was derived from their use in operations for strabismus and artificial pupil; we shall, however, for the sake of conciseness, limit ourselves as much as possible to the consideration of the former operation.*

Mr. France was the first in England to publish an account† of this method of fixing the eye during the first incision of the cornea. He says‡—"In operating for the formation of artificial pupil I first became aware of the practicability of holding the eye perfectly still and motionless, or as nearly so as possible, by the mere application of artery forceps. The idea at length was suggested of extending the use of this instrument to another operation, in which, as far as I know, it had never been employed (at least in this country) before; of availing myself, in short, of the same resource as in cases of artificial pupil (and with a similar object) in cases of extraction." How the idea was suggested may be learnt from another passage.§ "It is right that I should acknowledge myself indebted to the work of Desmarres, already referred to, for the *idea* of adopting artery forceps as an 'ophthalmostat' in extraction, but not for their advocacy." Hence it appears that Mr. France derived the idea from Desmarres, but that he believed himself to have first advocated the use of forceps. In another paper, "On the Use of Forceps in Extraction of Cataract,‖ Mr. France says, "I am desirous of drawing attention to a mode of procedure introduced to the notice of the profession last October. . . . . I use the forceps in keratonyxis as well as extraction; it is pretty generally employed in London in cases of artificial pupil; sometimes in removing extraneous bodies from the eye; and, in short, on every occasion (extraction hitherto excepted) when it is desirable to fix the globe."

Mr. W. J. Square says:¶ "It is now common to seize the conjunctiva and its subcellular tissue with toothed forceps, in order to steady the globe in operations for strabismus, artificial pupil, &c.; but this plan, so far as I am aware, was not extended to the extraction of cataract until advocated by Mr. France;" and Mr. A. Poland writes:** "These cases certainly confirm the opinions and experience of Mr. France in a very marked manner. We can, therefore, have no hesitation in pronouncing his method a most valuable addition in the operation of extraction."

These extracts†† sufficiently prove that this method was unknown to the majority of ophthalmic surgeons in this country till Mr. France

* And more exactly of the use of forceps during the first stage of the operation, whilst the surgeon is making the flap-incision of the cornea. Mackenzie employed the forceps in some rare cases at a later stage of the operation to draw down the globe from under the upper lid. (French edition of Warlomont and Testelin, vol. ii. p. 438. Paris, 1857.)

† On Ophthalmostasis, with an Account of an improved method in Extraction of the Cataract, by J. F. France (Guy's Hosp. Reports, third series, vol. iv. p. 81. 1858).

‡ Loc. cit., p. 92. § Loc. cit., p. 100.

‖ Ophthalmic Hospital Reports, vol. ii. pp. 20–24. 1859.

¶ The Address in Ophthalmic Surgery (British Medical Journal, p. 733. 1860).

** On the Use of Forceps in Extraction of Cataract; France's Method (Ophthalmic Hospital Reports, vol. iii. p. 269. 1861).

†† We may mention, also, that Mr. Walton (Treatise on the Surgical Diseases of

published his account. Those, however, who had been pupils of Professor Graefe, must have been acquainted with it; and we have no doubt that, even if Mr. France had never written his paper, it would soon have come into use. That it had been employed in Berlin several years before the publication of Mr. France's paper, is clear from the account given by Dr. F. Ravoth of Graefe's method of performing cataract operations:*—"Some recommend . . . . that an irritable eye should be previously touched, and thus accustomed to the irritation of the instrument. It is, however, more advisable to always make use of an ophthalmostat, and thus fix the eye . . . . It is best fixed by seizing a fold of conjunctiva with delicate spring-forceps."

At a still earlier period we find that Desmarres† described and figured the use of forceps in extraction; as he had, however, invented an instrument of his own, it is not very surprising that he recommended it in preference. C. G. T. Ruete‡ has also noticed the use of forceps for fixing the eye in cataract operations, and copied the figure of Desmarres.

In the sixth volume of the 'Annales d'Oculistique'§ there is an essay on a new method of performing extraction by J. E. Pétrequin, of Lyon; in it we find the use of the forceps strongly urged, and from it we learn that both he and Bonnet had formed the idea of employing some of the instruments used in operations for strabismus in those for cataract; that he had first applied them to depression, and Bonnet to extraction; he gives the notes of two cases of extraction, in which the forceps were used to steady the eye; and, finally, he mentions that he had also found this method very useful in cases of congenital cataract, in cataract with nystagmus, and in the removal of foreign bodies.

In 1841 a work was published by A. Bonnet|| in which we meet with a chapter, "De l'Extraction de la Cataracte modifiée par l'adoption de divers moyens employés dans l'Opération du Strabisme." After describing the difficulties found in making the first incision, he says: "Toutes ces difficultés peuvent être évitées en fixant l'œil comme on le fait dans l'opération du strabisme. La cornée restant alors toujours dirigée en avant et ne fuyant jamais devant le keratotome, la section d'une moitié de sa circonférence peut être faite avec une précision et une facilité qui dépassent tout ce que l'on pourrait présumer à cet égard. . . . . Je ne crois pas qu'il y ait de l'exagération à dire que l'opération de la saignée n'est pas plus facile que l'extraction de la cataracte exécutée de la sorte."

the Eye, second edition, p. 540, London, 1861) and Mr. Nunneley (Lancet, vol. ii. p. 7, 1862), in referring to this subject, only mention Mr. France's name.

* Schlemm's Operations—Uebungen am Cadaver von Dr. F. Ravoth, dritte Aufl., S. 219, 240, 249. Berlin, 1854.

† L. A. Desmarres: Traité des Maladies des Yeux, pp. 602, 608. Paris, 1847.

‡ Lehrbuch der Ophthalmologie, Band ii. S. 694, and Fig. 137, zweite Auflage. Braunschweig, 1854.

§ Vol. vi. No. 5: we quote from the reprint in the Encyclographie des Sciences Médicales, quatrième série, tom. ix. p. 365. Bruxelles, Février, 1842.

|| Traité des Sections Tendineuses et Musculaires, p. 313, and plate 9. Paris, 1841.

Finally, I must mention that J. C. Jüngken* says, that Ger. ten Haaf employed in extraction little forceps (Zangen) for fixing the eye,—a statement which we believe to be incorrect, for although we have been unable to meet with the original work, we find that Haller† says in reference to this matter—"Tenacula litteram Y referens pro firmando oculo, neque enim probat hamum per conjunctivam trajectum;" and D. van Gesscher‡ describes the instrument as a double blunt hook—"Ten Haaf doet zulks (bepaling van het Oog) door een dubbelen, stompen haak." His instrument was therefore a kind of blunt fork, and in no respect resembled forceps.

Our conclusions would thus be that—

1. In 1841, Bonnet first formed the idea of employing forceps to fix the eye in extraction; that he applied it in practice, and that his example was followed by Pétrequin.

2. Professor Graefe introduced this method in Germany.

3. That Mr. France was the first to write on this subject in England; that although he was not the first to employ this method, he was the first to diffuse a knowledge of it in this country.

Art. V.

On the Existence of a System of Anastomosing Arteries between and connecting the Visceral and Parietal Branches of the Abdominal Aorta. By William Turner, M.B. (Lond.), F.R.C.S.E., Senior Demonstrator of Anatomy, University of Edinburgh.

The question of the existence or non-existence of inosculations between the branches of the abdominal aorta which supply the viscera of the abdomen and the branches which are distributed to the parietes of that cavity, has from time to time excited the attention not only of the anatomist, but of the practitioner of medicine. In several of our standard anatomical works it has, indeed, been recognised that some of the abdominal visceral arteries—e.g., the renal, supra-renal, and spermatic, give off, previous to entering their respective viscera, small branches to the fat and areolar tissue, with which their organs are more or less completely surrounded.§

Of the anatomical writers in this country, Harrison appears to have been the one who has most distinctly announced this to be the case. Not only does he state (p. 59) that the renal artery, before entering the kidney, gives off small branches to the surrounding cellular membrane and muscles, but he even goes further, and on page 67 says that the middle sacral artery sends branches to the rectum, and on

* Die Lehre von den Augenoperationen, S. 688. Berlin, 1829. The reference he gives is to the Korte Verhand. nopende de nieuwe wyze van de cataracte te genezen. Rotterdam, 1761.

† Bib. Chir., vol. ii. p. 379.

‡ Hedendaagsche Oeffenende Heelkunde, Derde Deel, Bl. 186. Amsterdam, 1786.

§ Harrison: Surgical Anatomy of the Arteries. Cruveilhier: English translation, p. 681. Quain and Sharpey: ed. 1848, p. 561.

page 37 he points out that the hepatic artery communicates with the phrenic, intercostal, lumbar, epigastric, and internal mammary arteries.

Of late years, however, some writers have exhibited a tendency to ignore the existence of any such anastomoses between the visceral and parietal branches of the abdominal aorta, and to speak of them as altogether distinct systems; as communicating with each other only through the common trunk from which they both spring.*

This assumption of the absence of any direct communication between these two sets of vessels has been employed by some physicians as a powerful argument against the utility of local bloodletting in the treatment of inflammations of the abdominal viscera.†

For some time back I had been led to believe, from many observations made in the dissecting-room, that the visceral and parietal branches of the abdominal aorta communicated much more freely with each other than is commonly supposed and taught by anatomists. With the object, however, of testing my observations, and of coming to more precise conclusions on this subject than was possible by an ordinary dissecting-room examination, I made during the summer of last year and the present one a series of special injections, and have succeeded in demonstrating the existence of a very complete series of anastomosing arteries between the visceral and parietal branches of the abdominal aorta. In the course of this examination I have employed the bodies of an adult male, an adult female, and those of two children. The method I have pursued has been to open the abdomen along the middle line, and then with as little disturbance of the parts as possible to insert an injecting pipe into a visceral artery, and then inject viscera-wards. The course taken by the fluid employed and the parts to which it proceeded, were then carefully noted. The injection used was a stiff gelatine, sometimes coloured red, sometimes yellow. The latter colour was, however, preferred.

The arteries which were especially selected for observation were the two renal and two spermatic arteries, the colic branches of the inferior mesenteric, the ileo-colic and right colic branches of the superior mesenteric and the hepatic. To record in detail the results of each injection would probably be too tedious, and not, indeed, so satisfactory as a general summary of the results obtained, although, in one or two cases, it may perhaps be advisable to speak more particularly of the course which the injection took and the vessels which were filled by it.

By these injections I have succeeded in demonstrating the existence behind the peritoneum of a well-marked vascular plexus, to which I propose to give the name of sub-peritoneal arterial plexus. This plexus lies behind the peritoneum, not only in the lumbar and iliac regions, but extends upwards as far as the diaphragm, and downwards into the cavity of the pelvis. It consists of fine elongated arteries, which freely anastomose with each other, so as to form a wide-meshed and somewhat

* See especially a paper by Dr. Struthers in the Edinburgh Monthly Journal of Medical Science, April, 1853.

† Dr. Bennett: Clinical Lectures, 1859, p. 280.

irregular network. It communicates on the one hand with the arteries of the abdominal viscera, and on the other with the arteries of the different parts of the abdominal wall. Through it the visceral and parietal arteries are brought into anatomical communication with each other, so that the vessels in the wall can be injected from many of the visceral branches. The anastomoses, through the intervention of this plexus, are especially well-marked in, but not exclusively confined to, those vessels which supply viscera situated altogether behind, or only partially invested by the peritoneum.

The plexus communicated freely with the renal and supra-renal arteries, with the arteries of the pancreas, with those of the duodenum, cæcum, ascending and descending portions of the colon. It extended forwards also between the layers of the transverse meso-colon and anastomosed with the branches of the middle colic artery. It passed downwards between the folds of the mesentery, reaching as far as the coils of the small intestine, and communicated with the branches of the superior mesenteric artery, which supply that portion of the alimentary canal. It could also be traced upwards between the folds of the gastro-splenic omentum to the hilum of the spleen, where it anastomosed with the splenic artery. In the hepatic region also, it was seen passing between the folds of the falciform or suspensory ligament of the liver, as well as, and even more strongly marked, along the line of reflection of the coronary and lateral ligaments. In these localities it could be readily injected from the hepatic artery, and through it that vessel inosculated with the diaphragmatic arteries.

In the cavity of the pelvis also, the plexus was situated between the layers of the meso-rectum and anastomosed with the hæmorrhoïdal arteries. It extended in the female between the folds of the broad ligament to the uterus, ovaries, and Fallopian tubes, and it communicated also with the vesical arteries.

But this plexus does not appear to be confined in its office to that of an inosculating medium between the visceral and parietal branches of the abdominal aorta. The arteries of which it is composed evidently perform the function of nutrient arteries for many of the structures at the back of the abdomen, more especially the sub-peritoneal fat and areolar connective-tissue, which exist in such abundance in this locality. In addition, branches could be seen passing from it to the chains of lymphatic glands, which lie along the track of the great bloodvessels, and even to the coats of the great arteries and veins themselves, with the vasa vasorum of which they appeared to inosculate.

Since my attention has been directed to this plexus through the various injections I have made, I have more than once in the ordinary post-mortem examination of bodies, where the small abdominal vessels have been more than usually engorged with blood, traced its principal ramifications without the aid of any other than this their natural injecting material.

Such being a general description of the arrangement and connexions of the sub-peritoneal arterial plexus, it may be well, perhaps, in the next place to refer somewhat more minutely to its disposition in certain special localities. If a pipe be put into one or other of the renal arteries, and the injection forced into the organ, it will be found that not only is the kidney injected, but that the fluid employed finds its way into that portion of the sub-peritoneal arterial plexus which is situated more immediately in the fatty capsule in which the kidney is embedded. If the pressure be kept up, the injection gradually extends itself into the parts of the plexus situated at a greater distance, until in the course of time the whole is filled. If the injection has been made on the right side, many fine arteries may be easily traced from this plexus to the lower end of the ileum, cæcum, and ascending colon, with the arteries of which they communicate. If on the left side, to the descending and sigmoïd parts of the colon, the vessels of which they join. If now the abdominal wall be cut into, it will be seen that the parietal arteries are more or less perfectly filled, the lumbar, ileo-lumbar, circumflex ilii, lower intercostal and epigastric arteries all containing injection, which has evidently passed into them, not by the way of the trunks from which they arise, but through the inosculations between those of their branches which supply the inner aspect of the abdominal wall and the sub-peritoneal arterial plexus. By removing on one or other side the section of the colon lying anterior to the kidney, and by carefully dissecting down through the fatty capsule, the course which the injection has taken in its passage from the renal artery to the sub-peritoneal arterial plexus may be seen.

Various anatomists have taught that the renal artery gives off before entering the kidney small branches which pass to the fatty capsule surrounding the organ, and undoubtedly these are, in part, the channels along which the injection flows. But they are not the only, and apparently not the most important ones; for in all the cases examined I have traced small arteries passing out of the substance of the kidney, piercing its fibrous coat, and joining the arterial plexus in the fatty capsule. Occasionally these arteries pierced the fibrous coat of the kidney at various points on its surface without following any definite plan, but at other times the arrangement was of a more regular nature. In one case three well-marked arteries pierced the fibrous coat on the posterior surface of the organ—one about the centre of this surface, the others close to its extremities. They all passed outwards into the surrounding fat and areolar tissue. Those from the extremities inclined towards each other, joined, and formed a well-marked arterial arch, which again communicated with the artery which pierced the middle of the posterior surface of the gland. From this arch branches passed in various directions, so as to connect this with the adjacent parts of the sub-peritoneal arterial plexus.

In another case, not only were arteries found piercing the fibrous coat on the surfaces of the kidney, but, in addition, one very well-marked artery came through about the centre of the posterior convex margin. It immediately divided into several branches, some of which passed towards the surfaces, others towards the extremities of the organ, to anastomose with the arteries which had pierced the fibrous coat in those localities.

These perforating vessels, which arise from the renal artery whilst in the substance of the kidney, and which then pierce its fibrous coat, would appear not only to serve the highly important office of connecting the renal trunk with the arteries of the abdominal parietes, but, in addition, they doubtless fulfil a functional duty of some moment. Physiologists have been in the habit of considering that the entire force of the current of blood flowing along the renal artery, is expended in the propulsion of the blood through the complex vascular arrangements within the substance of the kidney itself. But the existence of these arteries perforating the fibrous coat, shows that such is not altogether the case. For some portion, small though it may be, of this force, is necessarily employed in propelling the blood along them into the surrounding sub-peritoneal arterial plexus. It is possible that this subsidiary outlet for the blood from an organ possessing so tense a fibrous coat, and so complicated an internal vascular structure as the kidney, may, at times, when, through engorgement of the venous system, the exit of the blood along its principal canal, the renal vein, is impeded, serve as a sort of safety-valve, and diminish the tendency to extravasation and effusion into its substance.

I have already stated, that through the vascular arrangements just described an injection may be forced from the renal arteries into those portions of the large intestine which lie in front of the kidneys. Other experiments which I have made show that an injection can travel in exactly the opposite direction from the intestine to the kidney. Thus I have placed a pipe in the ileo-colic artery, and filled the arteries of the lower end of the ileum, cæcum, and ascending colon, the injection has then passed into the sub-peritoneal arterial plexus lying behind the gut, with which these intestinal vessels communicate, and from this into the arteries which perforate the fibrous coat of the right kidney. Exactly corresponding results were obtained on the left side by injecting the left colic branch of the inferior mesenteric artery. In one case, in which I injected the renal artery yellow, and the colic branch on the same side red, I found the sub-peritoneal arterial plexus, between the kidney and intestine, partly containing the yellow and partly the red injection. In addition, in these cases, the injection had also passed into the vessels of the abdominal walls.

Very interesting and instructive results have also been obtained by injecting the spermatic arteries. Not only did the injection pass along these vessels into the substance of the testicles, but it was found that each artery, as it coursed down the lumbar and iliac regions, gave off numerous small branches, which communicated with the sub-peritoneal arterial plexus in the immediate vicinity of the spermatic trunks, and into which the injection proceeded. Further down, small branches were seen passing from each spermatic along the vas deferens into the pelvis as far as the bladder. Each artery also gave off, whilst in the inguinal canal, small vessels which communicated with the epigastric artery. In its course down the cord, the spermatic artery gave origin to many small twigs which proceeded to the coverings of the cord, and lower down in the scrotum branches proceeded from it to the parietal layer of the tunica vaginalis, in which they formed a very well-marked arterial network. These last-named branches assisted in the supply of blood to the various scrotal textures, and inosculated with the perineal and other cutaneous arteries sent to the region of the scrotum.

From the injections, the general results of which I have recorded above, the following conclusions may be drawn—

1st. That in the sub-peritoneal fat and areolar tissue a system of anastomosing arteries exists of greater extent and importance than has been hitherto generally recognised.

2nd. That there is a much greater amount of communication between the different branches of the abdominal aorta than is commonly supposed.

3rd. That not only is it possible to inject the arteries of the abdominal wall from those of the viscera, but that to some extent the bloodvessels of one viscus may be injected from those of another, and this not through the main trunk from which they both proceed, but through their mutual communications with an intermediate set of anastomosing arteries.

4th. Into the question of the efficacy or inefficacy of local bloodlettings in inflammations of the abdominal viscera it is not my intention to enter; but this may with certainty be said, that so much of the argument against its efficiency, as is based upon a supposed want of communication between the bloodvessels of the viscera and those of the wall, rests upon an assumption, and is not supported by careful experiment and observation.

PART FOURTH.

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY).

HALF-YEARLY REPORT ON PHYSIOLOGY.

By Edmund Montgomery, M.D., M.R.C.P.
Demonstrator of Morbid Anatomy, and Curator of the Museum, at St. Thomas's Hospital.

I. General Physiology.

1. John Hughes Bennett, M.D.: *Lectures on Molecular Physiology, Pathology, and Therapeutics, and their Application to the Treatment of Disease.* (Lancet, Jan. 3rd and 17th; Feb. 7th and 21st, &c., 1863.)
2. Bruecke, E.: *The Elementary Organisms.* (Moleschott's Untersuchungen zur Naturlehre, vol. viii. part 5, 1862.)
3. Bucquoy, E.: *The Effects of Compressed Air on the Animal Economy.* (Strasburg, 1861.)

1. To bring the phenomena of organic and inorganic nature under the same laws is a task which has often been attempted. Dr. Bennett's 'Lectures' contain another endeavour in that direction. He applies, namely, to organic nature the same old theory which has afforded such great convenience in the explanation of all physical phenomena. By simply substituting for the imaginary atoms of the physicists those last particles of matter just detectable with the microscope, by further transferring the supposed properties of the former to the actual presence of the latter, he prepares a material for the construction of his organic world. The molecules or granules having been thus endowed with all necessary powers, are merely pulled together and aggregated in the most natural manner, so as to form any desirable shape. By slight variations of this same process of molecular coalescence all the different elements of the tissues result, which, in their further arrangement, compose the whole organic frame.

2. Bruecke, on the other hand, believes that organization—that is to say, complicated structural mechanism—exists far beyond the limits which we are accustomed to draw for it. According to him, the so-called elements of the tissues are all highly organized beings, their different kinds varying from each other in the most decided manner. He calls them *elementary organisms*, but maintains, at the same time, that the term *elementary* is only provisional, in expectation of more penetrating knowledge; that with higher discerning powers these elementary organisms would be found to consist of further organized component parts. He does not see how, for instance, the motion which is exhibited by the ciliated epithelium can be otherwise accounted for, and thinks that from analogy we must infer that, as in the animal organism the different actions proceed from different organs, so also in the minute beings which are the elements of the larger ones, does each vital action depend upon special organs.

3. During the building of the new bridge at Kehl, the labourers had to work in compressed air. Bucquoy made observations of the effects of that air upon himself and others. He found that, in getting into the reservoir, the respiration lost its regularity. Restlessness was soon felt, and pain in the ears set in. It was as if a foreign body was driven with force into the external meatus. This pain was so intense that some people could not help crying. After a while it went off, then came back again, till it at last entirely ceased. The breathing grew also quiet; the inspiration was shorter than usual; the expiration longer; the pulse was more frequent than normal; hearing was impaired, and it felt as if an expanding body was lodged in the interior of the ear. The voice sounded as if it came through the nose, and it required an effort to speak out. At $2\frac{1}{2}$ atmospheric pressure it was impossible to whistle, and there was much perspiration. On returning into the open air, the breath formed a cloud; there was a sharp feeling of cold; the pain in the ears returned; there was palpitation of the heart; and the respiration got again irregular. On analyzing the air, it was found to contain 2·37 per cent. of carbonic acid; the remainder consisted of 19·23 per cent. of oxygen, and 80·77 per cent. of nitrogen. Those who had to work for a long time in the compressed air became emaciated. Many labourers lost their appetite, and looked as if they were just recovering from a severe illness. Muscular and rheumatic pains often occurred, and sometimes the effects of congestion of the lungs and of the brain were observed. The blood which was taken from veins presented, in some instances, a bright red colour; and this was especially the case when the person had remained long in the condensed atmosphere. The movements of the limbs appeared to be more easy than in the normal air.

II. Food and Digestion.

1. Savory, W.: *Experiments on Food; its Destination and Uses.* (Lancet, April 4th and 11th, 1863.)
2. Ravitsch, J.: *On the Influence of the Pneumogastric Nerves upon the Movements of the Stomach.* (Archiv für Anat. Phys., &c., 1861, p. 770.)
3. Meissner, G., and Buettner: *Researches on the Digestion of Albuminous Substances.* (Zeitsch. für rat. Med., 3rd Series, vol. xii. pp. 46–67, 1861.)

1. Savory made experiments upon the effects of nitrogenous, non-nitrogenous, and mixed diet. A hawk and several rats were fed with lean veal, which was found by Mr. Atsfield to contain only 1·55 per cent. of fat. The experiment was continued for a considerable period, during which the animals appeared to enjoy perfect health and strength. Though there was an absence of all active exercise, they maintained their original temperature, which shows that nitrogenous substances are not only somewhat calorifacient, but sufficiently so to produce alone the requisite temperature. Mr. Savory thinks it also highly probable that nitrogenous materials may prove directly calorifacient without previously forming tissue. The rats which were fed with non-nitrogenous food became extremely emaciated, and several of them died during the experiment. They were placed in an atmosphere of 65°, in which they maintained pretty nearly their original temperature. This seems to render it certain that non-nitrogenous substances are directly calorifacient without entering into the composition of tissue; that they are directly burnt in the blood. The nitrogen which was excreted in the urine in these cases, and even more than that amount, can be accounted for by the disintegration of the original tissues. The loss of tissue, and not the loss of temperature, must be looked upon as the great cause of death. When mixed diet was given, the animals maintained their normal weight and temperature. The rats which were kept on nitro-

genous diet passed a very large quantity of urine; in one instance, between four and five times as much as rats on mixed diet, and more than thirty times as much as rats on non-nitrogenous diet; and the urine yielded, moreover, a still larger proportional excess of nitrogen.

2. Ravitsch tied a ligature round the upper part of the œsophagus of various animals after they had filled their stomachs with food, and then divided the pneumogastric nerves in the neck. After the operation, the movements of the stomach ceased, but the secretion of gastric juice and digestion continued. The stomach remained full, and only that part of the food was digested which was in contact with its wall. The contents of the stomach of animals which had died from starvation, or which had undergone the section of the pneumogastric nerves, did not yield any peptone, even when, by artificial means, digestion was kept up; the peptones had been absorbed.

3. Meissner and Buettner procured pure fibrine from the blood of oxen and pigs. If, of two equal portions of it, the one was digested at a temperature of 40° C. with a quantity of hydrochloric acid amounting to 0·2 per cent., and the other portion was treated in the same manner, but with the addition of a few cubic centimetres of a concentrated solution of pepsine, it was found that, after twelve hours, the fluid which contained hydrochloric acid only had scarcely dissolved a particle of fibrine; whilst the other, which contained pepsine as well, had dissolved the entire quantity. The solution of fibrine thus obtained is of a brown colour, and a jelly-like substance is seen suspended in it, which, on being treated with alcohol and ether, changes into a friable powder; this powder exhibits the same properties as the dyspeptone of caseine. After exactly neutralizing the brown filtered fluid when it has cooled, a yellowish flaky precipitate is formed, which is the parapeptone of the fibrine. The neutral colourless fluid which has passed through the filter forms, on the addition of about 0·04 per cent. of a concentrated acid, a white precipitate, the metapeptone of fibrine. The remaining fluid holds pure peptone in solution.

III. Respiration and Circulation.

1. Traube, M.: *On the Physiology of Respiration.* (Med. Cent. Ztg. xxxi. 38, 39, 1862.)
2. Traube, M: *On the Relation of Respiration to Muscular Activity, and on the Signification of Respiration in General.* (Virchow's Archiv, vol. xxi. 1861, pp. 386–414.)
3. Edenhuizen: *Contributions to the Physiology of the Skin.* (Nachrichten der G. A. Universitat zu Göttingen, p. 288, 1861.)
4. Spring, W.: *On the Movements of the Heart, and especially on the Mechanism of the Auriculo-ventricular Valves.* (Annal. de la Soc. Med.-Chir. de Bruges, 1861, pp. 40–43.)
5. Beau: *On the Action of the Heart.* (Gazette Med. 1861, No. 27.)
6. Sucquet: *On the Circulation of the Blood in the Limbs and in the Head of the Human Subject.* (Bull. de l'Acad. xxvi. pp. 825–853.)
7. Schweigger-Leidel: *On the Passage of Solid Substances from the Blood into the Lymphatic Vessels.* (Studien des Physiol. Inst. zu Breslau, 1861, p. 57.)
8. Teichmann, L.: *The Lymphatic System from an Anatomical Point of View.* Leipzig, 1861.)

1. Traube put animals under the influence of narcotics, and made them inhale various gases by means of artificial respiration. He arrived at the following conclusions: 1. The phenomena of dyspnœa in mammalia are not due to a decrease in the amount of oxygen taken into the system, but are caused by a

decrease in the elimination of carbonic acid, which is constantly forming within the system. 2. The agent which incites in- and ex-piration, by stimulating the nervous centre of respiration contained in the medulla oblongata, is carbonic acid. 3. The phenomena of dyspnœa produced by the accumulation of carbonic acid within the body are the more energetic the larger the quantity of oxygen which is contained in the blood at the same time. 4. The carbonic acid acts as a stimulant on a portion of the terminations of the pneumogastric nerves in the lungs, and thereby promotes inspiration.

2. Traube distinguishes three important functions of respiration: 1. *The formation of cells.* This is the most general function of respiration, for all organized beings, vegetables as well as animals, require oxygen for the building up of their cellular structure. This function is manifested in the purest form in the vegetable kingdom, in which oxygen is required only for that purpose. 2. *Muscular activity.* This respiratory function belongs to all animals, warm-blooded as well as cold-blooded; and in the latter it constitutes the chief function of respiration. 3. *Production of heat.* Though this function requires, on the average, the largest amount of oxygen, it is only in the warm-blooded animals that it assumes vital importance.

3. Edenhuizen performed experiments on rabbits, sheep, a dog, and other animals, for the purpose of ascertaining what changes take place in the organism when the action of the skin is suppressed. When one-eighth to one-sixth of the skin of an animal was covered with glue, oil-colour, varnish, gum, tar, &c., it was sure to die of the effects. Edenhuizen concludes from his researches that in the healthy state a small quantity of nitrogen in a gaseous form is given off by the skin, and that, this function being suppressed, the nitrogen is retained in the blood in the form of ammonia, which is then deposited as triple-phosphate in the subcutaneous areolar tissue and in the peritoneum. The nitrogenous compound retained in the blood acts as an irritant to the nervous system, producing rigors, palsies, cramps, and tetanic attacks.

4. Spring maintains that the action of the heart ought to be divided into three chief periods—a præsystole, a systole, and a diastole. The præsystole consists, according to him, in the dilatation of the ventricles, the lowering of the atrio-ventricular valves, and the contraction of the walls of the auricles. During this action the blood is sucked into the ventricles. The change of position which the valves undergo produces a peculiar third sound of the heart, which has remained unnoticed hitherto, and which may be called the præsystolic sound. The systole by which the blood is driven into the arteries follows immediately upon the præsystole, beginning at the base and extending to the apex of the ventricles. During this period the atrio-ventricular valves ascend and shut up the opening, producing the systolic sound of the heart. The systole ceases suddenly. The diastole which then follows forms a period of rest, in which the walls of the ventricles remain in contact with each other. The diastolic sound of the heart is heard during this period.

5. Beau is of opinion that the ventricles are empty during the second period of the heart's action. He states that the true diastole and systole follow each other so rapidly, that the period which comprehends both has been generally regarded as merely the period during which the ventricles contract, and distinguishes, accordingly, a period of diastole-systole, in which the ventricles are at first dilated by the blood which enters into them, and then contracted by the action of the muscular fibres; and a second period, that of rest.

6. Sucquet's investigations go to prove that there exists a double communication of the arteries with the veins, forming two different kinds of circulations. The first, the capillary circulation, is called by Sucquet *circulatio nutritiva*; the second, formed by direct communication of the arteries with the veins, without intervening capillaries, is named by him *circulatio derivativa*. The smallest connecting arteries of this latter circulation possess: (*a*) Organic

muscles, which are easily seen, and which exist in abundance. (*b*) Contractility, and this in such a degree as to admit of vessels being entirely shut by it. (*c*) Vaso-motor nerves, of which a portion governs the contraction, and another the dilatation of these vessels. Thus, he thinks, it becomes intelligible that, as more or less blood enters the veins by these channels, the superficial veins of the head and of the limbs are seen more or less distended by blood. The deep, nutritive circulation remains always unchanged in itself, whilst the derivative circulation varies according to the quantity of the circulating blood, according to the energy and frequency of the heart's action, and according to the action of the vaso-motor nerves.

7 and 8. Schweigger-Leidel and Teichmann have both satisfied themselves, by means of injection, that the walls of the lymphatic vessels are impermeable, even to the finest pulverized substances, and that there exists no direct passage from the unimpaired bloodvessels into the unbroken lymphatics.

IV. Blood, Lymph, and Chyle.

1. Schmidt, Alexander: *On Fibrine, and the Causes of its Coagulation.* (Archiv für Anat. Physiol. &c., pp. 545–587, and pp. 675–721, 1861.)
2. Schöffer, A.: *On the Carbonic Acid of the Blood, and its Elimination by the Lungs.* (Zeitschr. für rat. Med., 3rd series, vol. xi. pp. 89–119.)
3. Weiss, W.: *Experimental Researches on the Lymph-Current.* (Virchow's Archiv, vol. xxii. p. 526.)
4. Schmidt, C.: *On the Chemical Composition and on the Formation of Lymph and Chyle.* (Milange's Physiques et Chim. tirés du Bull. de l'Acad. de St. Petersb., iv. pp. 671–691.)

1. Schmidt's very interesting researches show that it is principally the cells which contain the active principle in the process of coagulation. Lymph and chyle do not possess the property of spontaneous coagulation before their admixture with cells, which takes place in the lymphatic glands. But if cells are artificially added to pure lymph and chyle, coagulation begins at once. The difference in the appearance of fibrinous coagula was found to depend, not on a variety of fibrinous substances, but on the difference of the cells which produce the coagulation. Serous effusions coagulated when to their clear fluid a small quantity of defibrinated blood was added. The action of the active principle is not analogous to that of ferments, for it is used up in the process. It combines chemically with the coagulable substance. Schmidt calls those substances which have the power of inducing certain albuminous fluids to coagulate, *fibrinoplastic.* Among these are especially blood-lymph, chyle, and pus-corpuscles; but also the cornea, the watery extract of the crystalline lens, the humours of the eye, saliva, &c. The fluids which contain the coagulable principle in solution he names *fibrinogenous.* The fibrinoplastic properties he further shows to depend on a substance which is identical with hæmatoglobuline. As regards the influence which the gases of the blood exert upon its coagulation, he makes out that the presence of carbonic acid always retarded coagulation. This retarding influence is but very slight when exerted on fresh blood. He thinks that the differences in time which arterial and venous blood show in reference to coagulation may be accounted for by the different amounts of carbonic acid which they respectively contain. Oxygen and atmospheric air he found to have no direct influence in promoting coagulation; but when much carbonic acid is contained in the blood, oxygen promotes coagulation by displacing that gas. Blood which in vacuo had been freed of all its gases did not lose its fibrinoplastic power. Contact with animal tissues retards coagulation.

2. Schöffer found that the quantity of carbonic acid in the blood of dogs

increases with the amount of phosphoric acid which the latter contains, but that this does not take place in any exact proportion. He thinks that the greatest part of the carbonic acid of the blood is attached to phosphate of soda, and that only a small portion is merely diffused in it. Blood of dogs was obtained almost simultaneously from the carotid artery and from the right side of the heart. 100 vol. of the arterial blood contained 5·5 vol. more oxygen and 4·6 vol. less carbonic acid than the same quantity of the venous blood. The quantity of fixed carbonic acid is much greater in the venous than in the arterial blood. The lungs seem therefore to exert an influence as yet unknown on the elimination of carbonic acid from the body. Blood and blood-serum differ greatly in their amount of fixed carbonic acid. The latter contains a much larger quantity than the former. 100 vol. of blood yielded to the pump 41·48 vol. of gases. Among these were 24·62 vol. of carbonic acid; besides this, it contained 11·28 vol. of gases, of which 10·20 vol. were carbonic acid; the quantity of fixed carbonic acid was 23·77 vol. This observation naturally suggested that the blood-corpuscles have the power of setting carbonic acid free. To determine this point, serum was examined before and after its admixture with blood. It was found that the quantity of fixed carbonic acid contained in the pure blood was 0·81 per cent.; in the pure serum, 16·65 per cent.; and in the mixture of both only 1·77 per cent.; which proves strikingly the correctness of the above suggestion.

3. Weiss performed his experiments on foals and dogs which had been made insensible by means of opium or chloroform. He ascertained with the aid of the manometer the amount of pressure which was exerted by the lymph-current upon the walls of the right truncus trachealis. It was found to measure in foals from 10 to 20 millimetres of a solution of soda of 1·080 specific gravity; in dogs, from 5 to 20 millimetres. Calculation showed that the average velocity of the lymph-current in the truncus-trachealis of foals was 4 millimetres in the second. The quantity of lymph which passed by with the current was 0·65 gramme in the minute, which quantity amounts to one-fifth of the weight of the corresponding part of the body in twenty-four hours. Experiments which were performed on foals for the purpose of determining the quantity of lymph which passed through the upper end of their thoracic duct, yielded the following results:—84·2 grammes for the first foal, 97·8 grammes for the second, and 185·5 for the third, as the average quantity in twenty-four hours for one kilogramme of their weight.

4. C. Schmidt subjected lymph and chyle to careful chemical examination. Lymph taken from the lymphatic trunk of the right side of the neck of a foal which had been well fed with hay, was found to contain:

	1000 parts contain— 789·48 serum.	1000 parts contain— 10·52 coagulum.	1000 parts serum.	1000 parts coagulum.
Water	954·56	9·37	964·77	890·68
Dried substance	34·92	1·15	35·23	109·32
Fibrine, albumen, fats, and fatty acids	23·31	—	23·56	—
Other organic substances	4·48	1·05	—	100·4
Minerals	7·12	0·10	7·22	9·2
Chloride of soda	5·36	0·07	5·42	5·8
Soda	1·47	1·02	1·49	2·5 (Soda, Potass, Sulphuric acid)
Potass	0·03	—	0·03	
Sulphuric acid	0·03	—	0·03	
Phosphoric acid fixed to alkali	0·02	—	0·02	—
Phosphate of lime, phosphate of magnesia	0·21	0·01	0·21	0·9

He found that the contents of the thoracic duct and those of the large lymphatic trunk of the right side of the neck differed from each other quantitatively and qualitatively, scarcely otherwise than by a small quantity of iron contained in the former. Placed under the same circumstances, animals of a similar constitution yielded from the right lymphatic trunk of the neck one per cent. of their weight in lymph, and fourteen per cent. of the weight of half the head and neck. The quantity of chyle and lymph which flows in twenty-four hours into the general circulation was found to be as large as the whole amount of blood in circulation at any given time, containing as large an amount of salts as blood, but only half the amount of organic matter.

V. Secretion and Metamorphosis of Matter.

1. Salisbury, J. H.: *Experiments connected with the Discovery of Cholestearine and Seroline as Secretions in Health of the Salivary-, Tear-, Mammary-, and Sudorific-Glands; of the Testis and Ovary; of the Kidneys in Hepatic Derangements, of Mucous Membranes when Congested and Inflamed, and in the Fluid of Ascites and that of Spina-bifida.* (The American Journal of the Medical Sciences, April, 1863.)
2. Ritter, T. F.: *Some Researches on the Influence of Food upon the Quantity of Bile Daily Secreted.* (Marburg, 1862.)
3. Schiff, M.: *On the Relation which the Liver Circulation has to the Formation of Bile.* (Schweizerische Zeitschrift für Heilkunde, vi. p. 1.)
4. Hoppe, Seyler, F.: *On the Changes which the Bile undergoes in the Intestines.* (Virchow's Archiv, vol. xxvi. p. 519.)
5. Kaup, W.: *Contributions to the Uro-Physiology within the Bladder.* (Tübingen, 1860, and Canst. Jahresb. für 1861, vol. i. p. 135.)
6. Herrmann, M.: *On the Influence of the Pressure of the Blood upon the Secretion of Urine.* (Wiener Sitzungsberichte, 1861, vol. xlv. p. 317.)
7. Pettenkofer, M., and Voit, C.: *On Respiration.* (Ann. der Chem. u. Pharm., Suppl., vol. ii. pp. 1–70, 1862.)
8. Ranke, Joh.: *Elimination of Carbon and Nitrogen in the Human Subject during Rest.* (Archiv für Anat. Physiol., &c., July, 1862.)
9. Rev. S. Haughton, M.D., of Trinity Coll., Dublin: *Physiological Researches.* (Medical Times and Gazette, Feb. 7, March 7 and 21.)

1. Dr. Salisbury's experiments are a continuation of those begun by Austin Flint, the results of which we gave in our last Report. He arrived at the following conclusions: 1. Cholestearine occurs largely in the ova of the human subject and of animals. 2. In the seminal fluid of the human subject, seroline and cholestearine are largely present, the former more so than the latter. 3. Cholestearine occurs very largely as a secretion in the saliva. No seroline is found. 4. Neither seroline nor cholestearine occurs in healthy urine. 5. Cholestearine occurs largely, and seroline in small quantities, in jaundice-urine. (These bodies are probably always secreted by the kidneys whenever the liver, through organic or functional derangements, is unable to secrete them from the blood.) 6. Cholestearine and coloured blood-discs are secreted or effused from highly congested and inflamed mucous membranes. 7. Cholestearine is secreted or effused from the peritoneal membrane in ascites. 8. Cholestearine occurs largely in the fluid of spina-bifida tumours. 9. Cholestearine is secreted by the tear-glands. 10. Human milk, previous to birth, is rich in cholestearine. No seroline was detected in the experiment made. 11. After the birth of the child, and during nursing, the mammary glands secrete largely cholestearine and seroline. 12. The milk of the cow is rich in cholestearine and seroline. 13. Butter, beef- and hog-suet contain cholestearine

and seroline. 14. The primary forms of the crystals of cholestearine appear to be the cube and rhombic prism, and that of seroline the very acute rhombic or rhomboïdal prisms, though appearing usually as simply asicular. 15. Cholestearine and seroline are largely secreted from the blood by the sudorific glands during the sweating stage of intermittent fever. These glands become important blood-depurative organs in this disease. 16. The kidneys largely secrete cholestearine in intermittent fever. 17. The kidneys secrete cholestearine in varicella. 18. The kidneys secrete cholestearine in diphtheric conditions. 19. The kidneys largely secrete cholestearine in the disease known as diabetes mellitus. 20. The kidneys secrete cholestearine and seroline in remittent fever. 21. The kidneys largely secrete cholestearine in typhoïd fever. 22. Cholestearine is secreted by the sudorific glands in health.

2. Ritter's researches were made on a dog of 13½ kilogrammes in weight. The common bile-duct was divided after a double ligature had been applied, and a fistulous opening was established between the skin and the gall-bladder. An accurately-weighed sponge was affixed to the opening, and in this all secreted bile was caught. In the first series of experiments, which lasted seven days, the dog was daily fed on 2500 grammes of lean horseflesh, without any water. The weight of the animal increased during the time from 12,820 grammes to 14,000 grammes, and the average quantity of bile which was daily secreted amounted to 255·5 grammes. This gives (taking 13,629 grammes to be the mean weight of the dog) 18·75 grammes of bile for each kilogramme of the animal in twenty-four hours. When the dog was fed daily on 2000 grammes of horseflesh, 220·15 grammes of bile were secreted daily on the average, which gives 15·2 grammes of bile per diem to each kilogramme of the animal. Then the dog received 1500 grammes per diem. The weight of the body maintained itself almost unaltered; 196·5 grammes of bile were daily secreted, which for a mean weight of 14,544 grammes gives 13·4 grammes of bile to each kilogramme in twenty-four hours. The weight of the body decreased whilst the dog was fed during four days with 1000 grammes of flesh only. 148·1 grammes of bile were daily secreted, or, for a mean weight of 14,175 grammes, 10·5 grammes for each kilogramme in twenty-four hours. Though the quantity of bile diminished absolutely with the decrease in the quantity of food, relatively it increased. The dog was fed at 7 A.M. with 450 grammes of half-dried horseflesh and 300 cubic centimetres of water. In the first hour after the meal 12·1 grammes of bile were secreted; then the quantity secreted during each hour decreased, till in the fifth hour 5·5 grammes only were produced. In the sixth hour a second maximum was attained, amounting to 7·8 grammes, from which time the quantity slowly diminished, till in the evening it was 6·3 grammes. Ritter attributes the rapid increase in the secretion of bile, shortly after food has been taken, partly to the water getting into the circulation, and partly to the pressure exerted by the full stomach upon the liver and bile-ducts. The less the amount of food which has been taken, the sooner the second maximum in the secretion occurs. At the end of twenty-four hours after a rich meal, the bile continued to be secreted in greater quantity than it did at the same period after a poor meal. To determine the influence which the addition of fat to the food exerts on the secretion of bile, the dog was fed during two days with 1000 grammes of meat and 125 grammes of fat per diem, then during one day with 1000 grammes of meat only, and again during two days with meat and fat. On each of the first two days 210 grammes of bile were secreted, on the third day only 170 grammes, and on each of the last two days again 210 grammes.

3. Schiff made experiments for the purpose of ascertaining from which kind of blood the bile is secreted. He first tried to cut off all arterial supply of blood to the liver. For this purpose he found it necessary to tie the cæliac axis and the inferior diaphragmatic artery. The common bile-duct was also

tied, and the gall-bladder was opened and emptied, and a tube was fixed to it. A little time after the operation bright yellow bile began to flow from the tube. This contrasted in colour with the green bile which had been previously found in the gall-bladder, and was in consequence thought by Schiff to be newly secreted. The conclusion to which Schiff arrives is, that after all supply of arterial blood has been cut off from the liver it still continues to secrete bile, and that the quantity of bile thus secreted, compared with the normal quantity which Bidder and Schmidt have found in the case of cats, does not show any diminution. In the next series of experiments the venous supply was cut off. To effect this, Schiff isolated the artery from the lesser omentum, and tied the rest, the common bile-duct included, with a single ligature. The veins were also separately tied. Schiff does not think that the injury to the nerves which were tied along with the vessels could have had influence on the secretion of the bile in the short time during which the experiment lasted. A number of cats which had been operated upon in the above way died from within forty-one minutes to an hour and a half after the operation. Previous to their death, they lay in a drowsy state. Not a drop of bile escaped from the tube inserted into their emptied gall-bladder. To test the direct effect of the operation, all the steps of it were performed on a cat, with the exception of the tying of the ligature. The animal remained quite lively, and bright yellow bile dropped from the tube. An hour and a half after the operation the ligature was drawn tight, and scarcely had twelve minutes elapsed when the cat fell down comatose. Death occurred at the end of fifty-five minutes, and no bile had been secreted during that time. Schiff believes that it is anæmia of the liver which so rapidly causes coma and death, and suggests that the constituents of the bile thus retained in the blood may possibly act as a violent poison in such cases. A further series of experiments were performed for the purpose of determining the effects of gradual closure of the portal vein. This closure is known to take place in human subjects without the secretion of bile being interrupted by it, and Orè has produced the same state artificially in animals. In dogs and cats the portal vein was gradually constricted by means of a loop, till at the end of eight days it was entirely closed. The much-dilated superficial veins of the abdomen established a communication between the vena cava inferior and the vena mammaria. The kidneys were congested. The liver was partly congested and partly anæmic. Schiff does not think that either the experiments of Orè, or the pathological observations on the secretion of bile after obliteration of the portal vein, are contradictory to the notion that the bile is formed from the blood conveyed to the liver by the portal vein; but neither does he believe that the blood of the portal vein is especially adapted for the formation of bile. He endeavoured to show by experiment that bile may be formed quite as well from arterial blood, if this is carried through the channels of the portal vein. For this purpose he diverted the blood from the renal artery of cats, through an india-rubber tube filled with tepid water, into the portal vein. In one instance he believes that the experiment succeeded—that is, that the flow of the blood from the artery into the vein continued for an hour and a quarter, and that 17 grammes of bright yellow bile found in the gall-bladder, which had been emptied previous to the experiment, were secreted during that time.

4. Hoppe experimented upon dogs, oxen, and birds. He found that in the intestines, just as in putrefaction or in the boiling of bile with caustic alkalies, the tauro-cholalic acid was easily, the glyco-cholalic acid only with difficulty decomposed, but that both cholalic acid and glyco-cholalic acid appeared in the excrements. The division of the tauro-cholalic acid begins in the small intestines, but is completed more especially in the large intestines. This division is spontaneous, for it is not brought about by any ferment supplied from the intestines.

5. Koup sought to determine if any constituents of the urine were absorbed during its stay in the bladder. For this purpose he compared urine which had been passed hourly for twelve hours with such as had been passed only once at the end of twelve hours. He found, after several trials, that the average surplus in favour of the former amounted to—

87·3 cub. cent. urine.	0·933 grms. urea.
0·786 grms. chloride of sodium.	0·173 grms. phosphoric acid.
0·061 grms. sulphuric acid.	2·116 grms. firm constituents.

The decrease for one hour is therefore as follows—

7·2 cub. cent. urine.	0·079 grms. urea.
0·065 grms. chloride of sodium.	0·014 grms. phosphoric acid.
0·005 grms. sulphuric acid.	0·176 grms. firm constituents.

The decrease can only be accounted for by absorption, which the constituents of the urine undergo during their prolonged stay in the bladder.

6. Herrmann makes out that the small arteries which enter the capsules of the kidney have a share in the secretion of the urine. After the renal artery had been tied, he observed in 3 cases out of 18 that the urine was nevertheless secreted uninterruptedly, and even with greater rapidity than previous to the operation. The urine which was secreted after the circulation had been interrupted for several hours contained albumen, sometimes in large quantities. Herrmann performed also experiments on the partial closure of the artery, that is, on the diminution of the pressure of blood within the kidney. He concludes, from these experiments, that it is not the diminished rapidity of the current of blood, but the diminished pressure, which causes a proportionate diminution in the secretion of urine; and, as secretion ceases altogether when a certain minimum of pressure is attained, when a certain maximum of pressure is surpassed the urine assumes abnormal qualities. He believes that the urine secreted in the Malpighian bodies is very concentrated, and during its passage through the urinary tubes urea is taken up again into the blood. He supports this view by stating that only during very slow secretion of concentrated urine was it found that the amount of urea was diminished; that this diminution grew larger when pressure was exerted against the outflow of urine, and that the urea disappeared entirely when, by means of a ligature round the ureter, the urine was retained in the kidney.

7. A diagram and a description of the apparatus, with the aid of which Pettenkofer made his beautiful researches, is given in the 'Lancet,' Nov. 1st, 1862. In testing the accuracy with which the quantity of carbonic acid contained in his apparatus could be ascertained, he found that the greatest error amounted to 1 per cent.; the average was not more than 0·3 per cent. A dog, weighing 33·3 at the beginning of the experiment, was put into the apparatus immediately after having been fed, and was left there each time for twenty-four hours. He found that the elimination of nitrogen was not proportionate to that of carbonic acid. Whilst the quantities of urea varied from 8·3 to 108·8 grms, those of carbonic acid varied only from 289·4 to 840·4 grms. As regards carbonic acid, the conclusion may be drawn that in the same subject the quantity of heat produced by the process of decomposition may be at one period three times greater than at another. During starvation both the amount of urea and the amount of carbonic acid decreased, till at last only half the normal quantity of carbonic acid was eliminated. In estimating the quantity of substance used up during starvation in twenty-four hours by the urea secreted in that time, 200 grms. of fat were found to have been consumed; whilst the dog was fed on 400 grms. of meat and 250 grms. of starch and sugar, all nitrogen and carbon taken with the food were found in the excrements; but if the dog was fed on the same quantity of meat, and with 200 grms. of fat, all

the nitrogen was eliminated, but not all the carbon, which shows that in the latter case fat was added to the bulk of the body. In taking 400 grms. of meat and 200 grms. of glue, more carbon was eliminated by the lungs and skin than the food contained; but, on the other hand, not all the nitrogen was found in the urine, which indicates that in this case a quantity of non-nitrogenous substances belonging to the tissues of the body (probably fat) was consumed. The carbon of 800 grms. of bread was entirely eliminated in twenty-four hours. When 350 grms. of fat were given, the tissues of the animal yielded in excretion a nitrogenous substance, but nevertheless fat was formed in the body, for the carbon of the food was not entirely eliminated. The dog, being fed on 200 grms. of glue, a loss of nitrogen was observed, and more carbonic acid was eliminated than was contained in the glue. If a large quantity of meat was taken, the entire quantity of carbon contained in it was not found in the expired air, but, on the other hand, the whole amount of nitrogen reappeared in the urine. Fat was most likely formed in this case. In the first experiment, after the dog had taken daily 1500 grms. of meat for sixteen days, 63·7 per cent. of the inhaled oxygen reappeared in the carbonic acid. The dog, having then been kept fasting for ten days, eliminated at the end of that time much less carbonic acid than before, but more oxygen with it; whilst the dog was fed on meat and starch the quantity of exhaled oxygen kept increasing, till at last fifty per cent. more oxygen was contained in the eliminated carbonic acid than had been inhaled. This circumstance could only be accounted for by assuming that carbonic acid had been formed from the starch and sugar. If this was actually the case, the hydrogen must certainly have been liberated in sufficient quantity to be found in the atmosphere of the apparatus. An examination of that atmosphere proved that this view was correct.

8. Similar experiments to those which Bischoff and Voit performed on dogs were made by Ranke upon himself, for the purpose of determining the quantities of carbon and nitrogen which were eliminated from the body in a state of rest. First he endeavoured to make out whether it was possible, by any diet, so to regulate the elimination of matter from the body that in twenty-four hours exactly so much nitrogen is given off with the excrements as has been taken in with the food during that time. This equilibrium took place when not only the consumption of nitrogen, but also the consumption of carbon, is entirely covered during the time of the experiment. In Pettenkofer's apparatus, the quantity of expired carbon, as determined on a dog in which the above-mentioned equilibrium was established, amounted to 207 grms. in twenty-four hours. Insufficient food may contain too little carbon or too little nitrogen. In this case, more nitrogen is eliminated than has been taken. An addition of fat to the food diminishes the elimination of nitrogen. Ranke made experiments on fasting. He commenced them twenty-four hours after the last meal had been taken. They were continued for twenty-four hours, during which time the experimenter kept himself as quiet as possible. It was found that for an average bodily weight of 71·25 kilog., 9·01 grms. of nitrogen and 184·85 grms. of carbon were eliminated on an average. The average proportion between the eliminated nitrogen and carbon during fasting was 1 : 20·5. When large quantities of meat were given, a decrease in the weight of the body was, nevertheless, each time observed, provided the diet was insufficient to produce the entire quantity of carbon required in the act of respiration. Some experiments which were made on the influence of non-nitrogenous diet showed that by an average bodily weight of 72·57 nitrogen, 2·775 grms. of carbon, and 0·1124 grms. of nitrogen, were given off for each kilog. in twenty-four hours.

HALF-YEARLY REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.
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I. *On the Assimilation of the Lactate of Iron and its Superiority over the other Chalybeates in Reference to Digestion.* By Dr. A. Cordier. (Edinburgh Medical Journal, April, 1863.)

Dr. Cordier considers that the lactate of iron is the safest chalybeate preparation in point of digestion. It is the only chalybeate which can be prepared in the human body, as is proved by digesting for twelve hours, at a temperature of 104°, some iron fillings with distilled water and calf's rennet; hydrogen is disengaged, and lactate of iron is formed. Bernard has also proved that the lactate of iron may be injected into the veins in large quantity without producing any bad symptoms. It readily combines with the albuminous fluids, and its combinations are easily assimilated without fatiguing the stomach. Unlike other chalybeates, the lactate of iron, far from impeding digestion and impairing the appetite, rather strengthens them, as has been stated by M. Boulleaud, in his report on the lactate of iron to the French Imperial Academy of Medicine. MM. Fouquier and Hardy, in a note appended to this report, state that they have several times prescribed the pastilles of lactate of iron in cases of chlorosis with amenorrhœa, and after three or four days there was always such an increase of appetite that the patients complained of the insufficiency of their diet. M. Quevenne has found that a dose of thirty grains of reduced iron, or of the sesquioxide, even when taken at meals, produces diarrhœa and vomiting; while the lactate, administered eight times in a dose of fifteen to thirty grains, produces no inconvenience. The difference is evidently due to the fact that, in the first case, the stomach has to dissolve the iron and convert it into the lactate, while in the latter the preparation is already formed.

II. *On the Use of Perchloride of Iron in Diphtheria.* By M. Courty. (Researches on Croup and Diphtheria. Montpellier, 1862.)

It is now generally admitted, that the pseudo-membranous formations of the throat or larynx ought to be treated with tonics instead of antiphlogistics. M. Courty is persuaded that these diseases are of an adynamic nature, and he disapproves of any measure which would have the effect of weakening the organism, admitting only such remedial means as are of an opposite nature, such as a substantial and restorative diet, and among medicines, cinchona, and especially iron, which without being a specific, he regards as assisting essentially in the cure. He considers the solution of the perchloride of iron to be the best of the ferruginous preparations, but he thinks that there are some others which may be preferred in certain cases, as for instance, the iodide of iron in scrofulous children. M. Courty employs the perchloride in two forms:—1. Internally, in the dose of twenty-five to thirty drops in a glass of water in twenty-four hours, given in teaspoonfuls, and each dose followed by a mouthful of milk to remove the styptic after-taste. He continues the medicine, even after the cure, for a sufficiently long time to restore the strength and to shorten the convalescence. 2. He employs it locally, if not to the exclusion of every other topical application, still with a marked preference, and the reason of the preference is, that the solution has at once a caustic, hæmostatic, and tonic action, by virtue of which it very favourably modifies the surface after the removal of the false membrane, and spares the neighbouring parts, which are not denuded of epithelium; and besides, if it is impossible to remove the diphtheritic patch, the perchloride of iron possesses the valuable property of acting upon it and penetrating it, of infiltrating itself under its edges, and thus going on to reach and modify the subjacent tissue.

III. *On the Combination of Protiodide of Iron and Manna.* By M. FOUCHER, of Orleans. (L'Union Médicale, April 7th, 1863.)

The protiodide of iron, in the form of pills, is very commonly employed in France, and the preparation generally contains the protiodide with a certain proportion of honey, powdered liquorice, and powdered mallow. M. Foucher recommends a kind of sugar-plum (*dragée*), composed of protiodide of iron, purified manna in tears, and powdered liquorice and mallow. By the union of these substances, M. Foucher obtains a mass in which the combination of manna with the protiodide preserves the latter from any alteration, and it forms a true paste which, when divided into pills or *dragées*, softens by the heat of the hand. Each *dragée* broken into pieces presents internally the greenish colour characteristic of the well-prepared and well-preserved protosalt of iron. In this preparation, therefore, there are two essential conditions observed—namely, the preservation of the salt without chemical alteration, and its easy solubility in the digestive passages. But M. Foucher also believes that the manna, by its laxative operation and its stimulating the secretions of the digestive canal, would also act both in facilitating the absorption of the drug and preventing the tendency to constipation which is one of the most troublesome effects of the ferruginous preparations.

IV. *On the Treatment of Tetanus by Large Doses of Morphia.* By Dr. GRESSY. (L'Union Médicale, May 26th, 1863.)

In an extremely severe case of tetanus in a boy of fourteen years of age, twenty days after the cure of a puncture of the sole of the left foot by a thorn, Dr. Gressy employed the hydrochlorate of morphia in large doses; and although the tetanic convulsions had already seized the diaphragm and the extrinsic muscles of the thorax, he had the satisfaction of saving the patient's life. As soon as the first symptoms of *morphinic* poisoning (sleep and abundant sweating) were manifested—namely, less than twenty-four hours after the commencement of the treatment, the tetanic symptoms made no further progress. The fits came at longer intervals, but at every attack the trismus, opisthotonos, and the abdominal and thoracic stiffness preserved their intensity. The fits soon diminished in length, and lastly the convulsions became less violent. Dr. Gressy was obliged to wait a week before all traces of convulsion had disappeared, and during this time the nervous system of the patient remained permanently *morphinized*. When the sleep was tranquil and continuous, and the sweating profuse, the morphia was discontinued for twelve or twenty-four hours, and then its use was resumed. Miliary eruption and itching exhibited themselves at the same time that the tetanic convulsions began to diminish. Dr. Gressy therefore believes that the want of success which has often been observed in cases of tetanus treated by morphia, is due to the timidity with which the remedy has been employed; and he observes, that the tetanus, when arrived at its highest degree of intensity, began and continued its retrograde course in proportion as the *morphinic* poisoning affected the innervation.

V. *On the Application of Local Remedies to the Respiratory Passages.* By Dr. EDOUARD FOURNIÉ. (L'Union Médicale, February 5th, 1863.)

Dr. Fournié does not agree with many physiologists in affirming that liquids do not penetrate at all into the trachea, and he has proved experimentally that a certain quantity of liquid may penetrate into the larynx without producing any appreciable sensation. Hitherto the only medicinal agents which could be introduced into the bronchi were in the form of vapours, gases, and emanations, but the action of these substances was unsatisfactory, and pulverised liquids were substituted for them, although with little success. But although liquids in the form of dust do not penetrate into the chest with the regularity, precision, and certainty required by the medical practitioner, there is another class of pulverulent bodies which penetrates with too much facility, and researches with the laryngoscope have removed all doubt upon the subject. Dr. Fournié has clearly ascertained that the dust of charcoal, starch, flint, &c., penetrates into the bronchi, and he has endeavoured to render this fact available in the treatment of disease. In order to obviate any disagreeable sensation on the part of the patient, he recommends the medicinal powder to be breathed in a special apparatus which he describes. It consists of an oval wooden box fitted with two tubes, so arranged that the patient breathes the air in the box mingled with the medicinal powder. Laryngoscopic examination and the black expectoration after the use of this apparatus, when charcoal-dust has been employed, prove that the dust has entered the respiratory passages. The same apparatus is available for the inspiration of volatilized iodine, and Dr. Fournié has also thus employed for medicinal purposes starch, alum, tannin, subacetate of lead, and nitrate of silver. The diseases which he has successfully treated have been cases of chronic catarrh, bronchitis, and some of well-marked phthisis.

VI. *On the Use of Purgatives in the Treatment of Dysentery.* By Dr. J. DÉLIOUX DE SAVIGNAC. (Bulletin Général de Thérap., May 30th, 1863.)

After ipecacuanha, purgatives are the most suitable remedies for dysentery. Bretonneau was one of the first to demonstrate clinically the importance of this class of remedies in the disease in question, and the mortality returns, the relapses, and chronic cases, have diminished since the evacuant plan has superseded the antiphlogistic in the treatment. Purgatives, in fact, increase the peristaltic movements and the muscular contractions of the intestines, and afford a free passage at first to the matters, such as bile and excrement, which are accumulated in their cavities, and afterwards to the new products of secretion drawn to them by the dynamic action of the drugs; in other words, they re-establish the normal conditions of defæcation, they produce an afflux of blood and serosity, acting upon the liver, the orgasm of which they moderate, and upon the vascular network of the mucous membrane, which they disgorge of its contents; and lastly, they induce diarrhœa. But only mild purgatives should be given, because it is essential not to irritate, and especially not to inflame the intestinal mucous membrane. The energetic and irritating purgatives are not at all suitable in dysentery, such as colocynth and croton-oil; and even the resinous kinds in general do harm, such as jalap, scammony, and aloes; and calomel and rhubarb, which are frequently beneficial, sometimes become dangerous. Dr. Délioux, in the choice of purgatives, prefers the phosphate of soda, the tartrate of soda, and the tartrate of soda and potash, all of which exert a mild but at the same time an aperient action. Castor-oil is employed by Dr. Délioux very frequently, and the only objection to its use is that it may be inadequate to the purpose required. He does not much approve the use of calomel, except when it is necessary to act energetically on the biliary secretion when it has been obstinately suspended. Some pills prescribed by Segond, physician-in-chief in French Guiana, have been very much recommended by the French medical practitioners in Cayenne. They consist of a mixture of ipecacuanha, calomel, and the watery extract of opium, and although Dr. Délioux does not think them particularly serviceable in acute dysentery, he often uses them with success in the chronic form of the disease.

VII. *On the Employment of the Sarracenia Purpurea in Small-pox.* (Edinburgh Medical Journal, January, 1863; Lancet, January 10th, 1863; Medical Circular, June 10th, 1863.)

In our half-yearly report for January, we alluded to the successful results said to have been obtained in Nova Scotia in the treatment of small-pox by the Sarracenia purpurea, or Indian cup. Dr. Haldane has reported to the Medico-Chirurgical Society of Edinburgh in December last, that he has tried this plant in a considerable number of cases occurring in the Royal Infirmary during the course of an epidemic of small-pox. He first tried the leaves, and as these produced no effect, he employed the root, furnished to him by Messrs. Savory and Moore. The result of his observations was, that the root has no effect, for the cases ran through their course without the slightest modification, no change was produced in the characters of the eruption, and no physiological action of any kind was manifested. Dr. Haldane remarked that he took care to select only subjects who were not vaccinated, and who had not previously suffered from small-pox, for if irregularities had been met with in cases of modified small-pox, it would have been difficult or impossible to draw any conclusions relative to the value of the remedy employed. Dr. Haldane has observed no good effect to follow the use of the sarracenia in small-pox, and he has consequently abandoned its use, and the more particularly because the price is too high to justify the continuance of useless experiments. Dr. Goyder, of Bradford, relates in the 'Lancet' the particulars of two cases of small-pox treated by the sarracenia, but the first of these shows no evidence either for or against the use of the plant, and in the second the patient died. Mr. Marson, in a paper read at the Epidemiological Society on the 1st of June, related the particulars of a number of cases treated by the sarracenia at the Small-pox Hospital at Highgate, in which the plant seemed to produce no good effects. Mr. Cosmo Logie states that he has employed the sarracenia successfully, in the case of some soldiers attacked with small-pox, but they were all instances of small-pox modified by vaccination.

VIII. *On the Successful Use of Iodide of Potassium in the Treatment of Aneurism.* By Dr. Roberts, Manchester. (British Medical Journal.)

A collier, aged thirty-nine, had a pain in the chest, a troublesome cough, and difficulty of breathing at night. It appeared that he had been kicked in the chest about four months before he came under treatment, and from that time he had been subject to paroxysms of very severe pain, and after about two months from the receipt of the injury, he observed an unnatural prominence of the upper part of the sternum. When he was examined uncovered in the hospital, the first bone of the sternum and the parts immediately adjoining were observed to be bulged out, and to be the seat of a heavy pulsation. On the left side of this prominence, in the second intercostal space, close to the sternum, there was a soft pulsating elevation of a conical shape. The first sound of the heart was faintly murmurish over this elevation, and a distinct though faint systolic murmur existed in the course of the innominate artery, and this was intensified over the right carotid and in the acromial angle. Dr.

Roberts considered that the diagnosis was clearly aneurism of the arch of the aorta making its way forward through the parietes of the chest. The patient was ordered to take five grains of iodide of potassium three times a-day, to keep his bed, and to limit as much as possible the quantity of liquids taken as drink. Three days afterwards, the iodide was increased to seven grains and a half three times a-day. Six days after the commencement of the treatment, the patient reported himself a great deal better, the paroxysms of pain no longer recurred, the cough and difficulty of breathing were less troublesome, and the soft pulsating elevation was slightly less prominent. The dose of the iodide was increased to ten grains. The patient still went on favourably, and was free from pain, and the soft elevation had almost entirely subsided to the level of the surrounding parts. The dose of the iodide was successively raised to fifteen and twenty grains three times a-day, and on the twenty-fifth day of treatment, the area of dulness was very much reduced, and on the thirty-ninth day it was found that the tumour had diminished three-quarters of an inch in one direction, and an inch in the other. The patient was quite free from pain and difficulty of breathing, and felt himself exceedingly well, but all the local and general symptoms had not disappeared when he ceased to attend the hospital.

IX. *On the Calabar Bean as a New Ophthalmic Agent.* By Dr. D. ARGYLL ROBERTSON. (Paper read before the Edinburgh Medico-Chirurgical Society, Feb. 4, 1863.)

Dr. Robertson has for more than a year endeavoured to discover an agent which, when applied to the conjunctiva, should produce effects exactly opposite to those known to result from belladonna; which should stimulate the muscle of accommodation (the ciliary muscle) and the sphincter pupillæ, as belladonna paralyzes them. Dr. Robertson had sought in vain for such an agent until Dr. Fraser informed him that he had seen contraction of the pupil result from the local application of an extract of the ordeal bean of Calabar. Dr. Robertson, with some difficulty, obtained a few Calabar beans, and prepared from them three extracts of varying strength, made by digesting the powdered bean in alcohol, and evaporating the strained liquid to dryness. In one of the preparations, one minim of the solution corresponded to about half a grain of the bean; in a second, one minim corresponded to about two grains of the bean; and in a third, which was the strongest, one minim corresponded to four grains of the bean.

Dr. Robertson then proceeded to make experiments with these solutions on his own eyes. He first introduced a drop of the weakest extract into his left eye, and in ten minutes the peculiar effects were observed. The results were that the bean acted first on the accommodation of the eye, causing indistinct vision of distant objects to such an extent, that beyond eight inches from the eye they all appeared dim and indistinct. The next marked effect was contraction of the pupil, its diameter being reduced from 2′ lines to half a line; and as a natural consequence of less light passing to the retina of the eye experimented on, the pupil of the other eye became sympathetically somewhat dilated, while with the affected eye all objects appeared darker than natural. In a second experiment, a drop of the weakest extract was dropped into the eye, and after some of the expected results were produced, a drop of solution of atropine was introduced. This experiment led Dr. Robertson to the conclusion that the Calabar bean and belladonna are exactly antagonistic in their action on the eye. A third experiment was made to determine more exactly the relative properties of atropine and the Calabar bean. Solution of atropine was introduced into both eyes, and the physiological action was soon produced, and a drop of the strongest extract of the Calabar bean was introduced into the right eye. After a little time, during which the vision in both eyes was impaired, the extent of clear vision with the right eye had very much increased, while with the left only large-sized print could be read, and that only when held at a considerable distance from the eye. The result of all the experiments proved that the local application of the Calabar bean to the eye induces, *first*, a condition of short-sightedness, and, *secondly*, contraction of the pupil, and, sympathetically, dilatation of the pupil of the other eye. Atropine possesses the power of counteracting these effects, and *vice versâ* the Calabar bean is capable of overcoming the effects produced by atropine. The first symptom noticed is dimness of distinct vision, and shortly afterwards the pupil becomes contracted. The symptoms also subside in the same order, first, the derangement of accommodation, and then the affection of the pupil.

Dr. Robertson supposes the following to be the method of action of the Calabar bean. Its effects on the pupil may be produced either by causing constriction of the circular fibres of the iris, or by paralyzing its radiating fibres. He thinks that the contraction of the pupil is due to increased action of the sphincter pupillæ, and chiefly on the ground that the other effects produced by the Calabar bean can only be explained by an induced contraction of the ciliary muscle, the muscle of accommodation; and as the sphincter pupillæ and ciliary muscle are both supplied by the ciliary nerves, the bean may be regarded as a stimulant to those nerves.

As regards the cases in which the Calabar bean may be applied in practice, Dr. Robertson thinks that it may be advantageously employed in cases of retinitis with photophobia, to diminish, by contraction of the pupil, the access of light to the retina; but he expects it to produce the most beneficial effects in cases where paralysis of the ciliary muscle occurs as a consequence of long-continued debilitating disease. Such cases are occasionally reported as following attacks of typhus or other fevers, and the dimness of vision frequently following diphtheria may probably be also relieved by this agent. It may also be serviceable in cases of ulceration at the margin of the cornea, leading to perforation, or even when prolapsus of the iris has just occurred, as well as in cases where the iris has a tendency to protrude through a corneal wound; in such instances it would act by contracting the pupil, and thus drawing away the iris from the circumference. Dr. Robertson concludes by observing that in the Calabar bean we possess an agent that will soon rank as one of the most valuable in the ophthalmic pharmacopœia.

The Calabar bean is the fruit of the *Physostigma venenosum*, a poisonous plant of the leguminous order, which contains very few poisonous species; but the Calabar bean is an exception to the general rule in this respect.

X. *On the Effects of the Calabar Bean on the Pupil of the Eye.* By JOHN W. OGLE, M.D. (British Medical Journal, June 13, 1863.)

Dr. Ogle performed a number of experiments with a view of testing the power exercised by solutions of the bean in contracting the pupil (whether dilated by the use of atropine or not, and under various circumstances), also of ascertaining how long the contraction of the pupil produced by the bean would remain, and whether it would be easy by means of the atropine or belladonna to dilate the pupil which had been contracted by the bean. He came to the conclusion that we have in the Calabar bean a ready and effective agent for producing contraction of the healthy and natural pupil, and also of neutralizing the effects produced on the pupil by atropine; moreover, that the state of contraction induced by the Calabar bean "is *much less troublesome* to the patient, and also lasts a much shorter period of time, than does the dilatation from atropine or belladonna."

XI. *On the Effects of the Solution of the Calabar Bean on the Accommodation of the Eye and on the Pupil.* By J. Soelberg Wells. (Medical Times and Gazette, May 16, 1863.)

Mr. Wells confirms the statements made by Dr. Argyll Robertson as to the effects of the Calabar bean in stimulating the sphincter pupillæ and the ciliary muscle to contraction, and he adduces additional evidence as to the peculiar properties of this drug. A portion of the strongest solution was given to Mr. Bowman, who applied some of it to his own eye, and gave a minute and circumstantial detail of its effects. The general result was that it produced contraction of the pupil in the eye to which it was applied, together with indistinctness of vision and astigmatism. Mr. Wells had an opportunity of testing the effects of the Calabar bean in a case of paralysis of the circular fibres of the iris and of the ciliary muscles of the right eye (the left being perfectly normal), the patient having fortunately presented herself at the Middlesex Hospital in April last. The affection appeared to be of rheumatic origin. The sight of the right eye was so much affected that she could not see to read or to thread a needle when both eyes were open, and she was also troubled with diplopia. About the same time she likewise noticed a marked increase in the size of the pupil of the right eye. The solution of the Calabar bean was dropped into both eyes, and the effects were very accurately and minutely noted; but the general results fully illustrated the peculiar action of the drug upon the size of the pupil and the accommodation of the eye. The pupil of the healthy eye was rapidly contracted, and the paralyzed pupil, which before the application measured three lines and a half, was contracted to two-thirds of a line. The sight of the right eye also rapidly improved, for within a quarter of an hour of the application she could read with it the finest print.

XII. *On the Therapeutical Employment of Mineral Waters.* By Dr. Durand-Fardel. (L'Union Médicale, Jan. 8th and 10th, 1863.)

In an introductory lecture on the employment of mineral waters, Dr. Durand-Fardel explains in general terms the value of these agents in the treatment of disease. He regards as mineral waters all those natural waters which, by reason of their composition or their temperature, are employed with a therapeutic object. All spring and river waters are mineralized, and sometimes to as great a degree as some mineral waters endowed with powerful properties. Dr. Fardel places thermality in juxtaposition with mineralization, because there are some waters which would unquestionably lose a great part of their therapeutical value if they were deprived of their thermality. During the last twenty years, iodine and arsenic have been found in a great number of mineral waters in which they had not previously been recognised; and later still, the new method of spectrum-analysis has revealed the presence of metals, the existence of which was formerly unknown. It is not ascertained under what form chemical bodies exist in mineral waters, for although iron, arsenic, iodine, soda, and carbonic acid, obtained from mineral waters, are the same substances as those which are extracted from the soil and the atmosphere, yet they do not exist under the same conditions as in the inorganic compounds treated or created in the laboratory. It must be also observed, that mineral waters present, in a state of solution, principles which to the chemist are perfectly insoluble; that they are penetrated by gases in proportions which are almost inconceivable; that they contain inorganic matters, the true characters or origin of which are unknown; that some of their principles manifest a physiological and therapeutical action in doses which are inert in other media; that they secure a tolerance of principles which could not be administered with impunity in similar proportions; and lastly, that waters scarcely mineralized, as those of Mont-Dore, Plombières, Néris, and many others, exercise an action on the organism which could not be expected from their known chemical constitution. Dr. Fardel does not claim for mineral waters any magical property in curing diseases, but he points out the necessity of examining the composition of a mineral water in order to ascertain the diseases for which it is suitable; and again he urges that the nature of a disease should be well understood in order to know the mineral water which is proper for its treatment.

XIII. *On the Therapeutical Application of the Leaves of the Olive.* By Dr. Derblich. (Allgemeine Wiener Med. Zeitung, 1862.)

The leaves of the olive have an aromatic smell and a rather austere and bitterish taste, and they contain, according to the analyses of Pallas and Casale, a crystalline substance, to which a febrifuge action has been ascribed, a black resin, gum, a green extract, sugar, tannin, and gallic acid. The infusion forms a brownish, fatty, austere-tasting, astringent solution, which becomes very dark-coloured by the preparations of iron. When macerated with spirit of wine and strained, a greenish and not unpleasantly tasting tincture is obtained. Dr. Derblich has used the decoction more particularly, both internally and externally. In large doses it causes loss of appetite, disagreeable taste in the mouth, with eructation, and even vomiting, and occasionally severe dyspeptic symptoms. The urine becomes dark-coloured, the alvine evacuations less frequent and of a consistent nature, and containing a great quantity of oil-globules. The complaints for which Dr. Derblich employed the decoction and tincture in Dalmatia, were especially catarrhal affections of the mucous membranes, blenorrhœa, burns, and ulcers, the latter being treated locally by injections and poultices. Several patients who had suffered from obstinate diarrhœa, combined with violent pain, were cured in a short time by the sole use of these leaves. They also acted beneficially on the mucous membrane of the respiratory organs. A severe bronchial catarrh with violent cough and moderate expectoration, which had defied all possible remedies, was almost entirely relieved by the use of the decoction of olive-leaves for several weeks; in another case of catarrh of the lungs, complicated with asthmatic symptoms, the drug acted as an antispasmodic and tonic. In two cases of catarrh of the bladder, copious drinking of saturated decoction of olive-leaves afforded notable relief. Dr. Derblich gave a strong decoction, with injections of the leaves, in some cases of gonorrhœa, and in a short time the pain ceased, the secretion of mucus and pus diminished, and a cure ensued in six patients in between eight to twenty-six days.

XIV. *On the Therapeutical Applications of the Thapsia Silphium.* By Professor Schroff. (Wiener Zeitschrift, 1862.)

The *Thapsia silphium* is an umbelliferous plant, the growth of which appears to be confined to the neighbourhood of the ancient Cyrenaica, in the North of Africa. The bark of the root contains, as its active principle, a resinous milky juice, confined in special vessels. Professor Schroff prepared a powder, an alcoholic tincture, an alcoholic and ethereal extract, and three watery extracts, and he made some experiments on himself with these preparations. The operation of the powder of the root and of the tincture on the external skin, consisted of appearances of irritation and inflammation, with slight itching and burning up to the higher degree of pain, and the appearance of an eruption, but without much inflammation; the operation in the mucous membrane of the mouth and the stomach was altogether similar. According to experiments made by Professor Schroff, together with those of Sigmund, Ulrich, and Endlicher, it seems that the powder of the *Thapsia silphium* acts as a drastic purgative when taken internally. Applied externally, it acts as an irritant, but exhibits no specific medicinal effect. As a drastic purgative, it stands near jalap and turbith-root (*convolvulus turpethum*), and also colocynth, but it is more powerful in its operation. It may therefore be employed in all those cases where drastic purgatives are required. But for this purpose only the powdered bark of the root should be employed, and not the alcoholic or ethereal extract, the latter of which readily excites violent gastralgia, and even gastritis. Six to ten grains of the powder are sufficient to cause several evacuations.

XV. *On the Internal and External Use of the Preparations of Iodine in Skin Diseases.* By Professor HEBRA. (Allgemeine Wiener Mediz. Zeitung, 1862.)

Professor Hebra throws some doubt upon the favourable results which are said to have ensued from the treatment of non-syphilitic skin diseases by the internal use of iodine. As regards syphilis, Hebra maintains that he has much more frequently succeeded in curing affections of the skin and bones by the internal or external use of the preparations of iodine, than swellings or hard infiltrations of the lymphatic glands. The fact that after a long internal use of iodine, many non-syphilitic skin diseases disappear, is explained by Hebra partly by the spontaneous involution of the complaint (since all curable chronic dermatoses are capable of completing their course in a longer or shorter time even without the assistance of art), and partly by the circumstance that in many persons after a long-continued use of iodine, a sudden consumption of the organic solids, disturbance of nutrition, anæmia, and emaciation supervene, and thus cause the disappearance of the skin diseases, although it is too often observed that when the re-establishment of the health follows the discontinuance of the iodine, the skin disease again makes its appearance. There is another peculiar circumstance, that although during the use of iodine, some skin-eruption, as inflammation of the hair-follicles and sebaceous glands, may supervene, this may exert no influence on the form or the cause of the existing disease, as psoriasis, prurigo, or eczema. The local application of iodine is therefore preferable to its internal administration.

Hebra considers the following preparations of iodine as the most valuable:— 1. The tincture of iodine is particularly useful in alterations of the epidermis by increase of the pigment (chloasma and lentigo). In this case it is necessary to paint the skin of the face in quick succession every four hours for three days. The brown crust which is formed generally falls off in six days, and leaves behind a colourless epidermis. The tincture is not so successful in the treatment of the small-pox pustules. 2. The iodide of glycerine, of Richter, has the advantage over the tincture, that it contains a greater proportion of iodine, and the operation of this agent is more permanent, from the fact that it remains longer in the fluid state. It is peculiarly well adapted for the removal of the new formations of lupus, and possesses the advantage of leaving a slight and smooth scar. 3. The ointment of iodide of sulphur acts like a caustic. In the treatment of acne rosacea and sycosis, the iodide of sulphur is laid on in the form of a plaster for the purpose of local cauterization. After two or three days it is discontinued, in order to allow the irritation to pass off, and then it is again applied, until the pimples and pustules have ceased to exist, and no relapse is observed. 4. The protioduret of mercury has been repeatedly employed successfully by Hebra in psoriasis, lichen, and eczema squamosum. It is rubbed in three or four times a-day. 5. The deutioduret of mercury raises a blister, and acts as a caustic on excoriated and ulcerated parts, and hence it soon destroys infiltrations and new formations. Instead of this preparation, another may be used, made by rubbing together calomel and iodine, with chloride and iodide of mercury, and lard. According to the experience of Frodsham, the ointment of iodide of mercury is a very efficacious remedy in scrofula, even in cases which resist the treatment with iodide of potassium and iodine ointment.

XVI. *On the Whey-cure, and its Indications in the Treatment of Phthisis.* By Dr. THIERRY-MIEG. (Bulletin Général de Thérapeutique, April 15th, 1863.)

Dr. Thierry-Mieg does not recommend the whey-cure in all cases of phthisis, but he thinks that it may be useful in cases where it is advisable to weaken the reactionary tendency of the circulation and innervation. The ordinary depressing and antiphlogistic remedies effect this object, but whey does the same without weakening the economy. The cases suitable to the whey-cure, according to Dr. Thierry-Mieg, are those combined with hæmoptysis, and which are active and marked by a florid appearance; in such the whey evidently produces a sedative effect and a kind of resolution of the general reactionary condition. He has found this treatment diminish fever, restore sleep, soothe the different susceptibilities of the stomach, and suppress or relieve diarrhœa. The whey-cure would probably be also beneficial in bronchitis and chronic bronchopneumonia, and also in diseases of a different seat, but in which similar conditions exist to those above alluded to.

XVII. *On the Dietetic Treatment of Albuminuria.* (Bulletin Général de Thérapeutique, March 15th, 1863.)

The researches of M. Hamon, contained in a note on albuminogenesis communicated to the Académie de Médicine on the 29th April, 1862, appear to throw considerable light upon the question whether any kinds of aliment increase or diminish albuminuria. He found, for instance, that soft-boiled eggs were very easy of digestion, and exercised a very slight albuminogenic influence, and it has therefore been suggested that in albuminuria an albuminous diet might be ordered, composed principally of soft-boiled eggs and albuminous water. Vegetable diet has also been sometimes employed in albuminous anasarca, sometimes with partial, sometimes with perfect success. But all kinds of diet that have been adopted in albuminuria are inferior to that of milk, as recommended by M. Guignier. The latter physician adopts the treatment of albuminuria devised by M. Serres, and which is founded on the combination of three plans—namely, the diminution of drink, the milk regimen, and the use of raw onions. The plan consists in choosing the milk with great care as to its freshness and quality, in substituting one milk for another when the first disagrees with the patient, allowing the patients to determine the quantity they drink, uniting lime or magnesia with the milk when it causes acidity, and abandoning the treatment at the end of twenty days, if it has not produced a decided good effect. M. Guignier believes that the diuretic property of the raw onion has a beneficial effect upon the disease, and that these vegetables ought to form part of the milk regimen. The editor of the 'Bulletin Général de Thérapeutique' thinks that albuminuria may be cured by dietetic means, if they are adopted early, when the effusions are recent, and before the albuminuria has lasted long enough to have produced in the renal tissues the organic changes which place the case beyond the hope of cure.

XVIII. *On the Properties of the Thermal Waters of Luxeuil.* By Dr. MARTIN-LAUZER. (La Médecine Contemporaine, May 15, 1863.)

Some hydrologists believe that thermal sulphurous waters possess the power of unmasking latent syphilis, and this property must be claimed for the thermal waters of Luxeuil, both on account of their thermal saline arsenical springs, identical with those of Plombières, and still more for their thermal springs containing iron and manganese. These waters are said to possess the property of putting an end to the incubation of latent syphilis in a person apparently in good health, and to compel an existing disease to assume the characters of syphilis, if the latter forms a part of it. They form also a powerful auxiliary in the treatment of venereal affections, by curing several of its complications, and by assisting mercurials and the iodide of potassium in the cure of the disease itself. The waters of Luxeuil possess a high degree of thermality. The springs are divided into two perfectly distinct groups, some ranging from 33° to 44° Centigrade in the basins, and of a composition more particularly including chloride of sodium and arsenic; and the others, less warm, from 19° to 29° Centigrade, and more particularly containing iron and manganese, together with arsenic. These waters are therefore recommended in the diseases for which chloride of sodium, iron, manganese, and arsenic are daily prescribed. Among these diseases are those of the organs of digestion, as dyspepsia, gastralgia, enteralgia, diarrhœa, constipation, engorgements of the liver and spleen, paludal and African cachexia; diseases of the urinary passages, as gravel, vesical catarrh, dysuria, and diabetes: in man, blennorrhœa, engorgements of the prostate; in women, passive hæmorrhage from the uterus, displacement and engorgement of the body and neck of the uterus, leucorrhœa, amenorrhœa, dysmenorrhœa, chlorosis, and anæmia; diseases of the locomotive apparatus, as rheumatism, paralysis, sciatica, chronic arthritis, sprains; of the respiratory organs, as chronic coryza, ozæna, humid asthma; of the nervous centres, as the consequences of cerebral or spinal hæmorrhage, and spinal irritation; in cutaneous eruptions, as prurigo and chronic eczema; in general and diathetic diseases, as the consequences of scurvy, scrofula, latent syphilis (to excite its manifestation), chorea, hysteria, hypochondriasis, intermittent fevers, &c. Dr. Lauzer observes that the public may perhaps be astonished at these manifold virtues being attributed to the waters of Luxeuil, but that their chemical composition will account for their beneficial effects upon the diseases above enumerated.

XIX. *On the Action of Tartarized Antimony on the Heart.* By Professor ACKERMANN, of Rostock. (Virchow's Archiv, xxv., 1862.)

By means of tartarized antimony we are enabled to induce different degrees of collapse by varying doses of the drug. Slight appearances of collapse accompany the first excitement of nausea, and they increase as the nausea increases. The frequency of the pulse also increases until immediately before vomiting, while its force progressively diminishes. The temperature of the peripheral parts falls in proportion as the nausea and frequency of the pulse increase. After the act of vomiting, or when this does not ensue, after the termination of the feeling of nausea, appearances of reaction begin. Injections of tartarized antimony into the bloodvessels in dogs produce results similar to those observed in man. Professor Ackermann always observed, after injection of tartarized antimony, a diminution of the force of the blood in the aorta, and this took place whether the frequency of the pulse increased or diminished, but was greater with a slow pulse, and was greatest immediately before death. The operation of tartarized antimony may be explained by reference to the experiments performed. As this drug diminishes the arterial tension and the force of the heart's movements, it retards the circulation of the blood, and hence induces a decrease of animal heat, which becomes lower in proportion to its longer and more powerful operation. This lowering of the temperature appears to be occasionally interrupted by its elevation, and the latter seems to be dependent on the contemporaneous acceleration of the breath-movements. Professor Ackermann does not deny that the vomiting and purging may also hasten the cooling of the body, but he considers that these circumstances are not necessary conditions.

The irritability of the heart is perceptibly weakened by the use of tartarized antimony. In dogs killed by the injection of this salt into an artery, the irritability of the heart examined immediately after death was remarkably diminished, and sometimes altogether extinguished. The hearts of frogs placed in a solution of tartarized antimony lost their irritability much sooner than in pure water. The diminution of muscular power was also very remarkable. The breath-movements were sometimes calm, sometimes hurried, but for the most part very slow, with hasty, forced inspiration, and very long expiration, appearances which Professor Ackermann attributes partly to the congestion of the venous system and the deficient combustion, and partly to a directly debilitating effect of the tartarized antimony on the organs of expiration. The short and spasmodic inspirations which for the most part precede the long expirations, appear to be determined by a similar spasmodic contraction of the diaphragm as is seen before the act of vomiting.

The post-mortem examination of animals poisoned by tartarized antimony showed a general congestion of the whole venous system, the venæ cavæ and the portal vein being especially filled with dark blood; the liver, spleen, and kidneys excessively congested with blood, sometimes with extravasation; the lungs not, as Majendie maintains, changed in structure and partly hepatized, but for the most part normal, or at most showing some emphysema of the margins and small ecchymoses. The large intestine was regularly contracted both in length and breadth, and there were some ecchymoses on the folds of the mucous membrane.

HALF-YEARLY REPORT ON PATHOLOGY AND PRINCIPLES AND PRACTICE OF MEDICINE.

By John W. Ogle, M.A., M.D. Oxon, F.R.C.P.
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I. *On Entozoa in the Human Brain.* By Dr. G. Rodust. (Henle und Pfeuffer's Zeitschrift, Band xv. Heft iii. p. 283, 1862.)

This paper is devoted to the consideration of, firstly, the cysticercus cellulosæ; and secondly, the echinococcus altricipariens in the brain; and contains much information respecting the literature of the subject. As regards indications during life of the presence of cysticerci in the brain, the author refers them partly to "irritation," and partly to "depression," and groups them into—1. Symptoms which arise from the act of immigration of the embryo into the brain; irritation of the cerebral fibres owing to a more or less inflammatory affection. And 2. Symptoms which arise owing to the stationary existence of the cysticerci in the brain. The latter are supposed to be connected either with the movements of the creatures,* which give origin to the appearances indicative of irritation of the fibres, or with the pressure exercised by the vesicles on the surrounding brain substance.

* See Gräfe's Archiv, Band i. p. 453.

The author suggests that possibly also the process of decay or death of the creatures may give rise to definite symptoms, just as we are apt to associate certain symptoms with calcified tubercle in the brain.

A case of ascertained cysticercus, recent as well as obsolete, of the brain in a girl of nineteen years of age, is related with all details; and also of one of the same age, in which, during life, the symptoms, which are particularized, were supposed to be referrible to the invasion of the brain by the embryo of the creature.

The author also relates, in conclusion, with all the circumstances, a case of echinococcus altriciparíens, which occurred in the Göttingen Hospital in 1861, that of a boy aged nine years.

II. *On the Cerebro-Spinal Origin and Diagnosis of the Protrusion of the Eyeballs, commonly termed Anæmic.* By T. Laycock, M.D. (Read to the Medico-Chirurg. Soc. of Edinburgh, Jan. 7th, 1863.)

After a number of observations on the history and literature, both foreign and domestic, of the above-named affection, with special reference to Dr. Begbie's 'Contributions on Anæmia,' 'Enlargement of the Thyroid and Eyeballs,' &c., the author proceeds to consider the classification, various causes, the pathological anatomy, the diagnosis, &c., of the ailment, and arrives at the following conclusions—

"1. That the exophthalmos under consideration is specially due to disorder of the nervous system. 2. That it varies in character and diagnostic significance accordingly as it is associated or not with other phenomena involving the vascular system of the heart, and of the eyes, head, and neck—the carotideal as distinct from the vertebral system of capillaries. 3. That it is sometimes of spinal, sometimes of cranial origin; and that in either case its nature and seat may be diagnosed. 4. That it occurs under a variety of morbid conditions of the nervous system."

Dr. Laycock proceeds to remark—"If it might be permitted to theorize on the causes of symptomatic exophthalmos from these data, we might conclude that, when it occurs in strangulation, it is probably due to mechanical injury to the cervical sympathetic by the tightened cord or other violent means used; in the emotional form the condition is probably like that when the sympathetic is galvanized, the face being pale, and the eye staring; in certain morbid cerebral conditions, such as mania, with epilepsy and general paralysis, the lesion is probably in the first instance paralysis of the sympathetic, and subsequently of the fifth and seventh; and, finally, that in the class of cases under consideration, when the exophthalmos is symmetrical, it is spinal; the cervical and upper dorsal region being the seat, together with the corresponding cervical and dorsal divisions of the sympathetic; but when unsymmetrical, it is due to disease of the trigeminal ganglion and branches of the fifth pair."

In connexion with this subject of exophthalmos, &c., Dr. Laycock observes —"Three years ago, my friend Dr. Rorie, superintendent of the Dundee Asylum, examined, at my request, the eyes of patients under his care, in respect to their prominence, state of pupil, and other matters. Of each sex, 86 were examined; of the females, 26 had prominent and 4 very prominent eyes, together, nearly 35 per cent.; of the males, 11 had prominent and 6 very prominent eyes; showing the considerable excess of 15 per cent. amongst the females. It is required to know, however, what is the natural proportion, absolute and relative, before we can determine how far these states amongst the insane are morbid. Changes in the pupils were also observed by Dr. Rorie, and he found that there was a difference in the two eyes. Of 7 females, the left pupil was more dilated than the right in 5, and of 8 males 6; or, in other words, the left was more dilated than the right in the proportion of 11 in 15. But then he found also that the same difference could be observed in persons apparently healthy. One only in 172 patients had sunken or retracted eyes, and he was formerly subjected to paroxysms of rotatory movements, and one, an epileptic male, had the left eye more prominent than the right."

III. *On Senile Dementia and the Differences between it and General Paralysis.* By Dr. L. V. MARCÉ. (L'Union Médicale, May 28th, 1863, p. 398.)

The following are the results arrived at by the above-named physician at the Bicêtre:—

1. Senile dementia does not constitute a distinct morbid entity. It is in reality an aggregate of symptoms pertaining to different organic affections of the brain, and notably to apoplexy and softening.

2. It is made up of two series of symptoms: those, on the one hand, of motility, which become more or less affected; those, on the other hand, of intelligence, chiefly a progressive enfeeblement, to which is superadded maniacal or melancholic delirium.

3. The disturbances of motility are always explained by organic lesions in the course or at the origin of the motor fibres. To enfeeblement of intelligence there correspond atrophy of the convolutions, fatty infiltration, and more or less complete obliteration of the capillaries of the cortical layer, and atheromatous degeneration of the cells and nerve tubes.

4. Senile dementia may be distinguished from general paralysis in most cases by the signs above alluded to. In an anatomico-pathological point of view, these two diseases have atrophy and fatty degeneration of the tubes and cells as a common terminal result. But in general paralysis this atrophy is consecutive to a plastic exudation, which, arising around the wall of the capillaries, gives rise to adhesions between the pia mater and the cortical layer, diminishes the calibre of the vessel which it compresses, and obstructs the circulation of the blood. In senile dementia, on the contrary, the obliteration is consecutive to atheromatous deposits, which arise spontaneously in the capillaries, in consequence of advance of age and diminution of assimilative force.

IV. *On the Nature and Cure of Progressive Muscular Atrophy.* By Dr. H. J. GUTHZEIT. (Dissertatio Inauguralis Medica, &c., 1862. Berolini.)

In this *brochure*, the history, pathological anatomy, symptoms, causes, diagnosis, prognosis, and cure of the disease are very briefly treated of. Two cases of the affection are related as observed by the author,* in which cure took place under Professor Remak. As to remedial agents, he observes:—"Omnis cura tam interna quam externa, *si electricitatem exceperis*, adversus musculorum progressivam NIHIL profecit;" and goes on to say that even electricity, as advised by Duchenne, "forma rivi inducti," "non habuit successum constantem."

V. *On the Double Pulse and Double Heart Sounds.* By Prof. SKODA. (Allg. Wien. Med. Ztg., viii. 34, 1863.)†

The author observes that the reduplication of the pulse is not a phenomenon indicative of much danger. It is not connected with the heart's movements,

* See, also, the Oesterreich Zeitschrift f. prakt. Heilk. Wien, 1862.

† Schmidt's Jahrbücher, 1863, No. 4, p. 28.

but is for the most part a local manifestation, most likely arising from a change in the fixed position of the artery.

Reduplication of the heart's sounds is most readily explained on the supposition that the elements of the sound (*ton*) on both sides of the heart are not contemporaneous, as they ought to be, the aortic sound being produced at the end of the ventricular sound, so that between both of these a short period is perceptible, the sound being, as it were, split up (never properly redoubled); or if the sounds are doubled, the two ventricles do not contract at the same time, or the right one contracts twice while the left only contracts once.

The reduplication of the second sound is ordinarily produced by the reflux in the aorta and pulmonary artery not being contemporaneous; and in most cases this is the result of an augmentation in the tension of the pulmonary artery, by which a division or splitting of the second sound is occasioned. Such a splitting also appears to be possible in a single artery (aorta or pulmonary artery), owing to an unequal elevation of the valve flaps. A reduplication of the second sound may be produced by the second sound in the ventricle and heart in the arteries not being contemporaneous, the second sound, for example, being produced first in the pulmonary artery, and then in the ventricle. Another form of reduplication of the second sound may be produced by the heart's movements when exudation really exists in the pericardium, as in the beginning of pericarditis.

VI. *On Transitory Insufficiency of the Heart's Valves.* By Professor SKODA. (Allg. Wien. Med. Zeitung, viii. 1-20, 1863.)

A case of typhus is related, in which paralysis of the substance of the heart, especially of the musculi papillares, was diagnosed during life. The heart was found to be enlarged, and a systolic murmur and intensification of the second sound at the pulmonary artery was audible. During the patient's illness, the systolic bruit became less, the cardiac dulness diminished, and the apex of the head less inclined to the left side. After death the heart was found to be flaccid and very lacerable; the valves were natural. The author describes such temporary insufficiency of the heart's valves (specially of the mitral) as existing in the commencement of febrile diseases such as typhus, small-pox, scarlet fever, and also a dilatation of the heart, owing to transitory paralysis of its substance. Similar transitory paralysis of the musculi papillares is described by Skoda as occurring in ailments devoid of fever, as in hysteria, chorea, and epilepsy. Palpitations and dyspnœa exist, but rapid recovery occurs.

VII. *On the Diagnostic Value of an Accentuated Cardiac Second Sound.* By J. WARBURTON BEGBIE, M.D. (Edinburgh Medical Journal, June, 1863, pp. 1067.)*

After certain preliminary remarks bearing on the subject of his paper, respecting the constitution of the second cardiac sound and the conditions both of health and disease under which it is obscured, the author passes on to remark upon the "accentuated" second sound, one which he designates as being "booming" or having a "ringing boom," and which he describes as being heard in connexion with one or other of two conditions of disease—aneurysm of the aorta, and dilatation with an atheromatous state of the aorta. In this consideration, the accentuated second sound of the pulmonary artery, and also that which exists in some cases of hypertrophy and dilatation of the left ventricle, are excluded. When this accentuated second sound exists, of course the semi-

* Read at the Hunterian Medical Society, March 27th, 1863.

lunar valves are presumed to be competent, as if not so, a diastolic murmur would be originated. Dr. Begbie details two cases in which a loud ringing or booming second sound was conspicuous at the base of the heart associated with aneurysm of the aorta, one in the transverse, and another in an earlier part of the aorta. He also makes allusion to others of a similar nature, and observes that to distinguish between the cases in which this peculiar cardiac sound is due to aneurysm, and when to aortic dilatation, is not always easy. He observes:—"Reliance is chiefly to be placed on the associated physical signs in the former case, more particularly prominence, pulsation, extended percussive dulness, and the signs of internal pressure. If atheromatous dilatation exist, and that is the special condition, independent of aneurism, which gives rise to the accentuated second sound, there will probably be more or less pulsation in the jugular fossa, atheromatous condition of superficial pulse (radial, temporal arteries, &c.), noticeable, and probably the arcus senilis." Dr. Begbie proceeds to observe that the following appear to be important points in endeavouring to explain the mechanism of an accentuated second sound under the circumstances considered.

"I. The condition of the vessel both in cases of aneurysm and of dilatation with atheromatous degeneration being such as greatly to diminish, if not to destroy the support given to the circulation by the artery, there results an increased recoil of blood on the closing or closed valves.

"II. It is possible that a morbid condition of the valvular apparatus itself heightens or intensifies the sound. The valves are not incompetent, but in such cases they are sometimes found thickened, and even presenting a hard surface in parts.

"III. Something may, I conceive, be due to the increased calibre of the vessel, in connexion with the altered condition of its internal tunic, in causing the peculiarity of sound."

Both in aneurysm and in dilatation, the accentuated second sound is most readily appreciated over the aortic valves, but in dilatation it appears of more decided character over the manubrium sterni.

VIII. *On Albuminuria.* By B. J. Stokvis. (Nederl. Tijdschr. v. Geneesk., vi. 25, 26, July, 1862.*)

After a somewhat lengthy discussion of the subject, which we will not enter upon, the author concludes that albuminuria may exist independent of disease of the kidneys; and, indeed, that it is not dependent on a mere attenuation of the blood or perverted innervation. He supposes it to result from a change in the albumen of the blood, by which, as regards its capacity for diffusion, it becomes more or less like the albumen of the egg. This is in accordance with the view expressed by Mialhe in 1852,† and is supported by experiments upon dogs and rabbits. Whether, in addition to this change in the albumen, other causes of albuminuria exist, independent of disease of the kidneys, the author leaves as a matter of doubt.

IX. *On the Normal and Pathological Histology of the Kidneys.* By V. Rasmussen. (Translated from the 'Bibliotek for Læger,' for April, 1862.‡)

Passing over the normal histology, which the author goes into at considerable length, of the renal vessels, the renal parenchyma, and the interstitial

* Schmidt's Jahrbücher, 1863, No. 4, p. 40.
† L'Union Médicale, p. 361.
‡ By W. D. Moore, M.D., of Dublin.

connective tissue, we proceed to notice the pathological changes in the kidneys referable to the term "Morbus Brightii," as they are described.

The author prefaces his remarks on this subject by observing—"In studying renal diseases, the object of our investigation is to establish the three following principal points: 1. What tissue or tissues are affected (vessels, parenchyma, or interstitial connective tissue); 2, whether only the cortical substance or the pyramids are attacked, or both together; and lastly, 3, whether the affection is partial or diffused. In the commencement a definite tissue can always be indicated as the starting-point of the affection: later this is most frequently not possible; the several tissues are dependent on each other, so that they are often consecutively attacked; but, on the other hand, the several lobuli possess also independence of each other, and we have already seen that the vascular system of the cortical substance and that of the pyramids are to a certain extent independent of one another. It is especially chronic affections of the kidney which will be the subject of our consideration in an anatomico-pathological point of view. Formerly these were comprised under the name of 'Morbus Brightii,' but this is a very inaccurate denomination, and one which conveys but little information. The older investigators properly included under this term only what we now call parenchymatous nephritis, whose terminal stage is the characteristic and striking granular atrophy. Although this form is by far the most frequent, modern researches have revealed other pathological changes in the kidneys, which clinical physicians have not yet succeeded in definitely diagnosing from the parenchymatous nephritis; and it becomes, therefore, necessary to refer to these also the designation 'Morbus Brightii,' so far as such a name shall be retained as a common denomination for these extremely different conditions. The essential symptoms are the albuminuria and the diminished secretion of urine, while the so-called fibrin cylinders have not the signification which Frerichs ascribes to them; they occur, at all events, only in the parenchymatous nephritis, and not even constantly in that. Here are three essentially different affections to be considered, each occupying its own tissue—1. The amyloïd degeneration of the kidney; 2, the parenchymatous; and 3, the interstitial nephritis. They may complicate one another; nay, all three may be present at once; sometimes one, sometimes another occurs first, but the parenchymatous is most frequently the primary affection."

Rasmussen then proceeds to consider the above-named three affections.

The amyloïd degeneration can only VERY RARELY be recognised without having recourse to the chemical reaction (i.e., by the microscope alone), owing to the limitation of the disease to the vessels, to the exclusion of the parenchyma, and also to the fact that the disease is most frequently combined with parenchymatous or interstitial nephritis, or the interstitial fatty kidney, especially in people who have died from the dyscrasia of syphilis or of the mercury by which it may have been treated. The author observes, that in the amyloïd kidney we may or may not have a lardaceous or waxy appearance, that in very extreme cases of degeneration we may almost certainly detect this amyloïd condition by the glomeruli assuming a whitish-grey, shining, enlarged, and prominent character, which appearance is also influenced by the "vasa afferentia," and other neighbouring arteries. As regards the vessels which are affected, it is asserted to be the small vessels, and in this order: "first and foremost, the glomeruli and vasa afferentia, next the vasa efferentia and the capillaries in the cortical part, and finally, the 'arteriolæ rectæ;'" rarely are the large vessels affected, and it is long before other tissues become so.

Owing to the peculiar deposit affecting the vessels and glomeruli, the supply of blood becomes diminished, the cortical substance anæmic, while the hyperæmia increases in the pyramids, and hæmorrhage occurs at times owing to increased pressure on the inelastic vascular works, giving rise to reddish or brownish streaks or spots. This thickening of the vessels, &c., from amyloïd is not to be confounded with the thickening which results from a change corresponding to the "so-called end-arteritis," by which organization of newly-formed elements and subsequent atheromatous and fatty degeneration is produced, chiefly, indeed, in the larger vessels, but at times affecting the glomeruli. This fatty degeneration commences with an increase of nuclei of the capillaries, which divide, become separated, and thus elongate the loops in the glomeruli without increasing their calibre. If the process advances, small fatty particles accumulate around the nuclei, and increase with disappearance of the nuclei. Thus a whole glomerulus may degenerate, and the same result be produced as if we had amyloïd degeneration. Such a fatty degeneration of the glomeruli may, microscopically, simulate amyloïd, but the reaction and the microscopic appearances soon discover the difference. The author gives in detail the best method of obtaining the chemical reaction of amyloïd.

As respects the "*parenchymatous nephritis*," the author, after alluding to the relation of the epithelium of the renal canals to the urine, and to the fact that the cells in the convoluted tubes are larger and richer in albumen than those in the straight ones, points out that any disease of the former rendering them inactive will be of more serious import than disease of the latter, producing an actual change in the urine; and in consequence of this distinction, he establishes two forms of parenchymatous nephritis, the "papillary catarrh," or catarrhal nephritis, and the *proper* parenchymatous nephritis.

The Papillary Catarrh, situated in the straight canals and papillæ, and comparable to the bronchial catarrh, is often continued from the bladder or urethra, but may be caused by external agents, as the use of cantharides, acid diuretics, and alcoholic drinks. It is often complicated with parenchymatous nephritis, and may be the starting-point for it. Post-mortem examination shows the affected canals and papillæ to be attended by a whitish or yellowish striation, with hyperæmia of intervening vessels; and when the disease is owing to internal remedies, hyperæmia and bloody ecchymosis over the whole kidney exist. If the disease continues long, the distended urinary canals press on the bloodvessels, and thus the hyperæmia ceases. The disease is mostly limited to an abundant and varied production of cell-growth (nucleated, club-shaped, or fusiform, and, it may be, ramifying), mixed with mucous catarrhal products; but a process may exist, as in the acute forms, like that of the proper parenchymatous nephritis, with fatty metamorphosis and destruction of epithelium.

The Proper Parenchymatous Nephritis is described (after Virchow) as an hypertrophy of the cells of the convoluted canals, which take up large quantities of the albuminates, become distended, turbid, granular, and adhere closely together; subsequently the cells vanish, and the granular fatty mass becomes free, forming the "inflammatory globules." The author describes three stages of the affection (which he parallels with pneumonia), which may all be going on simultaneously, and delineates the anatomical characteristics of each one. This affection often coëxists at the later stage with intestinal nephritis. In the second stage (that which, when papillary catarrh exists, as nearly as possible constitutes "Bright's kidney"), the retardation of the venous blood is described, and the consequent formation of thrombus, and the continuation thereof to the vena cava and heart, and also transmission into the lungs. The third stage described is, in fact, a resolution or recovery, and corresponds to the complete fatty metamorphosis of the cells, generally, but not always, with loss of substance, induration, granulation, and formation of cysts in connexion with the urinary canals. The interspaces between the granular elevations of the surface are ascribed to the empty collapsed canals, which, owing to their pressure on the vessels having ceased, are often of a reddish colour; and the author points out that this granular atrophy is not analogous to cirrhosis of the liver, inasmuch as it is the parenchyma itself which is first affected, and only subsequently complicated with intestinal nephritis; whereas in the case of the liver it is not the hepatic cells which are first affected, but the inter-acinous connective-tissue. In the third stage the glomeruli are described as generally small, corrugated, surrounded by thickened capsules of connective-tissue, and possibly (as also the epithelium) in a fatty state, sometimes amyloïd, and sometimes calcareous.

In the *intestinal nephritis*, the change in the intestinal connective-tissue may preferentially affect the intercellular substance, which becomes hypertrophied, whilst the cells only become slightly increased in number, though they become larger, or the cells may multiply by frequent subdivision, whilst the inter-cellular substance is not much increased; and if this condition is very extreme, suppuration is the consequence. A third but rarer result is the interstitial fatty kidney, when the newly-formed connective-tissue passes into fatty degeneration. In the first and lower degrees of the second form, the connective-tissue contracts around the canals and glomeruli, and the circulation is more or less obstructed; the interspaces become increased, the urinary canals slender and sometimes constricted in a bead-like manner, and the tunica propria is often thickened and streaked; and the glomeruli are seen small, homogeneous, and in a more or less fatty state. Other interstitial changes, which might be mistaken for the above, may arise from venous stages in the kidneys, in diseases of the heart, or from increase of the capillary nuclei, which may be mistaken for the nuclei of connective-tissue. The author specially mentions a form of interstitial nephritis affecting the pyramids, or circumscribed (syphilitic), in which depressions and cicatrices form not unlike those from hæmorrhagic infarctions.

In the *interstitial fatty kidney* (which is rare) the organ is large and flaccid, and is full of yellowish or whitish striæ and marks, and there is often amyloïd or parenchymatous nephritis; the urinary canals are of diminished calibre, and separated far by fatty masses; the glomeruli, and generally the walls of the vascular cells are fatty or amyloïd.

As respects the albuminuria of chronic renal affections, the author supposes that the albumen is transuded from the intestinal capillary network, owing to the increased lateral pressure, as especially when the afflux of blood is arrested; for example, when the renal vein is tied. He supposes, however, that to a certain degree the albumen may be eliminated in the glomeruli or from the large albumen-holding epithelial cells.

Other so-called FIBRIN CYLINDERS are not to be looked on as an inflammatory product. Their origin is obscure, being found chiefly in the straight tubes and the pyramids, more rarely in those of the cortex, and scarcely at all in its convoluted tubes; also often in the constrictions and small cysts. They scarcely ever consist of fibrin, but are analogous to the so-called colloïd mass. Those occurring in papillary catarrhs are formed of mucin. The author seems inclined to look on these cylinders (with Key and Virchow) as dependent on changes in the albumen of the epithelial cells.

X. *Fatal Obstruction of the Bowels from the Pressure of an Hydatid Tumour springing from the Mesentery.* By J. SUTHERLAND, M.D. (The Medical Record of Australia, February, 1863.)

The patient, aged forty-eight, enjoying general good health, except a feeling of fulness of the epigastrium, was suddenly seized with severe cutting pains around the umbilicus, and constant desire to go to stool without effect, succeeded by troublesome vomiting and tympanitis of the abdomen. The pain continued, and tenderness on pressure came on, with furred tongue, bounding pulse, and hot skin, &c. In spite of venæsection and the use of leeches to the abdomen, &c., hiccough, subsultus tendinum, delirium, and coma came on, followed by death. On post-mortem examination, instead of invagination of the bowels or intussusception, which were expected, a very large oblong hydatid tumour (of the size of an adult human head and weighing 4 lbs.) was found in the left hypochondrium and umbilical region, which compressed several folds of the ileum so closely as to prevent the possibility of the passage of the contents, inducing gangrene in the parts compressed, and intense peritoneal inflammation in the adjacent convolutions. The tumour was adherent by a small root or pedicle to the left side of the mesentery, having an artery and vein enclosed, and traceable over the interior of the lining membrane of the sac or tumour. The reporter observes that had it been recognised during life, it could easily have been punctured and the fluid drawn off.

XI. *On Purulent Infiltration of the Walls of the Stomach.* By M. Raynaud. (Bulletin de la Société Anatomique de Paris, tome vi.)*

Raynaud has collected twelve cases of this affection, omitting those instances of small abscesses at times met with in the mucous membrane. A specimen presented by M. Cornil stands as a good type of the rest. The stomach is thickened, the mucous membrane injected in parts and softened, and in two places presents ulcerations of the size of a franc piece, only including the mucous membrane. A section of the stomach's wall is from half a centimetre to one centimetre in some parts. Beneath the mucous membrane a uniformly extended layer of concrete pus existed, infiltrating the meshes of the areolar tissue. M. Raynaud called attention to the fact that in these cases there was almost always extensive concomitant lesion, apparently indicating a general affection, such as pleuritic effusion, purulent pericarditis, grey hepatization of the lungs, &c. This affection of the stomach exists more frequently in men than in women, always in people of middle age. Its causes are very obscure.

As regards symptoms, they are more general than local; chiefly more or less generalized symptoms of peritonitis; vomiting, anxiety, small pulse, &c.; almost always interference with the mind, as violent delirium and extreme prostration. The condition is very like that which characterizes severe fevers, along with a marked tendency to the formation of pus in different organs.

XII. *On Tropical Dysentery.* By J. Ewart, M.D. (The Indian Annals of Medical Science, No. 16, April, 1863, p. 403.)

This paper is a long one, and forms part of a 'Review of the Treatment of Tropical Diseases.' We have not space to do more than to allude to the vast amount of material collected in this communication, beyond noticing what the author says of the "ipecacuanha" treatment:—"When the ipecacuanha fails to preserve the life of the patient, its failure may generally be attributed to,— 1. Coëxistence of the abscess of the liver. 2. Unchecked aguish or malarious poisoning. 3. Irretrievable constitutional cachexia. 4. Addison's disease. 5. Bright's disease. 6. Phthisis or tuberculosis. 7. Strumous diseases of mesenteric glands. 8. Permanent enlargement of spleen or liver. 9. Peritonitis with or without perforation of the gut. 10. The existence of extensive sloughing.

Dr. Ewart, proceeding to observe on the "advantages" of the ipecacuanha treatment in the congestive, exudative, and ulcerative stages of almost every

* Archives Générales, Dec. 1862, p. 732.

form and type of acute dysentery, as well as in the acute attacks supervening upon chronic dysentery, describes them as consisting of:—"1. Its simplicity. 2. Its safety. 3. Its certainty compared with any other method. 4. The promptitude with which the inflammation is stopped. 5. The rapidity with which recovery takes place. (*a*) By resolution. (*b*) By granulation and cicatrization. 6. Conservation of the constitutional powers. 7. Abbreviation of the period required for convalescence. 8. Decrease in the frequency of chronic dysentery. 9. Decrease in the frequency of abscess of the liver. 10. Diminution of mortality to cases treated, all of which are accomplished,—(*a*) Without local or general blood-letting. (*b*) Without salivation. (*c*) Without calomel and irritating purgatives. (*d*) Without opium by the mouth.

"The objections which have been advanced against large doses of ipecacuanha in dysentery are *first* its 'depressing influence' kept up by nausea and vomiting; and, *secondly*, that it is liable to set up 'uncontrollable vomiting.'

"*1st Objection.*—The depressing power, nausea, and vomiting, have all been over-estimated. Nausea is only a temporary and evanescent effect. Vomiting is an exceptional occurrence, and even when it does supervene, it seldom lasts long. As much nourishment, therefore, as may be required to support the strength, can be allowed in the intervals between the large doses of ipecacuanha. But what contributes more to the conservation of the patient's stamina, and to the prevention of depression or asthenia, is the speedy cessation of the dysenteric process accomplished by the drug, followed by refreshing sleep and the power of digesting and assimilating nourishing food. Such remarkable results as these soon reconcile any patient suffering from dysentery to an otherwise disagreeable remedy.

"*2nd Objection.*—When uncontrollable sickness and vomiting succeeds the employment of this medicine in the manner already described, the existence of one or other of the serious diseases enumerated above may be more than suspected. In the absence of these complications, unmanageable vomiting is seldom, if ever, witnessed. Hence, in a preponderating majority of the cases of dysentery, observed in this country, this objection is untenable. The truth is that every physician who has used ipecacuanha in heroic doses soon learns that depression of the vital powers is not to be feared, and is surprised at the small amount of vomiting that follows its administration, and at the unexpected ease with which the stomach tolerates its presence."

XIII. *Syphilitic Affection of the Liver.* By Dr. F. KEESBACHER. (Wien. Med. Wochenbl., quoted in Schmidt's Jahrbücher, 1863, No. 4, p. 42.)

A patient in whose neck ulcers had healed under the use of mercury, experienced vague rheumatic pains, bloody and discoloured discharge from the nostrils, discoloration of the skin, and a feeling of weight in the right hypochondriac region along with debility, loss of flesh, and fever towards evening. The liver was found much extending below the ribs, especially towards the stomach, where it was felt as very hardened, and affected by a hard immovable knot of the size of a fowl's egg, as well as by a second one of a smaller size. The liver, as also these projections, were free from pain on pressure. All other organs appeared to be natural, and no jaundice existed. The diagnosis was established as follows:—Supposition of hyperæmium of the liver was excluded by the partial increase of size, a nutmeggy state, also by the absence of heart or lung affection, and of jaundice. Hydatids and abscess were excluded by the want of any fluctuation. In spite of a denial of primary symptoms, the disease of the liver was conjectured to be syphilitic, and in nine weeks, under the use of iodide of potassium, in increasing doses, the aspect became natural, pain removed, and the liver was restored to its natural size, in spite of an intercurrent periostitis of the frontal bone.

XIV. *On Specks upon the Cornea.* By M. De Pietra-Santa. (Académie des Sciences: see Archives Générales, December, 1862.)

Regarding the causes of these, the following are the conclusions arrived at:

1. The causes of these specks are abnormal secretions of the conjunctiva, which, under the form of filaments or of molecules of pus or mucus, are deposited on the portion of the cornea which is exposed.

2. The molecules of pus or mucus may lodge in the thickness of the cornea by imbibition, even before the latter ulcerates.

3. The specks of the cornea are formed most frequently on the centre, owing to the mechanical action of the lids, and to the slowness of regeneration of the cornea at this part.

4. The specks are often formed at those points of this membrane which are in more immediate connexion with the abnormal secretions of the conjunctiva.

5. They are more apparent in proportion as the substances which penetrate the cornea are more solid.

XV. *On the Trichina in the Human Body.* By Dr. C. Laseque. (Archives Générales, December, 1862, p. 716.)

This communication embraces a short digest of most of the contributions on the subject.* We can only devote space for the enumeration of the results gathered from their consideration, which are as follows:

I. That the trichinæ are not so inoffensive as was at first supposed, and the observations on man and experiments on the lower animals seem to contradict the conclusions put together by Davaine in the following words,—viz., "That individuals in whom the trichinæ have been found have experienced no pain or particular symptom owing to their presence; the existence of the trichinæ appears to be free from every inconvenience, for they are not reproduced in the muscles affected by them, and they always die without having undergone development."

2. That the symptoms noted in the patient whose case was detailed by Friedreich (for the particulars of this case we refer our readers to the January number last of this Review, p. 255), correspond with those observed in the only three cases in which the trichina has been noticed during life.

3. That the instances of infection are more numerous than has hitherto been imagined—at least, in certain regions.

4. That the kind of food exercises a very considerable influence, infection by the trichinæ being essentially owing to the use of food affected by these parasites.

XVI. *Double Vision with each and both Eyes.* By Dr. Bethune. (Boston Medical and Surgical Journal, Feb. 12th, 1863.)

The following is the description given by the reporter of this case:

"Mrs. ——, aged thirty-seven, the wife of a physician, who had formerly taught in one of the highest of our private schools, consulted me first on the 10th of March, 1862. Naturally far-sighted, and without previous trouble in the eyes, eighteen months ago, after unusual exertion in studying, she was seized with double vision with each and both eyes. She suffers no pain, photophobia, or other subjective symptom. This diplopia does not extend to very near objects, the nearest point being from four to five feet. The new moon she

* It comprehends a review of the observations of Hilton, Owen, Wood, Farre, Luschka, Bristowe and Rainey, Henle, Virchow, Zenker, Leuckart, Davaine, Turner, and Friedreich.

describes as seeing with four horns. The false image seems always to *the left*, except in the case of horizontal lines, as, for instance, a telegraph wire, when it appears *below*. The appearances are the same with either eye covered. By bending the head, she *thinks* to the *left*, the two images coincide. She thinks there has been no variation in the double vision since she first observed it, *when the circumstances are the same*. She observes it more when looking towards the sky, and in clear light, than when the light is weaker.

"She is rather a delicate-looking person, though her general health is good. Her hands and feet are apt to be cold. About six months ago she was attacked with tinnitus of both ears, which still continues.

"On examination, the only abnormal appearance in the eyes is a little tinea ciliaris. She was advised to rest the eyes, to apply a lotion of acetate of lead to them, and a stimulating lotion to the spine. To take sherry wine, and exercise in the open air up to her strength.

"*Jan.* 17*th*, 1863.—Was again seen to-day. No change in eyes. Other things as above, except that the tinnitus has diminished. The left pupil was dilated with atropine, and the eye examined with Burow's ophthalmoscope. *Nothing abnormal seen.* Two repetitions of this examination, on different days, were made with the same result."

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *On Traumatic Detachments of the Skin.* By M. Morel-Lavallee. (Archives Générales, 1863, tome i., pp. 20, 172, 300.)

Several years since, M. Morel-Lavallée published a memoir upon *traumatic effusions of serosity;* but as such effusions are only the consequence of a primary lesion, he designates these his additional observations upon the subject by the name of the lesion in question. The detachment (*décollement*) of the skin from the subjacent tissues here described, is produced by the oblique direction in which a vulnerant body of large surface impinges against the integuments, dragging them with it, and separating them from their subjacent connexions. In the great bulk of the cases, the wheels of carriages have been the operating cause; and the skin is sometimes thus detached to an incredible extent, with little or no external mark. When the number of blood and lymphatic vessels which are ruptured in such an accident is considered, a speedy effusion into the cavity thus created might be anticipated; but no effusion is, in fact, so slow in formation, or so small in quantity, this being almost imperceptible sometimes at the end of several weeks. Some days after the accident fifty grammes of fluid will scarcely be found in a cavity capable of holding more than ten litres. In most cases this fluid is not blood, but serosity, either quite limpid or only reddened, an abundance of blood-globules being quite exceptional. In general, there are many fatty globules floating on the surface. The most important feature of the collection, however, is, that it never fills its receptacle. The symptoms of the affection are mostly dependent upon this small amount of effusion, the slight tumefaction present being frequently imperceptible to the inexperienced eye. The fluid is displaced by changes of attitude, trembles on percussion, and can by pressure be pushed back, so as to enable the limits of its receptacle being accurately ascertained. In very large detachments of the skin, it is sometimes necessary to force the fluid to where the skin is thinnest, when a slight undulation following percussion betrays its existence. The skin, deprived of a portion of its vessels and nerves, is generally paralyzed. The diagnosis of these detachments may be of importance. Thus, when a fracture in the neighbourhood calls for amputation, if the detachment be overlooked the incision may be carried through integuments which will certainly become gangrenous. Slow as is the formation of the effusion, its tendency to diminish is slower still. It has, indeed, a tendency to become encysted, and its spontaneous disappearance would have to be long waited for. When the lesion is a simple one it readily yields to proper treatment; but when it is of excessive extent, the patient usually sinks stupefied, as after other severe injuries. Penetration of air, through a wound or an eschar, into these cavities, when large, usually gives rise to putrefactive suppuration; but when the detachment is only a small one, much inconvenience does not arise from this. In 3 of his 50 cases the author has met with intercurrent erysipelas, which, although it induced suppuration in the sac, only retarded the definitive cure. In simple cases, the treatment consists in evacuating the fluid by means of an exploratory trocar, the application of a flying blister and elastic compression. The blister excites absorption, and the compression keeps the walls of the sac closely in contact; and when the effusion is not too considerable, these two means may suffice without puncture. When the sac is opened by a penetrating wound or the fall of an eschar, the aperture, if small, should be closed; but where this is too large, or where the lips of the wound are too irregular to admit of immediate union, the flow of fluid must be favoured either by enlarging the aperture, a dependent position, or the formation of a counter-opening.

II. *On Naso-Pharyngean Polypi.* By M. Michaux. (Bulletin de l'Acad. de Méd. de Belgique, tome v., p. 679.)

M. Michaux, in this paper, details several cases, the subjects of which were exhibited to the Belgian Academy, periods varying from fifteen years to seven months having elapsed since the performance of the operations without relapse. His conclusions are—1. That in the case of large polypi, having strong and broad insertions and numerous branchings out, of all procedures the total resection of the superior maxillary, followed by tearing away, excision, scraping, and cauterization, is that which is to be preferred. This resection, with or without that of the malar bone, completely exposes the insertions of the polypus, and enables us to apply the actual cautery immediately after the operation, and to repeat this application at any points where reappearance occurs. It is not a dangerous operation, and is attended with little deformity. While exposing the bone prior to its removal, we should endeavour to preserve its periosteum. 2. If the polypus is small, and its mucous adhesions are feeble, tearing away, or tension may be tried. 3. If the pedicle of such a polypus is thin, but resisting, and does not yield to traction or torsion, the extemporaneous ligature of Maisonneuve or linear *écrasement* may be tried. 4. Excision of polypi is an insufficient measure, and risks the production of a hæmorrhage which it may be difficult to arrest. If a polypus of medium size, without extensive insertions, cannot be torn away or tied, we must reach it by opening a passage by excision, or partial excision, of the velum palati. Still, operations whether by the natural passages or by partial resections, are exceptional occurrences rarely met with in practice.

III. *On a Lesion of the Conjunctiva coinciding with Hemeralopia.* By M. Bitot. (Bulletin de l'Acad. de Med., No. 14.)

In a recent report on hemeralopia, M. Gosselin noticed the fact of slight blepharitis or conjunctival catarrh being connected with the night-blindness assisting to explain its epidemic character, its persistence in the same regiments, and its recurrence in the same men. In the present paper, M. Bitot indicates the coincidence of hemeralopia with a lesion of the conjunctiva, occupying not the lids, but the globe of the eye, and not exhibited by signs of inflammation, but by an assemblage of shining white spots, producing a pearly or silvery spot by the side of the cornea. The author has made his observations in twenty-nine cases occurring in the Bordeaux Children's Hospital, nineteen being males and ten females; the ages varying from nine to nineteen years, these children being employed as tailors, shoemakers, and dressmakers. The lesion has been always found near the lateral part of the cornea, generally on the external side. The spot, of a pearly or silvery appearance, seems constituted of an aggregate of minute points, and may vary in form not only in different individuals, but in the two eyes of the same person. In general, it is triangular, having its somewhat concave base turned towards the cornea. The form is susceptible of undergoing change when pressure is made on the eyelids, the parts constituting the spot being simply in juxtaposition. The extent of this spot is proportionate to the completeness of the hemeralopia, and at the commencement of the disease only a few pearly points are visible. In some cases these have furnished the first indication of the approaching hemeralopia, the patients not being aware then of any disturbance of their vision. The course of the spot follows that of the hemeralopia, increasing in size as this becomes more complete, and diminishing slowly or rapidly, according to the rapidity of the cure. Not a trace of the spot remains visible when the normal vision has become restored. The existence and duration of the spot thus become a measure of the principal disease; but before concluding this to be the case, M. Bitot examined the other children of the establishment, in order to ascertain whether its presence might not be a mere coincidence due to the lymphatic or scrofulous constitution so prevalent there. On examination, however, the subjects of hemeralopia were found to be some of the most healthy children, while cases which exhibited marked scrofula, independently of hemeralopia, never manifested the spots in question. These spots cannot be detached by the finger-nail, but seem to consist of epithelial layers. The conjunctiva situated between them and the external angle of the eye, loses its normal characters. It is less moist, soft, shining, and pliable, and pressure made by means of the eyelids, exhibits a very clear line of demarcation between its changed and healthy portions.

[M. Villemin, an army-surgeon, states,* without having been aware of M. Bitot's investigations, that he had met with this white spot in an epidemic of hemeralopia occurring in a battalion stationed at Strasbourg in 1860.]

IV. *On Vulcanized Caoutchouc Catheters.* By M. Nelaton. (Gazette des Hôp., No. 37.)

In a clinical lecture, M. Nélaton called attention to the case of a man who had suffered from retention of urine due to enlarged prostate, as exhibiting the advantages of the vulcanized caoutchouc catheters. The ordinary gum-elastic catheter sometimes causes false passages, and especially when the patient uses it himself hastily, pressed by his desire to pass water. But the extreme suppleness of the vulcanized caoutchouc enables the instrument to follow all the sinuosities of the canal, without lacerating the mucous membrane. Then, again, when the instrument has to be left in the bladder, the rigidity of the ordinary gum-elastic catheter gives rise to an uneasy sensation, amounting to pain upon the slightest movement being made, an eschar or perforation of the bladder occasionally resulting from the too-prolonged pressure of the point. The portion of the caoutchouc catheter, on the contrary, which remains within the canal, yields to the influence of the contraction of the bladder. The patient,

* Gazette Hebdomadaire, No. 21.

too, can go about, or even travel, having this catheter in the urethra. The tissue of the gum-elastic catheter, moreover, undergoes change after being for some days in contact with the urine, and incrustations are deposited on its corroded surface; but in the present case, the patient had worn a vulcanized caoutchouc catheter for twelve days, and after washing, it was as clean as on the first day.

V. *An easy means of Reducing a Dislocated Humerus.* By Dr. GARMS. (Archiv. der Heilkunde, 1863, No. 2.)

Dr. Garms describes the following modification of Cooper's procedure. The patient is laid upon the floor, not on his back, but on his belly, some cushions intervening. A towel is attached to the humerus above the elbow, and another, passed around the upper part of the humerus, is given into the hands of the assistant, standing on the side of the dislocated arm. The operator, sitting down on the floor, on the same side, lays hold of the lower towel, and applies the heel of the foot lying nearest the patient to the axilla. He makes extension backwards and downwards, while the assistant draws laterally. The dislocation is thus reduced with surprising facility, the agency of chloroform not being required. The advantage of this modification is that extension backwards may be far more easily executed than when the patient is in the supine position; and this is the direction required in dislocation forwards, which prevails in the great majority of cases. For dislocation backwards, which is very rare, Cooper's procedure is the best.

VI. *On the Performance of Tracheotomy in Children.* By M. GIRALDES. (Bulletin de Thérapeutique, No. ix.)

M. Giraldès is of opinion, that the rules laid down for this operation in surgical treatises are not explicit, and that the great variety of instruments which have been proposed tends rather to increase than to remove difficulties, and to confuse the mind of the operator. Ingenious in their construction, they seem capable of fulfilling every indication, and of enabling hands, however inexperienced, to perform the operation without much difficulty. Most of these inventions testify rather to the ingenuity than to the experience of their constructors. For its rapid execution, tracheotomy requires none of these special instruments—a convex bistoury, slightly pointed, a dilating forceps, and two blunt hooks, constituting all the necessary apparatus. The canula in croup is indispensable. The following rules for the operator may be laid down:—

1. *The Position of the Patient and Assistants.*—This is a very important preliminary, embarrassment and difficulties sometimes resulting from the faulty manner in which the patient has been placed and maintained. The child should be laid on a mattrass placed upon a table, having his neck supported by a bolster, and his head thrown forcibly backwards, an assistant kneeling down behind, supporting it firmly in this position by placing his hands over the jaws. Another assistant should fix the shoulders so as to prevent the slightest movement. The patient is thus maintained immovable, and there are none of the oscillations of the trachea which various instruments have been contrived to prevent.

2. *The Operation.*—The operator, standing on the right of the patient, carries his incision, three or four millimetres in length from the cricoïd cartilage, rapidly, but without precipitation, as deep as the thyroïd gland before it becomes necessary to stop and sponge away the venous blood. The forefinger is then passed into the wound and fixes the trachea, its nail serving as a con-

ductor to the bistoury with which the puncture in the trachea is made. Without removing his nail from the wound in the trachea, the operator slides in the dilating forceps along it, and by a slight pressure secures enough dilatation for the admission of the canula. The child should now be set upright, in order to facilitate the expulsion of false membrane or blood from the air-passages. The end of the canula should be carried directly to the bottom of the wound, in order to prevent its sliding off in front of the trachea. Before securing it, the fact of its entrance into the air-passages must be carefully ascertained. The aperture in the trachea ought not to be of too large an extent, and even if it be made too small it may be easily enlarged by means of a probe-pointed bistoury. During the operation the child should be well covered, and carefully protected from all chills.

3. *Accidents during the Operation.*—The sliding of the canula in front of the trachea has already been adverted to. *Hæmorrhage* usually ceases when normal respiration has become established naturally or by artificial means, such as frictions or taps of the thorax, made with the view of regularizing the play of the respiratory muscles. The hæmorrhage almost always proceeds from veins, which are sometimes numerous and distended; and when the incision has been carried to a great extent, so as to approach the sternal *fourchette*, there is a great probability that numerous and voluminous venous trunks may have been opened. If the bleeding persist, rounds of agaric, dipped in Commander's balsam, should be applied. When the blood bubbles up by the side of the canula, the wound of the trachea has been made too large, so that the blood gets entrance during inspiration. A larger canula should at once be substituted. When the operation has been a laborious one, *emphysema of the neck*, sometimes extending to some distance, may be met with. It usually results from a want of parallelism between the cutaneous and the tracheal wounds. Ill-repressed movements of the child may have displaced the trachea, or too great a delay in the introduction of the catheter may have favoured the passage of the air into the cellular tissue. The same result may occur from the tracheal wound being too large or the canula too small. Frictions and shampooing the emphysematous region should be employed.

VII. *On Amaurosis from Tobacco-smoking.* By M. Sichel. (L'Union Médicale, No. 54.)

M. Sichel observes, that among cerebral amauroses there are two forms but little known. One of these observed in drinkers, he himself described as symptomatic of delirium tremens several years ago. The other, due to the use of tobacco, and first indicated by Mackenzie, he once doubted the existence of. Subsequent experience has, however, convinced him of its reality, so much so that he is now of opinion that there are few persons who have smoked during a long period more than five drachms of tobacco per diem, without having their vision and frequently their memory enfeebled. Both these forms of amaurosis are characterized by the absence of well-marked symptoms of cerebral congestion, the symptoms vibrating between those of sthenic and asthenic amaurosis, and the surgeon remaining in uncertainty as to their seat and nature until the special cause is discovered. The ophthalmoscopic symptoms, as in most old cerebral amauroses, are negative or slight, and common to other cerebral amauroses. These two forms of amaurosis, like all affections dependent upon an inveterate habit, are very refractory to treatment. Generally, the two forms are observed separately, but it is not rare to find them united, and it then becomes difficult to assign the respective shares to the alcohol and tobacco in the production of the amaurosis. M. Sichel relates an interesting instance of this combination, remarkable for yielding in so short a period as six weeks, while from three to twelve months are usually required to effect amelioration in these cases. In treating them, discontinuance or diminution of the habit is a great and a difficult desideratum. Depletion, even local, should be employed with the greatest caution; and stimulating liniments or flying blisters may aggravate the symptoms. A purgative, consisting of equal parts of magnesia and cream of tartar, is an excellent means when the function of the stomach is active, alternating it with pills of gum ammoniac and aloes; but in the disordered stomach of drinkers, small doses of rhubarb and magnesia, given twice a day, one hour before meals, form a good corrective. Bathing the eyes and forehead with cold water, irritant pediluvia, and dry cupping or flying sinapisms applied to the extremities, are excellent adjuvants. In M. Sichel's case, an ointment composed of one part of the black oxide of copper, and ten parts of lard, was applied to the temples, and was succeeded by flying blisters. M. Mercier, in corroboration of the unsuspected effects of tobacco in generating disease, related a case in which a cough, which had persisted for a year, and purpura, which had lasted for seven months, soon yielded after the cessation of smoking, which had been excessive. His own practice has furnished him with full proof of the depressing effect of this agent upon the generative functions.

VIII. *Summary.*

Amputation.—Ashurst's Surgical Cases. (Amer. Jour. of Med. Science, April.) Illustrative of the Danger of Secondary Amputation.—Legouest on the Comparative Results of Immediate and Consecutive Amputation. (Gaz. Hebd., No. 14–15.) — Swinburne's Rules for Amputating in Military Surgery. (Amer. Med. Times, No. 13.)—Talichet on Amputation by means of Chloride of Zinc. (Mem. de la Soc. de Méd. de Lyon, vol. i. p. 36.)

Anchylosis.—Busch's Case of Anchylosis of the Hip and Knee treated by Forcible Rupture. (Langenbeck's Archiv, vol. iv. p. 50.)—Camillo on Cases of Forcible Rupture of Anchylosis of the Knee. (Gaz. Med. Ital., No. 7–16. See Jaw.)

Anus.—Thaden's Case of Artificial Anus Treated by Fine's Method. (Langenbeck's Archiv, vol. iv. p. 154.)

Aspermatism.—Cosmao-Dumenez on Aspermatism. (Gaz. Méd., No. 12 and 14.) Impotence characterized by defective ejaculation, although erection is normal, and due to various diseases of the genito-urinary organs.

Astigmatism. — Schweigger on Astigmatism. (Graefe's Archiv, vol. ix. p. 178.)

Bladder.—Brodrick's Case of Rupture of the Bladder from external violence, with recovery. (Madras Journal, No. xi.)

Bone.—Richet on Vascular Tumours of Bone. (Bull. de l'Acad. de Méd., No. 14.)—Pitha on the Operative Treatment of Necrosis. (Allg. Wien. Med. Zeit., Nos. 10–15.)—Rabl on a Cause of Necrosis. (Wien. Gesell. Wochenblatt, Nos. 4–5.) In 7 cases out of 23 of necrosis, the affection is attributed to metastasis during typhus.

Bronchocele.—Lepine on the Treatment of Cystic Bronchocele by Cauterization. (Mem. de la Soc. de Méd. de Lyons, i. p. 335.) Seventeen cases were treated by cauterization with chloride of zinc; only one proved fatal.

Catheterism.—Mercier on a New Mode of employing Invaginated Catheters. (Gaz. des Hôp., No. 46.)

Cicatrices.—Philippe on the Employment of Serres-fines in the Treatment of Cicatrices. (L'Union Méd., No. 56.)

Delirium Tremens.—Surgeon Blacklock's Cases of Delirium Tremens. (Madras Journal, No. xi.)

Dislocation.—Busch's Observations upon Dislocations. (Langenbeck's Archiv, vol. iv. p. 1.) Illustrative of the importance of reposition rather than of forcible traction, the tension of the capsule being the chief obstacle to reduction.—Bell on the Nomenclature of Scapulo-humeral Dislocation. (Edin. Med. Jour., May.—Dittel on Dislocation of the Thumb backwards. (Allg. Wien. Med. Zeit., No. 14.)—Morel-Lavalle's Case of Subacromial Dislocation of the Clavicle. (Gaz. des Hôp., No. 26.)—Gaulke on a Rare form of Dislocation of the Patella. (Deutsche Klinik, No. xi.)

Ear.—Anderson's Cases illustrative of Diseases of the Ear. (Glasgow Jour., April.)—Gruber's Clinical Observations on Diseases of the Ear. (Spital's Zeitung, Nos. 3, 8, 12, 14.)—Gruber on Discharges from the Ear. (Allg. Wien. Med. Zeit., Nos. 12, 13, 15.)—Boudet on Noises in the Ear. (Mem. de la Soc. de Méd. de Lyons, vol. i. p. 173.)—Voltolini on Diseases of the Labyrinth and Auditory Nerve. (Abhand. Schlesisch. Gesellsch., 1862, No. 1.)—Politzer's New Mode of treating Deafness dependent upon Impermeability of the Eustachian Tube. (Wien. Med. Woch., No. 6, 7, 10.)

Elephantiasis.—Butcher's Case of Elephantiasis treated by ligature of the femoral artey. (Dublin Quart. Jour., May.)

Entropion.—Fano on the Treatment of Spasmodic Entropion by Subcutaneous Section. (L'Union Méd., No. 50.)

Excision.—Husted's Case of Excision of the Elbow. (Trans. N. York Med. Soc., 1862, 327.)—Butcher's Case of Excision of the Knee. (Dublin Quar. Jour., May.)

Eye.—Schiess' Observations on Panophthalmia. (Graefe's Archiv, vol. ix. p. 22.)—Wecker's New Procedure for Displacement of the Pupil. (Bull. de Thérap., vol. lxiv. No. 6.)

Face.—Debout on Reparative Surgery after Mutilations of the Bones of the Face. (Gaz. des Hôp., No. 55-56.)

Gangrene.—Annandale's Case of Gangrene dependent on Obstruction of the Femoral Artery. (Edin. Med. Jour., April.)

Gun-shot Wounds.—Legouest on the Complications of Gun-shot Wounds. (Gaz. Méd., Nos. 14, 15.)—Water's Report on Cases of Gun-shot Fracture of the Thigh treated conservatively. (Amer. Med. Times, Nos. 15, 16.)—Carruthers on Comminuted Fracture from Minié Balls. (Amer. Jour. of Med. Soc., April.) The author recommends conservative treatment, maintaining regular and moderate extension.—Lecomte on the Exploration of Balls. (Mem. de la Méd. Militaire, No. 2, p. 94.)—Baudry's Case of Gun-shot Wound. (L'Union Méd., No. 47.) Both these last authors write especially in reference to Garibaldi's case.

Hare-Lip.—Butcher on a Case of Complicated Double Hare-Lip. (Dublin Quar. Jour., May.)

Hernia.—Streubel's Analysis of recent Papers on Hernia and its Treatment. (Schmidt's Jahrbuch, vol. cxviii. No. 1.)—Colson on the Operation for Hernia without opening the Sac. (Archives Générales, vol. i. pp. 273-561.) The author strongly advocates this operation, but is very imperfectly aware of what has been done in this country in the matter.—Sand's Three Cases of Operation for Strangulated Hernia without opening the Sac. (Amer. Med. Times, No. 13.) These seem to be some of the first cases of this operation performed in America.—Busch on a Cause of a Rare Form of Congenital Hernia. (Langenbeck's Archiv, vol. iv. p. 47.)

Hip-joint Disease.—Jordan on Treatment of Hip-joint Disease by Extension. (Gaz. des Hôp., No. 45.)—Discussion on Treatment of. (Bulletin of N. York Acad. of Med., vol. i. pp. 191-211.)—Sayre on Treatment by Extension. (Amer. Med. Times, No. 19.)—Dumreicher on the Reduction of Consecutive Dislocation of the Hip. (Allg. Wien. Med. Zeit., Nos. 7 and 8.)—Two Cases treated successfully by rotation externally.

Hydrophthalmia.—Jeffries on a Case of Hydrophthalmia with Enucleation of the Eyeball. (Boston Jour., vol. lxvii., No. 26.)

Iridectomy.—Coccius on Glaucoma and its Treatment by Iridectomy. (Graefe's Archiv, ix. p. i.)

Jaw.—Heath on Closure and Immobility of the Jaw. (Dublin Quar. Jour., May.)—Grube on True Anchylosis of the Jaw relieved by chiselling the ramus. (Langenbeck's Archiv, iv. p. 168.)—Schut's Case of Anchylosis of the Jaw forcibly ruptured. (Allg. Wien. Med. Zeit., No. 14.)—Case of Immobility of the Jaw. (L'Union Méd., No. 52.)—Boeckel on Temporary Resection of the Jaw for extirpating Cancer of the Mouth. (Gaz. Hebd., No. 19.)—Legouest on Deformities which ensue after Resection of the Jaw. (L'Union Méd., No. 52.)—Butcher's Cases of Excision of the Upper Jaw. (Dublin Quar. Jour., May.)

Knee-Joint.—Jobert on Foreign Bodies in the Knee. (Gaz. des Hôp., No. 62.)—Bron on Deviations of the Knee inwards. (Mem. de la Soc. de Méd. de Lyon, vol. i. p. 16.)

Larynx.—Budge's Instruments for Removal of Foreign Bodies from the Larynx. (Trans. N. York Med. Soc., 1862, p. 293.) See Polypus.

Lithotomy.—Watson's Clinical Remarks on Lithotomy. (Edin. Méd. Jour., June.)

Lithotrity.—Civiale's Course of Lectures on Lithotrity. (Gaz. des Hôp., Nos. 39, 54, 58.)—Santopadïe on the Accidents accompanying Lithotrity. (Omodei's Annali, clxxxiii. p. 30.)—Ivanchich's Report on Cases of Lithotrity. (Wien. Wochenschrift, Nos. 11 and 14.) Continuation of former Reports by the author of cases occurring in his own practice, now comprising 126 cases.)

Lymphatics.—Carter on Varicose Lymphatics in connexion with Elephantiasis and Chylous Urine. (Trans. Bombay Med. Soc., N.S., No. 7.)

Military Surgery.—Detmold's Lectures on Military Surgery. (American Med. Times, No. 4–10.)—Laveran on the Mortality in Armies during Campaign. (Annales d'Hygiène, vol. xix. p. 242.)

Nerves.—Schuh on Resection and other Operations on Nerves. (Wien. Wochenschrift, Nos. 1, 5, 11.)

Nose.—Ollier on Restoration of the Nose by Osteoplastics. (Bulletin de Thérap., No. 9.)

Ophthalmoscope.—Galezowski on the Ophthalmoscope in Diseases of the Retina and Optic Nerves. (Annales d'Oculistique, March.)—Practical Instructions on the Ophthalmoscope and the mode of its employment. (Recueil de Mem. de Méd. Militaire, vol. ix. No. 1.) Issued by the Medical Council of the French Army.

Osteoplastics.—Wolff on Osteoplastics in relation to Surgery and Physiology. (Langenbeck's Archiv, vol. iv. p. 183.) The author details his own experiments, and furnishes an exhaustive review of the literature of the subject.

Polypus.—Verneuil's Case of Naso-Pharyngeal Polypus. (Gaz. des Hôp., No. 38.)—Turck's Instruments for Removal of Polypi of the Larynx. (Allg. Wien. Med. Zeit., No. 16.)—Stark's Extirpation of Polypus of the Larynx by crushing. (Archiv der Heilkunde, No. 3.)—Verneuil on Surgical Treatment of Polypi of the Larynx. (Gaz. Hebdom., No. 22.)—Gilfillan's Treatment of Polypus Uteri. (Trans. N. York Med. Soc., 1862, p. 349.)

Prothesis.—Debout on Prothesis after Operations on the Eye. (Bull. de Thérap., vol. lxiv. p. 188.)—Minor Report on Artificial Limbs. (Bulletin of N. York Acad. of Med., vol. i. p. 163.)—Busch on an Artificial Limb after Amputation of the Thigh. (Langenbeck's Archiv, vol. iv. p. 32.)—Debout on Prothesis in Congenital Deformities of the Lower Limbs. (Bull. de Thérap., vol. lxiv., pp. 233–420.

Purulent Infection.—Jules Guérin on the Doctrine of Purulent Infection. (Gaz. Médicale, No. 16.)

Spinal Cord.—Leudet on Congestion of the Spinal Cord from falls or violent efforts. (Archives Gén., vol. i. p. 257.) Leudet describes a temporary congestion relievable by local antiphlogistics. The paper is of interest to those having to deal with railway accidents.

Strabismus.—Donders on the Pathogeny of Strabismus. (Graefe's Archiv, ix. p. 99.)

Stricture.—Civiale on Stricture and Urethrotomy. (Gaz. des Hôp., Nos. 24, 27, 32, 35.

Sutures.—Letenneur on the Advantages of Silver Sutures. (Gaz. Méd., No. 16.)

Syphilis.—Diday on the Form of the Initial Symptom in Syphilis. (Gaz. Hebd., Nos. 21, 22.)—Ricordi on the Cases of Syphilis treated at the Maggiore Hospital, Milan. (Omodei's Annali, vol. clxxxiii. p. 184.)—Wertheimber's Treatment of Congenital Syphilis. (Jahrbuch für Kinderheilkunde, No. 1.) —Köbner's Experiments with Chancre Virus. (Abhand. Schlesisch. Gesell., 1862, No. 1.) — Förster's Pathological Anatomy of Congenital Syphilis. (Würzburg Med. Zeit., No. 1.)—Sigmund on Primary Syphilis of the Mucous Membrane of the Mouth. (Wien. Wochensch., No. 18.)—Diday on Chancre of the Tonsil. (Mem. de la Soc. de Méd. de Lyon, vol. i. p. 45.)

Teeth.—Icard on Accidents produced during the Development of the Wisdom-teeth. (Mem. de la Soc. de Méd. de Lyon, vol. i. p. 131.)

Tetanus.—Wood on the Prevention of Traumatic Tetanus. (Madras Jour., No. 11.)—Lowe on the Pathology and Treatment of Tetanus. (Ibid.)

Tongue.—Fischer on Galvano-Caustic Amputation of the Tongue. (Wien. Wochenschr., No. 16.)

Tourniquet.—Lee on a New Elastic Tourniquet. (Trans. N. York Med. Soc., 1862, p. 305.)

Tumours.—Fleury on the danger of Temporizing in Encysted Tumours. (Gaz. Méd., 16 and 17.) The author dwells on cases which after long quiescence have assumed a malignant form.

Uranoplastics.—Weber's Case of Uranoplasty in a young infant. (Langenbeck's Archiv, vol. iv. p. 295.)

Varicocele. — Packard's Apparatus for the Radical Cure of Varicocele. (Amer. Med. Times, No. 17.) A simplification of Ricord's procedure.

Varicose Veins.—Deroubaix on the Cure of Varicose Veins by Caustic. (Presse Médicale Belge, No. 18.)—Turner on Exciting Adhesive Inflammation over Varicose Veins. (Amer. Jour. Med. Science, April.) Small blisters are applied over the projecting portions of the veins, and dressed with a paste formed of tartar emetic and croton oil.)

Venesection.—Pissin on Venesection by the Ranine Vein. The author proposes the revival of this operation for affections of the throat and fauces, especially in children above a year old.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D., F.R.C.P.

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I. The Non-pregnant State.

1. *Fatal Peritonitis after Perforation of a Non-pregnant Uterus.* By Luigi Concato, of Bologna. (Annali Univ., May, 1861.)
2. *Extirpation of an Intra-uterine fast-adherent Polypus weighing a pound and a half by the "Spiral Incision."* By Dr. Alfred Hegar. (Monatschr. f. Geburtsk., March, 1863.)

1. The case of Dr. Concato is a remarkable one. A woman, aged sixty-eight, had borne several children, and after a long widowhood, menstruation having

long ceased, married again at sixty-four. She had always enjoyed good health, but from this time complained of pain *in coitu*. When admitted she had symptoms of peritonitis, collapse, abdomen much distended with fluid, and tympanitic. Under these symptoms she died.

Autopsy.—The abdomen and pelvis were filled with about twelve pounds of a yellowish-green, fluid, foul-smelling alkaline matter, containing pus. In the place of the uterus, between rectum and bladder, there arose a round tumour in the posterior segment of which was a large opening through which a greyish-black soft substance escaped. The absence of body of the uterus, with presence of the cervix, and normal condition of the appendages, made it apparent that the body of the uterus had been converted into a kind of cyst. The author discusses the possibility of cancer and of tuberculosis of the fundus uteri, of a fibroïd tumour or polypus, and concludes that the perforation of the uterus was the result of a degeneration proceeding from within, and brought about by a diphtheritic inflammation.

2. In a former Report,* a method was described after Professor Simon, of Rostock, for removing large fibroïd polypi from the uterus, the basis of which was so large as to render application of ligature or écraseur impossible. This consisted in making a series of incisions into the capsule of the tumour, and then dragging upon it. The incisions so favour elongation by dragging, that it soon becomes possible to reach the origin and divide it. Dr. Hegar, of Darmstadt, describes a case in which this method is somewhat modified. The tumour was chiefly intra-uterine, emerging an inch and a quarter beyond the cervix uteri. The whole circumference of the tumour was adherent to the uterine walls. The adhesions were divided by scissors as far as possible; the point of the scissors was then turned directly upon the tumour, which was then cut, first in front, then on the side, proceeding to incise in a circular direction, gradually rising so as to form a spiral series of cuts. Thus, by dragging on the tumour, transfixed below by a strong thread, it became elongated, so that the root was reached and cut through, and the tumour removed. Some fever and peritonitic symptoms followed, but the patient entirely recovered.

II. Pregnancy.

1. *A new Contribution to the Knowledge of Amniotic Bands*. By G. Braun. (Zeitschr. der Gesellsch. der A. in Wien, 1862; and Mon. f. Geb., March, 1863.)
2. *On a Case of Acute Yellow Atrophy of the Liver in a Pregnant Woman*. By Dr. C. Hecker. (Mon. f. Geb., March, 1863.)

1. Dr. Braun contributes a useful essay towards the elucidation of the mode of formation of bands and membranes in the amniotic sac, which are occasionally found in connexion with intra-uterine amputation of fœtal limbs. He describes the formation of the amnion through cell-multiplication and special relation of growth of the outer layer of the germinal membrane. By a rapid surface-growth at one narrowed spot the first amniotic folds arise; these are carried in a definite manner towards the back of the embryo, and the final coalescence of the approaching folds at one spot occurs; upon this the amnion remains yet for a time in connexion with the serous cavity. Anomalies of this connexion are most frequently caused through deficiency of amniotic fluid, or through a too-late secretion of it, so that in various ways the development of the embryo may be interfered with. Dr. Braun relates two interesting cases:—(1.) He found on the body of a newborn child, weighing four pounds and a half, at the back of

* British and Foreign Medico-Chirurgical Review, April, 1863.

the cranium, a skinny, flaccid sac, fissured for a length of two inches, and covered with shreds, in aspect and structure resembling amnion. The right frontal bone and the occipital bone were undeveloped. In the cavity of the cranium were sero-fibrous strings running from one side to the other. The palate was fissured. On the right hand the first three fingers were completely amputated; from the metacarpal bone of the index, to the stump of the middle finger, ran a sero-fibrous string, having near its end an appendage covered with normal skin. On the left hand were several strings which cut the thumb incompletely, divided the index finger in two unequal halves, compressed and bent the little finger; in lieu of the fourth and little fingers was a small protuberance covered with skin. On the left foot the second and third toes had grown together. On the right foot the third toe was bound to the fourth by a sero-fibrous string; the nail-joint of the second was amputated. On the placenta were found appearances of amniotic bands.

(2.) This case related to a fœtus born alive, male, $15\frac{1}{2}''$ long. On the forehead were several scars on the skin, and non-development of the frontal bone. These scars were connected internally with the dura mater, and through this with the inner membranes of the brain, and from which ran several torn pseudo-membranous strings. At the seat of the nose was a right smaller, and a left larger fissure. The upper lip was split. On the right hand the last joints of the three middle fingers were undeveloped, and from the end of the ring finger a string ran to the index finger. On the left hand the thumb was undeveloped, the last three fingers had grown together. The right foot was clubbed; left foot in normal direction, but the phalanges of the third and fourth toes were very rudimentary, the rest had no trace of nail-formation, and were united together by a bridge of skin; on the second toe was a torn pseudo-membranous string. From the inner surface of the amnion, which looked normal in smoothness and colour, ran several strings, hanging free in the amniotic cavity, or attached to the opposite spots. At the seat of insertion of the umbilical cord, near the edge of the placenta, there was a roughened spot, denuded of amnion, from which sprang several thin shreds and strings.

2. Dr. C. Hecker relates a remarkable case of acute yellow atrophy of the liver occurring in a pregnant woman. He observes that Spaeth reports having out of 33,000 pregnant women only twice met with this affection. On the 7th August, 1862, a woman, aged twenty-eight, applied to the obstetric policlinic at Berlin, who had complained of illness about thirty-six hours. The first symptoms—diarrhœa, headache, and great thirst—came on so suddenly as to cause suspicion of her being poisoned by a piece of sponge she had swallowed. On the next day, vomiting and pain in the abdomen set in. The patient being in her sixth month, interpreted these as indications of commencing labour. The pain in the hypogastrium was intense. There was no meteorism. There was pain also in the lumbar regions. The vomiting and diarrhœa ceased; then inordinate thirst appeared, dry tongue, painful expression of face, great restlessness, hot skin, pulse 112. Treatment did no good; urgent vomiting of chocolate-coloured matter returned; the pain spread more widely over the abdomen. There was now noticed a yellowish coloration of the skin, which soon increased, but was confined to the upper half of the body. As the skin became icteric, the brain was affected; delirium set in; collapse followed; coma and death sixty-two hours from the beginning of the illness.

Autopsy twenty-eight hours after death.—The head was not examined; the lungs were healthy; the heart was unusually flaccid, of distinct yellow colour; on its surface, and especially at the origin of the great vessels, there were many punctiform ecchymoses; the endocardium had an icteric tinge. On microscopic examination, the primitive muscular fibres were seen in advanced fatty degeneration; no trace could be found of cross-stripings, but abundance of fat-molecules. The liver did not appear smaller, but bulged in normal wise under the false ribs; it was of intense ochre-colour; its weight was 1370 grammes.

The gall-bladder was collapsed, and contained a small quantity of a brown thin gall. The ductus cysticus and choledochus were pervious. On section, there oozed forth a yellow fluid, which looked under the microscope as if holding milk; the whole field was covered with fat-globules of the most various sizes, and amongst them liver-cells containing bodies like colostrum-corpuscles. The spleen was of normal size, quite unaltered. The kidneys were in the second stage of parenchymatous inflammation. The epithelium of the straight urinary canaliculi quite filled with fat-molecules. The uterus and fœtus showed nothing remarkable. The urine could not be examined during life.

Dr. Hecker insists upon the simultaneous and equal disease of the heart, liver, and kidneys, as indicating the operation of a common cause, a primary disease of the blood affecting all parts of the body; that later the excitation of the function of the liver played an important part.

III. Labour.

1. *A Case of Spontaneous Evolution.* By Dr. S. Johnson, of Congleton. (Dublin Quart. Journ. of Med. Sc., May, 1863.)
2. *Report of the Proceedings in the Lying-in Hospital of the Charité at Berlin during the Winter* 1859–60. By Dr. Nagel. (Annal. des Charité Krankenhaus, Berlin, 1863.)
3. *Hair-cyst of the Cervix Uteri as an Obstruction to Labour.* (Presse Médicale Belge, 1862.)
4. *Can Abortion be Provoked by Imitation?* By Dr. Durand-Fardel. (L'Union Méd., March, 1863.)
5. *On Cephalotripsy repeated without Traction; or, a Method for Delivering Women in Extreme Contractions of the Pelvis.* By Dr. Pajot. (Arch. Gén. de Méd., May, 1863.)
6. *On the Liberation of the Arms in Labours in which the Head is last Born.* By Dr. V. Hüter. (Mon. f. Geburtsk., March, 1863.)

1. Dr. Murphy communicated to the Dublin Obstetrical Society a case of apparent spontaneous evolution reported to him by Dr. S. Johnson. Dr. Johnson, attending a patient in her first labour, diagnosed a head-presentation in the third position. At this time the os uteri was somewhat larger than a crown-piece, and the membranes were unbroken. No further examination was made until the liquor amnii had escaped, when the os uteri was found three parts dilated, and the *breech presenting*. The funis also descended. The child was nearly still-born, but was restored with some difficulty. The child had a swelling on the upper part of the left parietal bone, extending towards the occipital, thus confirming the first diagnosis of head-presentation in the third position.

[The Reporter is informed by Dr. S. Johnson that the liquor amnii was not in marked excess; that the child was of full size, and is still living.—R. B.]

2. During the first four months of the winter 1859–60, the Charité Lying-in Hospital of Berlin served for the instruction of midwives; during the remaining two, for the conduct of the State obstetric examination. Patients were generally discharged on the eleventh or twelfth day after labour. The term began with ten pregnant women, and twenty-three puerperal women in the wards. There were received during the six months 337 patients.

The funic souffle was heard many times. It was mostly heard as a simple frequent whizzing, sometimes near the situation of the fœtal-heart sounds, sometimes exactly over this spot, and sometimes at a considerable distance from it. The cord was found in these cases more or less surrounding some part of the child's body.

The diagnosis of twin-pregnancy was made seven times correctly, but cases were not wanting in which the diagnosis was not verified.

The slighter degrees of pelvic contraction were frequent. Sixteen instances of the more marked contractions were observed. The external conjugate diameter of the brim measured in these last from six inches and a half to seven inches. In these cases there had been early rachitis.

Two cases of epilepsy were received. One woman had been subject to epileptic fits since childhood. Before pregnancy (her first), the fits had a four-weekly type; during pregnancy, they recurred fortnightly. Labour set in at the normal time, and was not severe. Immediately afterwards she had a strong fit. No other fits occurred during her stay in hospital. The other epileptic patient suffered a fit during labour without the course of labour being influenced.

In 46 cases, labour came on prematurely. The cases ascertained were:—Constitutional syphilis, 12; violent concussion of the body through accident, 3; gross ill-treatment, 1; straining in lifting heavy weights, 6; violent coïtus, 3; twin-pregnancy, 2; severe general illness, 2; too tender youth, 2; placenta prævia, 1.

The frequency of the presentations were as follows:

1st head-presentation		194
2nd ditto		53
3rd ditto	passing into 2nd	31
4th ditto		1
1st face-presentation		2
1st breech-presentation		2
2nd ditto		5
Footling-presentation		3
2nd shoulder-presentation		2

A twin-case was remarkable from the fact that the second child was delivered after spontaneous evolution. The first child was born having presented in the first head-position. The second child presented immediately after rupture of the membranes, by the left shoulder, head to the left, back forwards. During chloroformization preparatory to version, the left side of the breech was pressed down by the force of the uterine contractions, and in a short time a child seventeen inches long, weighing four pound, still-born, was delivered.

[It is not stated whether the shoulder receded.]

Artificial aid was necessary in sixty-one cases.

The forceps was applied to the presenting head	29	times.
"Smellie's hand-grasp" used to following head	9	"
Turning by feet	2	"
Extraction by the feet	2	"
Reposition of prolapsed funis by hand	2	"
Lateral incisions of the labia majora	16	"
Detachment of adherent placenta	1	"

The forceps operations were therefore one in eleven labours.

The indications for this instrument were, pelvic contraction, 10; feeble uterine action, 15; large varices of external genitals, 1; prostration in a phthisical patient, 1; prolapsus of cord, 1; eclampsia, 1. Twenty-four children were delivered alive.

The case of puerperal convulsions is interesting. Three days before labour at term, the patient complained of headache, syncope, and loss of appetite. On the day preceding labour, she was seized suddenly with unconsciousness, general clonic cramps. The fits recurred very frequently. The convulsions sometimes were of chief severity in the muscles of the neck and face; the lower jaw was violently moved up and down, and during the fits the face was deeply cyanosed. The urine contained abundance of albumen. Venesection, uterine douche, chloroform, induced no change. The membranes burst spontaneously, and the head was delivered by forceps. *The child exhibited in its arms and legs strong contractions of the flexors,* and could not be restored to life. The uterus contracted well. The convulsions did not cease after labour. The patient died the following night in a fit. At the autopsy there were found small capillary hæmorrhages in the dura mater of the brain, a considerable hæmorrhage in the tissue of the endocardium, and a commencing parenchymatous nephritis.

Twenty-nine, or nine per cent. of the patients, were transferred to the department for sick women. Nine deaths are specified.

3. A woman had borne two children without anything remarkable, when at the age of thirty-nine, being in labour Dr. Cousot attended. The labour became lingering. He found the anterior lip of the cervix effaced, but the posterior one was very resisting, of great size, hard, stretched, and appearing to pass into the posterior wall of the vagina without lines of separation. The head presented above the tumour. The forceps was applied, and extraction performed, some force being required. The tumour was expelled along with the head, although pains were taken to keep it back whilst supporting the perinæum. The seat of rent in the cervix could not be felt, and no hæmorrhage followed. The woman did well. The cyst measured eleven centimetres by six. It contained a sero-purulent fluid, and a considerable quantity of black hair. No trace was discovered of higher organization.

4. Dr. Perrin having related to the Société Médicale d'Emulation the case of a midwife, who, at the sight of women in labour, always experienced uterine contractions, and, if pregnant, aborted, Dr. Durand-Fardel cited the experience of a distinguished agriculturist, who had been appealed to for information as to analogous facts amongst animals. This gentleman absolutely contradicts the idea emitted, that pregnant cows, in company with cows falling in labour, were disposed to abort. [What is the experience of lying-in hospitals?]

5. Dr. Pajot proposes to extend the use of the cephalotribe to cases of greater pelvic contraction than those to which it has been commonly limited, and thus, in so far, to diminish the necessity of resorting to the Cæsarian section. In the most ordinary contractions, he says—namely, those between $2\frac{1}{4}$ inches and $3\frac{3}{4}$ inches—cephalotripsy is a good operation, of moderate difficulty, and the applications of the instrument need not exceed two or three. Even here the cephalotribe is not, indeed, a perfect instrument. It compresses the head in the transverse diameter, when the contraction is in the antero-posterior, thus elongating the fœtal part in the contracted direction of the pelvis; but by an artifice of the operation, which consists in imparting to the head during traction a slight rotatory movement, the two branches of the instrument are gradually brought nearly fore and aft, and the defect of the instrument is partially corrected.

But in extreme contractions, those which lay between $2\frac{1}{4}$ inches and $1\frac{1}{4}$ inch, cephalotripsy is admitted to be extremely dangerous, compromising the mother's life as much as the Cæsarian section. The impossibility under these circumstances of reaching the base of the cranium with the instrument, the disproportion between the passage and the irreducible part of the head, the excessive tractions necessary—and often fruitless—to bring the mature fœtus through, the bruisings, the frictions, the lacerations, and death immediate or remote which often result, all are recognised by those who have often practised this operation in these conditions, and explain how it is that the Cæsarian section has been placed in competition with it.

M. Pajot, however, carries the application of the cephalotribe to cases in which the contraction measures one inch and a quarter only. He begins as soon as the os uteri is sufficiently open to admit the instrument. Following or not the practice of Dubois, of perforating the cranium as soon as the os uteri will permit, the first application of the cephalotribe must be made as early as possible, insisting chiefly on maintaining pressure on the hypogastrium to keep the head fixed; carrying the handles of the instrument well backwards, and pushing the blades as deeply as possible so as to bury the joint in the orifice of the vagina. All this is necessary, in order to be able to crush the base of the skull. On this first cephalotropsy the success of the whole operation depends. Once the impression of each blade is made in the cranium, this impression becomes, until the head is turned, a cause of arrest for the ends of the instrument in the subsequent applications. The first crushing thus made, he endeavours to effect a movement of rotation with the instrument, so as to place the lessened dimensions of the head in the contracted diameter of the pelvis, groping with gentleness to the right or left, to find the direction where there is most room. If there is any resistance, he desists altogether from rotation. The uterus almost always succeeds in moulding the new form given to the head by crushing to the shape of the canal, by imparting that rotation which was found difficult with the instrument. The head crushed as much as it can be, the instrument is disjointed and withdrawn *without having exercised any traction.* A second, and if necessary, a third introduction and crushing is effected, always without traction. The patient is then left. According to her condition, as to strength and uterine contractions, M. Pajot then repeats every two, three, or four hours the multiple crushings, two or three at a sitting. The head being sufficiently broken up, one or two crushings usually suffice for the trunk. This is the method to which M. Pajot has given the name, "Cephalotripsy repeated without tractions." He points out that the distinguishing character of his operation is not extraction, but to effect the progressive diminution of the parts in proportion as the uterine contractions mould the crushed head, and dispose it themselves in the direction most favourable to enable it to traverse the contraction, taking, he says, his model in the most natural mechanical phenomena of labour.

In answer to the objection, that the labour is thus very protracted, Mr. Pajot urges, that beginning the operation as soon as the commencing dilatation of the os uteri will permit, the labour may take from six to eighteen, and possibly twenty-four hours.

M. Pajot relates 7 very interesting cases in which his method was practised. In 4 of these the antero-posterior diameter measured only six centimetres, or about two and a quarter inches; in the remaining cases the contraction was below two inches. In 6 cases, the fœtus was expelled; in 1, in which the operation had been begun with a defective instrument, the woman died undelivered. One of the first 6 cases also died after the operation.

[It cannot be doubted that these are all cases in which the Cæsarian section might be considered necessary or justifiable, and it is not probable that the results would have been equally satisfactory.—R. B.]

6. Dr. Hüter discusses a very important question in practical midwifery, concerning which little or no information is found in many systematic works. How is the obstetrician to liberate the arms when the head is last born, as in

breech and turning cases? After giving an useful summary of the methods recommended by other writers, he explains a proceeding of his own. He grasps the thigh or thighs of the child, and drawing downwards and then forwards, turns them near the middle of the mother's abdomen, thus causing the trunk to revolve around the symphysis. The result of this manœuvre is to lower the shoulder which is hindermost, and which has to make the longest circuit. In this manner the elbow is brought lower down, within reach of the index finger, which can then complete the extrication. To liberate the second arm, he then adopted the plan described by V. Ritgen, which consists in giving a rotation-movement to the child on its long axis, the effect of which is throw the arm across its breast.

IV. Puerperal State.

Report on the Puerperal Affections occurring from November 1st, 1861, to April 15th, 1862, in Professor Traube's Department of the Charité. By Dr. E. Leyden. (Annalen der Charité zu Berlin, 1863.)

In Dr. Leyden's Report of Prof. Traube's department at the Berlin Charité are many observations of the variations of the temperature of the body during puerperal diseases, and many interesting autopsies. The histories of 83 cases are related, some minutely. Of these cases there were 60 of metritis and consequent phlegmonous processes, 9 of phlebitis, 5 of phlegmasia, 1 of polyarthritis and pericarditis, 2 of pleurisy, 1 of eclampsia, 4 of mental disorder, 1 of leucæmia. The cases of metritis arose from an epidemic; 26 of the 60 cases ended fatally. Of the remaining 23 cases 13 died. Ulcerations of the genital passages were frequently found. In the malignant cases the uterus had often a clearly marked diphtheritic aspect, and Professor Traube several times pointed out this resemblance to hospital gangrene. In some cases the vagina exhibited malignant gangrenous affections. In nearly all the fatal cases diffuse peritonitis was found associated with metritis, and often with double pleurisy. The elevation of temperature in these pernicious cases is always considerable. But there were cases in which the temperature did not reach the greatest height, and in which a premature collapse set in. In other cases the temperature rose to 104° and 107° F. Marked remissions of temperature, with marked falling of the pulse, were very rare in the malignant cases. When such a remission happened, it almost always bore a favourable meaning. On the other hand, towards the end of the disease there was almost constantly observed a considerable sinking of temperature, with an undiminished or even accelerated pulse-frequency. A very remarkable symptom was the commonly accelerated respiration, even in cases in which the lungs and pleura were quite intact. The frequency of the respiration rose *pari passu* with the pulse towards the end up to 40 and 50 in the minute. The cause of this lies in the hindrance to inspiratory movements of the diaphragm, consequent upon the meteorism and painful peritonitis. The sensorium was mostly affected. Delirium often appeared at night, and sometimes even mania. Meteorismus was scarcely absent, and towards the end attained an enormous degree.

V. Summary.

The following papers, either for want of space, or because they are published in readily accessible journals, are referred to by title only:—

1. Notes on Puerperal Convulsions. By George K. H. Paterson, L.R.C.P.E. (Glasgow Med. Journ., April, 1863.) Dr. Paterson relates two cases. In neither was there any œdema before or during labour. Both were successfully

treated. In one case great benefit was derived from an aqueous solution of antimony.

2. On the Pathological Relation between Albuminuria and Puerperal Mania. By A. S. Donkin, M.D. (Edinb. Med. Journ., May, 1863.) Dr. Donkin relates a case of puerperal mania in which the urine was albuminous. He submits the following proposition: "That the acute dangerous class of cases are examples of uræmic blood-poisoning, of which the mania, rapid pulse, and other constitutional symptoms are merely the phenomena; and that the effusion, therefore, ought to be termed uræmic or renal puerperal mania, in contradistinction to the other form of the disease."

3. Cæsarean Section, with successful result for mother and child; a year later, artificial induction of labour and rupture of the uterus, with successful gastrotomy. By T. J. Freericks, of Bussum. (Schmidt's Jahrb., No. 3, 1863.)

4. Cases (Two) of Ovariotomy. By Thomas Keith, M.D., F.R.C.S.E. (Edinb. Med. Journ., April, 1863.)

5. Case of Ovariotomy. By Dr. Lyon. (Glasgow Med. Journ., April, 1863.)

6. Case of Retroversion of the Uterus, causing Retention of Urine during Pregnancy. By John Dickie, M.D. (Edinb. Med. Journ., April, 1863.) The subject of this case was a primipara (?). The retroversion was presumed to be caused by the tenesmus induced by croton oil, taken with the view of inducing abortion. The uterus was reduced by placing the patient in the prone posture, and pressing on the fundus by two fingers in the rectum.

7. Case of Undeveloped Uterus. By R. T. Tracy. (Australian Med. Journ., Jan. 1863.) In this case, growth or development of the uterus was stimulated by the use of galvanic intra-uterine pessaries.

8. Practical Observations on the Injurious Effects of Chloroform Inhalation during Labour. By Robert Johns, M.B. (Dublin Quar. Journ. of Med. Science, May, 1863.)

9. Case of Sac-shaped Dilatation of the Posterior Segment of the Uterus, with observations on the oblique posterior situs of the uterus, and retroversion of the uterus at the normal term of pregnancy. By Dr. Walther Franke. (Monatsschr. f. Geburtsk., March, 1863.)

10. Case of Transmigration of an Ovum in a Sheep. By D. F. A. Kehrer. (Monatsschr. f. Geburtsk., March, 1863.)

MEDICAL INTELLIGENCE.

The Epidemiological Society and the Statistics of Disease among the Pauper Population of England and Wales.

A MEMORIAL has been addressed by the above Society to Mr. Villiers, President of the Poor Law Board, and Chairman of the Select Committee of the House of Commons, to inquire into the operation of the laws relating to the poor, requesting permission to bring before the honourable Committee a scheme which they have drawn up, by which the statistics of disease among the pauper population of the country might be collected and arranged. It is estimated that the number of cases of sickness among the out-door and workhouse poor must at the very lowest estimate largely exceed a million per annum; and it is proposed that the statistics of these cases which are now made by the parochial medical officers to the local boards of guardians should be digested by the Poor Law Board, through the voluntary co-operation of the parochial medical officers, as a basis for an annual report "illustrative of the health of that very class of the community among which the greatest amount of preventible disease prevails."

The Society having received assurance from Mr. Villiers that their memorial should be brought under the notice of the Select Committee, have since prepared a fuller statement, explaining the grounds on which this memorial rests. As this document appears to us to contain so much of intrinsic importance, and has reference to so much which cannot fail to have an interest for the majority of our readers, we do not hesitate to give it in full. After referring to our present want of means for determining what are the "most frequent maladies existing from time to time among the labouring classes in our towns, villages, and rural districts, as also when or where epidemics are most prevalent, or if they vary much in frequency and severity in different parts of the country, and also the influences of age, sex, condition, and occupation on their development and fatality," it proceeds to state as follows:

"Neither can we tell what are the most frequent chronic ailments or incurable infirmities among the poor at different periods of life, which occasion permanent disablement and life-long chargeability upon the parochial rates, with but one exception, we believe—viz., insanity and idiocy.* That the amount of sickness from fever, for example, is annually very large, the number of registered deaths abundantly testifies. On the average of the last twenty years, this number exceeds 17,000—a mortality which probably represents upwards of 170,000 persons attacked in the course of the twelve months. The victims, too, are generally among the early adults and the middle-aged, the parents often of young families; hence so many of the children in workhouses are the offspring of persons who have either died from the disease, or who, if they recovered, were reduced to beggary in consequence. The orphans and widows of working men, prematurely cut off in this way, form a considerable proportion of the permanent recipients of parochial aid in every part of the kingdom. The sad prevalence of small-pox in many districts, from the neglect of vaccination, often serves to swell the number. How much the prevalence and fatality of fever and of small-pox may be reduced by due attention to well-known sanitary and hygienic regulations, it is unnecessary in the present day to illustrate. Then, again, the great excess of mortality among the children—mainly owing to the circumstance of the other eruptive fevers, and of diseases of the bowels, lungs, &c., being aggravated fourfold by domestic causes of insalubrity—attests the enormous amount of illness in infantile and early life among the poor. There are, moreover, various groups of disease which often cause much suffering and distress, but which very seldom prove fatal, and are therefore scarcely indicated in the Registrar-General's returns, such as maladies of the skin and of the eyes—a not unfrequent cause of protracted disablement; and as both these groups are largely dependent on unwholesomeness of the dwellings, poverty or unsuitableness of diet, neglect of personal cleanliness, &c., it is obvious that they might be easily prevented to a great extent.

"Whatever will diminish the amount of sickness among the working classes must correspondingly diminish the amount of the parochial taxes and *vice versâ*. That the first of these desirable objects is within our reach admits of no doubt; the results of the Common Lodging-House Act, and the low rates of sickness and death in most public institutions now, as compared with what they used to be, are sufficient proof on this head. Nor are instances wanting in several parts of the country where a not inconsiderable abatement of the parish charges has recently been effected by the improved health of the districts. If it be true, as has been stated on respectable authority, that three-fourths of all the actual paupers in the kingdom have become paupers, directly or indirectly, by disease, the large extent of the field for the labour of enlightened beneficence is strikingly apparent.

* On the 1st of January, 1862, the number of insane and idiot paupers was 34,271. Of this number, 18,318 were in country or borough lunatic asylums; 1193 in registered hospitals or licensed houses; 8603 in union or parish workhouses; 983 in lodgings or boarded out; and 5172 resided with relatives.

"It is scarcely possible to over-estimate the benefits to the whole community which would acrue if the attention of parochial boards and other local authorities, as well as of influential residents in a district, were regularly and systematically drawn to the current state of the general health, and to the prevalence or otherwise of epidemic disease among their out-door poor, and also to the hygienic condition of the inmates of their workhouse. In no way could this be so easily or so effectually done as through the returns—were these duly tabulated and arranged—of the medical officers who attend upon the poor in sickness; for none know so well as these gentlemen the evils which sap the health of the labourer, and which so often issue in pauperism and mendicancy. All agree that much of the illness and mortality in humble life is due to circumstances not inevitable or inseparable from mere poverty, but which are superadded to it either from ignorance or wilful neglect, or from causes over which the poor themselves have no control, however capable the evils may be of easy correction or removal.

"There are in England and Wales upwards of seven hundred workhouses, great and small, and six district schools where pauper children are lodged and fed. The total number of inmates, of recent years, has averaged about 140,000 persons, of whom 50,000 are under sixteen years of age. In the infirmaries of workhouses, there are usually—besides the ordinary sick wards, the infirm wards for aged men and women, and the nurseries for infants and young children—fever wards, and infectious and foul wards; a fact which alone indicates the prevalence of these maladies throughout the country among the poor. The general death-rate in our workhouses is not known; but that it is very high may be inferred from the fact that, in some years, one in every eleven deaths in London occurs in the metropolitan workhouses. In 1861, the number was 5755; while the total number in all the civil hospitals of the metropolis was only 3723. 'The death of so many persons in the large workhouses demands inquiry,' remarked the Registrar-General.

"The sanitary condition and arrangements of the workhouses in different parts of the country are reputed to be far from satisfactory; the occasional severe outbreaks of epidemic disease, and the inveteracy of various chronic maladies among the inmates, can only be accounted for in this way. In a late quarterly return of the Registrar-General, the large mortality which occurred in a provincial workhouse was stated to be due 'to the crowded state of the house, and the defective drainage of the premises.'

"The want of reliable information as to the current amount of sickness and death among the out-door and in-door poor has been so much felt, that several efforts have recently been made to obtain the desiderated data in separate districts and localities. The metropolitan medical officers of health attach the utmost value to this subject in the prosecution of their inquiries, and have laboured hard to establish regular statistical returns of disease occurring in the metropolis. The Sanitary Association of Manchester and Salford has also applied itself with great zeal to the same object in respect of their population. At the International Statistical Congress, held in 1860, the importance of the accurate registration of diseases and of their results in hospitals throughout the kingdom was strongly urged in the Public Health Section, and steps were then taken to carry the suggestion into effect as regards these institutions. Such a measure is equally, if not still more, needed in respect of workhouse infirmaries. Among various other matters of great interest to the public health, on which useful information might be obtained from this source, the discovery of the amount of incurable blindness, deafness, and deformity among the poor may be mentioned. Every consideration thus shows how inestimable would be the value of a general and connected system of disease-registration among the pauper population over the entire country.

"The scheme proposed by the Society for this end is, that there should be a monthly return of the number of cases of illness treated by each parochial medical officer, and of the number of deaths among these cases—arranged upon such a plan as that in the annexed schedule, in which a few of the supposed details are entered, and the general results given, to indicate the mode of filling it up. But the exact form best suited for the purpose will doubtless need much consideration. The great object sought for is to turn to useful account the statistical records of disease now required to be made by the medical officer, but which, hitherto, have been valueless and unknown; and thus to furnish a ready means for ascertaining, from month to month, the nature, the extent, and the gravity of the sickness among the poor prevailing in different districts of the country, together with the approximate ages of the sick, and a brief notice of the local circumstances affecting the health of the people. That the parochial medical officers would very generally afford willing co-operation in the carrying out of the proposal, the Society anticipate with confidence; none of their professional brethren have shown themselves more active promoters of every reasonable measure for improving the condition of the poor, and for advancing the best aims of the healing art, than these gentlemen. What has been done by the medical officers of the metropolis and of Manchester and Salford would doubtless be done elsewhere. The labours of each and all, by becoming instrumental to an important scientific and social end, would rise in public usefulness, and therefore in public esteem; for whatever exalts a profession in character, is sure to strengthen it in the long run in influence and weight.

"By the monthly returns being regularly transmitted to the Poor-Law Board, or to the Medical Department of the Privy Council, the current state of the public health over the country would be, to a great extent, ascertained at short intervals of time, and the springing up and threatened prevalence of dangerous zymotic diseases would be discovered early, and before the leaven had leavened the whole mass. What is now being done by the Board of Trade for meteorological inquiries might, with no less advantage to the whole community, be done by another Government department for hygienic research. An annual report, founded on these monthly returns and embodying their chief facts and results, on the same plan as the annual reports of the health of the army and of the navy, could not fail to be of great scientific value. It would, moreover, be directly and immediately useful in various ways. The labours of local boards, for instance, would be aided and guided by the authentic information made accessible, and the results of these labours would become generally known. Thus the good example of one place would stimulate imitation in another; means and appliances, found useful here, would be copied elsewhere; and in this manner local experience would be made profitable to the whole community. No other country in the world possesses such facilities for the attainment of the object in view as England, for no other country has such a well-organized system of pauper relief; and when it is considered that nearly six millions sterling are annually expended for this purpose, it is but right that the working of the system should subserve, as far as practicable, the promotion of science and the advancement of the general good.

"B. G. Babington, *President.*
Gavin Milroy, } *Hon. Secs.*"
J. N. Radcliffe, }

"*April*, 1863.

We heartily hope that this attempt on the part of the Epidemiological Society to procure the utilization of the vast mass of material alluded to as already existing throughout the land, but now remaining unfertile, may be crowned with that success which it deserves.

Government Inquiry on the Subject of Leprosy.

The Committee appointed last year by the College of Physicians, at the request of the Secretary of State for the Colonies, is now engaged in analysing and collating the numerous replies already received to their interrogatories, which were printed in our number for January. These replies are from many of the West India Islands and from New Brunswick; from Constantinople, Smyrna, Rhodes, Cyprus, Crete, &c.; from Aleppo, Damascus, and Jerusalem; from Cairo, Tripoli, Tunis, and Sierra Leone; and from Hong Kong, Shangai, and Japan. No returns have yet come in from the East Indies or Ceylon, where the disease is known to be prevalent in many districts; but Dr. John Davy and Dr. Jackson have communicated the results of their experience in former years both in the East and the West Indies.

No endemic chronic malady is more widely spread over the globe than leprosy; and although none involves more sad and painful consequences to the sufferer, there is probably no form of disease which is so imperfectly understood by the profession, and in respect of which ignorance has given rise to more distressing results. The traditional stigma attached from the remotest ages to the mere name of "leprosy," and the well-nigh universal popular belief still prevalent that the disease is eminently contagious, have led to the most cruel treatment of the afflicted, who are generally either left in the most deplorable neglect, or are hunted out of society as if they were criminals rather than sufferers. This is the case at the present time in several of our own colonies, as well as in many foreign countries. Now, the prime question comes to be—Is the malady really communicable from the diseased to the healthy, as is generally imagined? We are informed that in a very large majority of the reports communicated to the Committee, the opinion is strongly and decidedly expressed in the negative. The authoritative settlement of this one point may alone lead to the most beneficent consequences. Meanwhile, all medical men who have had experience of the disease abroad will do well to communicate the results of their observations to the Committee of the College.

St. Thomas's and Bethlehem Hospitals.

Among several questions recently occupying attention, both of the medical profession and public, few have been so zealously discussed as the future sites of the above-named charities. To enter into the varied phases which this *vexata questio* has assumed would occupy too large a portion of our remaining space; therefore, we can only at present remark that neither party concerned—viz., the governors of these hospitals—seemed disposed at first to make any satisfactory arrangement, notwithstanding the great talk and protocolling which have ensued during many months between various contending influences. Irrespective of local interests, the prejudices of particular individuals favouring their own institution, or the desire expressed by certain parties to build the new hospital of St. Thomas in some country situation, away from the metropolis and its smoke, we must emphatically observe, that such prolonged debates have occasioned injurious consequences much to be regretted by the well-wishers of both institutions.

Although all admit that the present site of Bethlehem is admirably adapted whereupon to construct an hospital for patients labouring under bodily disease, the authorities of the above charity have refused 150,000*l*. as an inducement to quit their present premises and migrate out of town. This movement they will not make, notwithstanding it is almost unanimously recommended by the medical profession, advocated by the press, and warmly supported by the public generally. The chief reasons assigned for rejecting the liberal offer of St. Thomas's Hospital Committee are curious; and as these were enumerated at a public meeting of Bethlehem governors, they deserve being recorded, in order to show what kind of arguments partisans can urge against an advantageous proposition. Bethlehem, objectors said, ought not to be transferred to a rural site away from London—because 1st, it is now easy to transport London lunatics to the existing hospital at Lambeth; 2nd, relatives and friends can readily visit patients there without travelling any distance; 3rd, the asylum being in town, governors are hence able to inspect the institution more conveniently; while the official executive would find any country site less suitable; 4th, the charity ought to incur no expense whatever by being located elsewhere; 5th, the lunatic inmates like so much to gaze at the fine shop windows decorating Newington Causeway; and 6th, others seem so often delighted with feeding the ducks in St. James's Park, that to deprive them of these gratifications would be injurious in medical estimation.

In support of moving Bethlehem into the country the advantages are so obvious and universally acknowledged, that to enumerate them here seems almost superfluous. Nevertheless, it may be stated that one of the governors, a medical practitioner, who has taken an active part in this controversy——namely, Dr. Webster—stated, that the present hospital having been built about half a century ago, was far behind many similar receptacles, not only in Great Britain, but on the Continent, where new institutions for lunatics were being constructed much superior to the one it was proposed to remove. Even in Spain, Sweden, and Denmark, the respective governments are now building asylums which would excel Bethlehem in many essential respects, and these were all located away from crowded populations. London must follow this universal movement, and not appear retrograde in the race of benevolent emulation, especially as England at one time occupied the van, not the rear of improvement. Further, Government is removing the criminal lunatics to a country asylum; the City is also erecting a new building for their lunatic paupers near Dartford; the Commissioners in Lunacy have stated, in a late Report to the Home Office, that they strongly object to Bethlehem remaining in its present site, and Lord Shaftesbury is to bring the whole matter before Parliament.

However, as the Bethlehem governors have finally agreed to meet a deputation from St. Thomas's to discuss the points at issue, there is yet some hope that the late unseemly contest, rather about pecuniary remuneration than the advancement of science and the best mode of promoting benevolence, may still be brought to a satisfactory termination, which shall prove beneficial as well to patients suffering from bodily disease as those who are victims of mental alienation.

BOOKS, &c., RECEIVED FOR REVIEW.

A Clinical Memoir on certain Diseases of the Eye and Ear consequent on Inherited Syphilis, &c. J. Hutchinson, F.R.C.S., Senior Assistant-Surgeon and Lecturer on Surgery at the London Hospital, &c. London, Churchill and Sons. 1863. pp. 229.

A Year-Book of Medicine, Surgery, and their Allied Sciences, for 1862. Edited by Dr. Montgomery, Dr. Handfield Jones, Mr. Windsor, Dr. Graily Hewett, and Dr. Sanderson, for the New Sydenham Society. London, 1863. pp. 520.

A Yachting Cruise in the Baltic. By J. R. Graves, Commodore of the Royal Mersey Yacht Club. London, Longman and Co. 1863. pp. 399.

The Pharmacopœias of Thirteen of the London Hospitals. Arranged in groups for easy reference and comparison. By P. Squire, F.R.L.S. &c. London, Churchill and Sons. 1863. pp. 152.

Illustrations of Disease with the Microscope. Prize Essay. By F. P. Porcher, M.D. Part. I. Charleston, 1861. pp. 133.

Outlines of a New Theory of Muscular Action. (Thesis for degree of M.D.) By the Rev. S. Haughton, M.D., F.R.S. London, Williams and Norgate. 1863. pp. 23.

On the Phenomena of Diabetes Mellitus. By Rev. S. Haughton, M.D., F.R.S. (Reprint from Dublin Quarterly Journal of Medical Science, Feb. 1863.)

The Natural Constants of the Healthy Urine of Man. (Concluded.) By the same. (Reprint from the same, Nov. 1862.)

On the Cure of Club-Foot without Cutting Tendons, and on certain New Methods of treating other Deformities. By R. Barwell, F.R.C.S., Assist.-Surgeon to Charing-cross Hospital, &c. London, Churchill and Sons. 1863. pp. 224.

On Rupture, Inguinal, Crural, and Umbilical; the Anatomy, Pathology, Diagnosis, Cause, and Prevention; with New Methods of effecting a Radical and Permanent Cure; embodying the Jacksonian Prize Essay for 1861. By T. Wood, F.R.C.S., Assistant-Surgeon to King's College Hospital, &c. London, Davies. 1863. pp. 326.

The Present Aspect of the Doctrine of Cellular Pathology: a Lecture. By W. Turner, M.B., Senior Demonstrator of Anatomy in the University of Edinburgh. 1863. (Pamphlet.)

On Malaria, Miasmata, and their Influence in the Production of Typhus and Typhoïd Fevers, Cholera, and the Exanthemata; founded on the Fothergillian Prize Essay for 1859. By J. H. Barker, M.D. London, Davies. 1863. pp. 251.

Sur les Déformations Plastiques du Crane. Par Dr. J. B. Davis, de Shelton. (Memoir, written in English, translated and read at the Société d'Anthropologie, Août 21, 1862.)

Annales de l'Electrothérapie: revue des applications thérapeutiques de l'électricité et du magnetisme, de l'électrophysiologie, de la pathologie nerveaux et musculaire. Par le Docteur A. Tripier. No. 2. Avril, 1863. Paris, J. Baillière et Fils.

An Essay on the Senses of Smell and Taste. By Norton Folsom. Boston, 1863. (Pamphlet.)

Zur Diagnostik und Behandlung der Chronischen Laryngitis. Ein Laryngos Kopischer Beiträg. Von Dr. H. Vogler, zu Ems. Berlin, 1863. (Pamphlet.)

Dentition and its Derangements: Course of Lectures delivered in the New York Medical College. By A. Jacobs, M.D., Professor of Infantile Pathology and Therapeutics. New York, Baillière. 1862. pp. 172.

Practical Lithotomy and Lithotrity; or an Inquiry into the best modes of Removing Stone from the Bladder. By H. Thompson, F.R.C.S., University College Hospital. London, Churchill and Sons. 1863. pp. 277.

Transactions of the Epidemiological Society of London. Vol. I., Part III. London, Davies. 1863.

Studies in Physiology and Medicine. By the late R. J. Graves, F.R.S., Professor of the Institute of Medicine in the School of Physic in Ireland. London, Churchill and Sons. 1863. pp. 428.

The Causes and Treatment of Imperfect Digestion. By A. Leared, M.D., Physician to the Great Northern Hospital, &c. London, Churchill and Sons. 1863. pp. 222.

A Practical Treatise on the Ætiology, Pathology, and Treatment of the Congenital Malformation of the Rectum and Anus. By W. Bodenheimer, M.D. New York. pp. 368.

Du Fucus Vesiculosus (chêne marin, laitue marine), de ses propriétés fondantes et de son emploi contre l'obésité, &c. Par L. V. Duchesne - Duparc. Deuxième Edition. Paris, Baillière. 1863. (Pamphlet.)

Cours de Médecine comparée. Par P. Rayer, Membre de l'Institut, &c. Introduction. Paris, Baillière et Fils. 1863. pp. 52.

Infant Feeding, and its Influence on Life, &c. By C. H. F. Routh, M.D., &c. Second Edition. London, Churchill and Sons. 1863. pp. 462.

The Progress of Ophthalmic Surgery, from the Invention of the Ophthalmoscope (in 1851) up to the Present Time. By J. Z. Laurence, F.R.C.S., Surgeon to the Surrey Ophthalmic Hospital, &c. London, Mitchener. 1863. (Pamphlet.)

On the Causes and Treatment of Closure and Immobility of the Jaws. By Christopher Heath, F.R.C.S., Assistant-Surgeon to, and Lecturer on Anatomy at, the Westminster Hospital. Dublin, Falconer. 1863. (Pamphlet.)

Physiological Researches. By John Davy, M.D., F.R.S., Inspector-General of Army Hospitals, H.P. London, Williams and Norgate. 1863. pp. 448.

Sulla Cura Zuccherina del Diabete. Osservazioni del Prof. P. Burresi di Siena. (Esttratto del Sperimentale, Aprile, 1863.) Pamphlet.

The Nullity of Metaphysics as a Science among the Sciences. Set forth in Six brief Dialogues. London, Longman and Co. 1863. pp. 104.

Physiological Memoirs. By W. A. Hammond, M.D., Surgeon-General U.S. Army, &c. &c. Philadelphia, Lippincott and Co. 1863. pp. 348.

Outlines of Surgery: being an Epitome of the Lectures on the Principles and Practice of Surgery delivered at St. Thomas's Hospital. By T. Le Gros Clark, Surgeon to the Hospital, &c. London, Churchill and Sons. 1863. pp. 258.

Menton. Essai Climatologique sur ses différentes Régions. Par Dr. J. F. Farina. Baillière. 1863. (Pamphlet.)

The Calabar Bean as a New Ophthalmic Agent. By D. Argyle Robertson, M.D. (Reprint.)

Clinical Researches on Auscultation of the Head. By M. H. Roger, M.D., Physician to the Hospital for Children, &c. Translated from the French by Alfred Meadows, M.D., late Assistant-Physician for Diseases of Women and Children, King's College Hospital. London, Renshaw. 1863. (Pamphlet.)

On the Occurrence of the Malignant Pustule in England. By W. Budd, M.D. (Reprint from the British Medical Journal. Pamphlet.) Richards, 1863.

On the Formation of Phenic and Benzoic Acids from Benzole; also Analysis of Red Chalk. By A. H. Church, B.A. Oxon. (Reprint from Journal of the Chemical Society.)

On some Reactions of Hydride of Benzoyle. (Reprint from the Philosophical Magazine for June. Supplement. 1863.) By the same.

Note on Myxotrichum Chartatum (Kunze). By the same. (Reprint from the Annals and Mag. of Nat. Hist., Jan. 1862.) By the same.

Ulcus corrosivum duodeni. En Kasuistisk Sammenstilling af Dr. F. Trier. Kjöbenhavn, 1863. (Reprint from the Ugeskrift for Læger.) pp. 79.

On Cancer of the Female Sexual Organs. By T. H. Tanner, M.D. (Reprint from London Medical Review.) London, Renshaw. 1863. pp. 60.

The Urine in Health and Disease: being an Exposition of the Composition of the Urine and of the Pathology of Urinary and Renal Disorders. By A. H. Hassall, M.D., Senior Physician to the Royal Free Hospital, &c. &c. Second Edition. London, Churchill and Sons. 1863. pp. 416.

Waste: a Lecture delivered at the Bristol Institution for the Advancement of Science, Literature, and the Arts. By J. A. Symonds, M.D., F.R.S., Edinb. London, Bell and Daldy. 1863.

On the Cerebro-spinal Origin and Diagnosis of the Protrusion of the Eyeballs termed Anæmic. By T. Laycock, M.D. (Reprint.)

An Account of the Medical Evidence connected with the Trial of Jessie M'Lachlan, 1862. By G. H. B. Macleod, M.D. (Reprint from Glasgow Medical Journal.)

Essai sur la nature et le traitement du Choléra. Par A. Rodriques-Barraut, M.D. Ile Maurice, 1863. (Pamphlet.)

On the Anatomy of Nerve-Fibres and Cells, and the Ultimate Distribution of Nerve-Fibres. Three Demonstrations delivered by Prof. Beale at King's College, 1863. Abstract and Remarks by G. V. Ciaccio, M.D., of Naples.

Further Observations on the Distribution of Nerves to the Elementary Fibres of Striped Muscle. By L. S. Beale, M.D. (Transactions of Royal Society, received June 13th, read June 19th, 1863.)

On Human Entozoa; comprising the description of the different species of Worms found in the Intestines, &c.; and the Pathology and Treatment of the various Affections produced by their presence. By W. A. Smith, M.D., &c., Senior Assistant-Physician to the Metropolitan Free Hospital. London, Lewis. 1863. pp. 251.

Transactions of the Obstetrical Society of London. Vol. IV., for year 1862. London, Longman and Co. 1863. pp. 338.

Lectures on Surgical Pathology, deilvered at the Royal College of Surgeons of England. By James Paget, F.R.S., &c. Revised and edited by William Turner, M.B. Lond., F.R.C.S.E., &c., Senior Demonstrator of Anatomy in the University of Edinburgh. London, Longman, Green, and Co., 1863. pp. 848.

Reports, &c.; Reviews, Journals, &c.

Norsk Magazin for Lægevidenskaben, &c. XVII. Bind, 1 Hefte. Christiania.

Edinburgh Medical Journal. April, May, June.

Glasgow Med. Journal, No. XLI. April.

The Dublin Quarterly Journal of Medical Science, No. LXV. May, 1863.

The Medical Record of Australia, Vol. III., Nos. I. and II.

The Medical and Surgical Review (Australasian). Melbourne, April, 1863.

Report of the Committee of the Manchester and Salford Sanitary Association, for 1862.

Fifth Annual Report of the General Board of Commissioners in Lunacy for Scotland. 1863. pp. 226.

Fifth Annual Report of the Medical Superintendent of the Provincial Hospital for the Insane, Halifax, Nova Scotia. 1863.

Annual Report of the Richmond District Lunatic Asylum, Dublin, for 1862.

The Seventh Annual Report of the United Lunatic Asylum for Nottingham, for 1862.

State of the Norfolk and Norwich Hospital, from Dec. 1861 to Dec. 1862.

The Journal of Mental Science. Edited by C. L. Robertson, M.B. Cantab. April, 1863. London, Churchill and Sons.

Sussex County Lunatic Asylum. Fourth Annual Report, 1862.

Fifteenth Annual Report of the Somerset County Pauper Lunatic Asylum, 1862.

The Twelfth Annual Report of the Committee of Visitors of the County Lunatic Asylum at Colney Hatch, 1863.

Annual Report of St. George's Hospital for 1862.

The Stethoscope; a Quarterly Review of the Modern Practice in Consumption and Chest Disease. London, Davies. No. III.

Canada Lancet. Vol. I. Nos. 1, 2, 3.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1863.

PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *Beiträge zur Kinderheilkunde.* Von Dr. E. Hennoch, Professor der Medicin an der Königl. Universität zu Berlin und Dirigenten einer Poliklinik für Kinderkrankheiten.—*Berlin*, 1861. SS. 213.
Contributions to Pædiatrics. By Dr. E. Hennoch, Professor of Medicine at the University of Berlin, &c.
2. *Familiar Letters on the Diseases of Children, addressed to a Young Practitioner.* By James Bower Harrison, M.D., Fellow of the Royal College of Surgeons, &c.—*London*, 1862. pp. 197.
3. *Infant Mortality and its Causes.* By Arthur Leared, M.D., M.R.S.A., &c.—*London*, 1862. pp. 13.
4. *Excessive Infant Mortality: how can it be Stayed? To which is added a short Paper on Infant Alimentation..* By M. A. Baines. —*London.* pp. 20.
5. *Observations on some of the Causes of Infanticide.* By George Greaves, M.R.C.S., Lecturer on Midwifery and the Diseases of Women and Children, Manchester Royal School of Medicine, &c.—*Manchester*, 1863. pp. 26.
6. *The Practice of Hiring Wet Nurses (especially from the "fallen") considered as it affects Public Health and Public Morals.* By M. A. Baines.—*London.* pp. 16.

A natural inclination towards the study of the diseases of early life, strengthened by a clinical assistantship of seven years' duration in the service of M. Romberg, prompted Dr. Hennoch to found for himself a dispensary for the diseases of children, by which he could continue to foster his early bias. Notwithstanding the useful establishments connected with pædiatrics already existing in Berlin, the new founder received an ample share of patronage, six hundred cases being treated by him and his assistant, Dr. Aron, during the course of the first year's trial. As materials for generalization collected in the case-books, Dr. Hennoch conceived the idea that the results he might arrive at should not be altogether devoid of interest to his professional brethren. He therefore decided on placing his inferences before them; and hence is explained the origin of these "contributions," which are based upon the experience of between four and five hundred cases. Had our author been either a tyro in his art, or a novice in the "speciality" he was dealing with, it would have been preferable that he should have permitted more than one year to have passed over ere he favoured us with the experience drawn from his establishment. But, as it happens, Dr. Hennoch is well known as an able and experienced clinical teacher; hence the work before us may be consulted with advantage by such as desire either to refresh or strengthen their knowledge of the pathology of childhood and infancy. It may be said that the chief purport of it is to show how far the author's own cases bear out the usually accepted histories of the particular maladies referred to, and to what extent and wherein they fail to do so. This renders the account the author gives more fitted for one already tolerably acquainted with the general subject, than for the commencing student of the pathology of childhood. Diseases of the nervous system are first discussed: amongst them we may refer to the examples given of tubercular meningitis, as illustrating that variety of the malady in which the initiatory vomiting lasts for a very long period, and where this, combined with its isolation as a symptom, may give rise to the formation of a wrong diagnosis. A child not presenting any other trouble than vomiting pretty continuously for a whole week, may readily lead the inexperienced away from the suspicion of cerebral mischief. But Piet's instance of 9 days' initiatory gastric regurgitation exceeds the 7 and 8 days of Hennoch and Romberg. When the disease quickly passes into the second stage, the emesis may continue 12, 15, or even 25 days, ceasing, in fact, only with death, (Rilliet and Barthez.) The author discusses the question as to whether (as many maintain) there are any signs by which the vomiting from insidious brain diseases may be distinguished from that attendant upon indigestion, "febrile gastric catarrh," &c., in cases where the emesis is the only symptom present. Certainly, considerable trouble has been taken to demonstrate such, but with little satisfaction, as Dr. Hennoch considers. It is true that the influence of the upright position in exciting cerebral vomiting is very frequently observed, but it is not always so; and in one of Hennoch's cases, the child could repeatedly sit erect in its mother's lap without the slightest emesis following, whilst in another very frequent vomition ensued when the little patient was kept in the recumbent posture. On the other hand also, dyspeptic vomiting may frequently be induced by the sudden removal from the lying into the upright position. Even the constipated state of the bowels, which forms so general an accompaniment to the vomiting of brain mischief, may not only be wanting, but actually diarrhœa may be present. The want of a feeling of nausea, as preparatory to the gastric regurgitation in brain disease, is a point upon which we may very readily go astray in the case of children. The remembrance of these and analogous facts will show that over-much ,dependence should not be placed upon any single symptom in the earlier stages of brain disease in infancy.

Under diseases of the respiratory organs we meet with the following case :—Gustav C——, eleven weeks old, was attacked by cough upon the 23rd of February. On the 25th, he exhibited all the signs of a highly acute pulmonary affection. There was frequent cough, extreme dyspnœa, moaning expiration, high temperature, a pulse of 196, and very small, and the breathing was frequent and irregular. Percussion was everywhere duller than normal; the respiratory murmur was universally very sharp or acute, with a bronchial "timbre" anteriorly and laterally upon the left side of the thorax; there was, here and there, slight moist rattling. At eleven the same night the child died. At the necroscopy several small patches of atelectasic lung were found upon the posterior surfaces of the lower lobes; these patches were readily inflated; the small bronchial twigs alone traversing them were filled with muco-purulent matter. Not a trace of pneumonia existed; the bronchial tubes, with the exception of those just mentioned, were universally void of secretion; whilst the mucous membrane, from the bifurcation of the trachea to the smallest ramifications of the bronchial tree, was deeply reddened, and by so permanent a colour, that it remained when the mucous membrane of the small transparent bronchia was removed from the pulmonary parenchyma. We give some of the author's comments in his own words:

"What is particularly striking in this case—running its course within forty-eight hours—is the want of secretion observed almost throughout in so intense and diffused a form of bronchitis. A very small portion only of the smaller bronchi closely connected with the atelectasic portions of the lungs was filled with muco-purulent matter, and which, in fact, was the cause of the atelectasis. In by far the greater number of the air-tubes not a drop of fluid was met with, in spite of the intense redness present. With these conditions, too, the physical signs existing during life agreed. Scarcely any other view remains to be taken than to regard the case as one of a rapidly supervening and progressing hyperæmia with subsequent tumefaction of the bronchial mucous membrane, which, through diminishing the calibre of the air-tubes, obstructing the entrance of air, and hindering the oxydation of the blood, put an end to life before the stage of secretion was attained. Since Rilliet and Barthez, in reference to this point, observe (loc. cit. 454)—'Il est vrai que la preuve anatomique manque, car l'autopsie n'est jamais faite avant la secrétion des mucosités; et le fut-elle, elle ne donnerait probablement aucun résultat, le gonflement ayant disparu après la mort.' I may add that, so far as the latter statement only is concerned, is the present case a testimony. The writers in question draw attention to analogous circumstances connected with other mucous membranes, *e. g.*, of the nose, of the larynx, of the trachea, in which, in consequence of a suddenly supervening catarrh with swelling of the mucous membrane, a partial obstruction of the affected tubes with corresponding symptoms may be developed, and by which, for example, in the latter case, the phenomena of pseudo-croup may be produced. Such congestive states pass, it is true, not infrequently into the stage of secretion (nasal and tracheal catarrh); but they can also, as daily experience teaches, subside without having reached the latter. Rilliet and Barthez believe the same to hold good as regards the bronchi, and remark—'Nous avons vu des cas dans lesquels un accès de dyspnée avec râle sibilant disparaisait sans qu'il eut à la suite ni rejet de mucosités, ni râles bullaires; mais nous le répétons c'est seulement par analogie et d'après des preuves symptomatiques qu'on peut admettre ces congestions subites avec gonflement de la muqueuse bronchique.' As it is, then, I believe that *actual proof* of the position is afforded by the above case." (p. 35.)

Under this division may be found some good remarks upon pulmonary tuberculosis. (p. 61.) Of the latter, a case is detailed in a child only four months old, in whom a considerable cavity existed in the lung, the smooth parieties of the vomica leading to the belief that the latter was not of recent formation.

"Gastric catarrh," remittent fever, and "abdominal typhus" are critically discussed by Dr. Hennoch; but we cannot say that we have received much enlightenment from the author's endeavours to explain the nature of all cases of so-called "remittent fever." No one at present doubts that a pyrexial state of a remitting character is common in early life as the constitutional reaction of many different local maladies, nor that this state so related has been often looked upon as a primary or idiopathic fever, and described as "infantile remittent." But the question still remains unanswered—viz., what is the exact nature of the remitting febrile condition in children, which, so far as we can see, is neither typhoïd, nor the secondary, nor symptomatic fever of a local lesion. In reference to this we do not receive any help from Dr. Hennoch. To pass to the digestive, &c., organs, we may observe that the extract of nux-vomica—first recommended by Schwartz—has been found by the author highly serviceable in the treatment of prolapsus ani. Duchaussoy and Foucher have likewise praised this agent as increasing the tonic and contractile power of the sphincter. The former applies strychnia epidermically (as it were) upon a small blistered surface near the anus, whilst the latter injects a small quantity of a solution of strychnia into the anal subcutaneous cellular tissue, by means of Pravaz's syringe.

A subject to which Dr. Hennoch has devoted considerable attention is that of *Rachitism.* We have ourselves upon two previous occasions (vol. xviii. p. 6, vol. xxvii. p. 371) discussed this morbid state with some minuteness. We must perforce refer to it again, however; for when dealing with the work of a German author, the latter will assuredly expect to awaken our curiosity when he touches upon a malady known to Continental medicine as 'Der Englische Krankheit.' But we deny the "soft impeachment" here involved, though we may listen to Dr. Hennoch. The experience of our author tends to prove that rachitism is not more common in one sex than it is in another, and it negatives the doctrine of Friedleben, that its maximum of frequency occurs just before and after the completion of the first year of life. By far the greater number of the Berlin cases occurred between the ages of fifteen months and two years and a half. It is well-known that Friedleben strongly opposes Elsässer's teaching, that in cases of "cranio-tabes" pressure upon the brain is liable to take place through the attenuated spots of the occipital bone, by which certain spasms and convulsions may be produced. In a case of Dr. Hennoch's, which answered to the description of Elsässer's "cranio-tabes," neither spasm of the glottis nor convulsions occurred, although it was evidently painful to the child for the head to rest upon the same side as that on which the soft cranial spot existed. On the other hand, in a rachitic boy a year and a half old, with compressible margins to the lambdoidal suture, but with a firm occiput, very severe fits of spasm of the glottis and eklampsia were observed.* In association with rachitism, the author has bestowed much attention upon "cerebral auscultation," to which subject we have before directed the attention of our readers. (vol. xxvii. p. 374.) In 24 rachitic children with open fontanelles, and whose quietude permitted of careful examination, a distinct bellows sound was perceptible. In one case only where the fontanelle was closed, was it to be heard; and here, as in Hennig's and Wirthgen's cases, the sound could be distinguished at various portions of the skull. In reply to the question as to whether this systolic sound over the anterior fontanelle be of pathologic import, or simply of physiologic nature, Dr. Hennoch confesses that he is much in the same position as was M. Rilliet :*

"Until some months back I have examined almost only *rachitic* children in reference to the murmur. I cannot therefore, from my own inquiries upon healthy or non-rachitic children, venture to give a definite opinion upon the matter. This much only can I say, that in at least 25 children who did not present any rachitic evidences, and in whom the anterior fontanelle was more or less membranous (in correspondence with the age), the blowing sound could not be perceived in spite of careful attention. In one boy (aged one year) only, with a very small fontanelle, who, with the exception of having *eczema capitis*, appeared blooming and healthy, did we hear a very loud systolic murmur over the fontanelle, whilst the heart and carotid sounds were normal. This single example, nevertheless, appears to me sufficiently important to render us careful in judging of the *negative* results belonging to the other series of observations." (p. 173.)

Dr. Hennoch's experience agrees with that of Rilliet, Barthez, and Bouchut—viz., that the cerebral murmur does not exist in cases of chronic hydrocephalus. This statement is opposed to single observations of Fisher, Hennig, Wirthgen, and even of Roger. Admitting that further investigations are necessary before we can satisfactorily determine all the fundamental points connected with the subject, the author does not hesitate to say that there is sufficient evidence before us to prove that rachitism is the condition of the infantile system with which a systolic bruit over the anterior fontanelle is by far most frequently associated. If the fontanelle be open (and such is in general the fact) the murmur is nearly constant, if it be closed it is wanting. The practical character of the "contributions" forbids their author entering upon a critical analysis of the opinions of different writers touching the actual locality and mode of production of the sound. He remarks, however :

* See vol. xviii. p. 67, of this Review. † Gaz. Méd., 1859, p. 762.

"According to my belief, the cerebral blowing springs from the large arteries running at the base of the brain (the carotid and its larger branches), the small vessels of the convexity being, as Hennig has already observed, too small to produce such a murmur. . . . . I am not yet in a position to explain even approximatively *how* it is produced. This much only is clear, that the sound is not likely to be conducted from the heart, as in all cases, even in those where a carotid blowing existed, the sounds of the heart were perfectly pure." (p. 178.)

That the murmur is due to an anæmic state of the blood is doubted by the author. Friedleben has advanced the peculiar doctrine that the fundamental cause of rachitism is to be sought for in a continuous disturbance of the function of respiration. So far as the anatomic facts go upon which this theory is based, they are opposed by Dr. Hennoch, who regards the deformity of the thorax and the lesions of the lungs as secondary, and not primary, to the other manifestations. Friedleben's investigations, speaking generally however, are praised by our author.

In *Syphilis*, even in its worst forms, the osseous system of infants is extremely rarely affected. A few instances in which it has happened to be involved are upon record, and Dr. Hennoch informs us that he has met with an example in which destruction of the vomer and adjacent muscles followed upon syphilitic coryza. (p. 192). The last chapter but one is occupied by the details of a case of "bronzed skin" and disease of the supra-renal capsules. The patient was a boy only twelve years of age. After a treatment of about three months death took place. The whole surface was very deeply coloured, especially the face, neck, and backs of the hands. The following was recorded at the post-mortem examination:

"The left supra-renal body is very firmly united to the capsule of the kidney, and is scarcely 1″ long, $\frac{1}{4}$″ broad, and $\frac{1}{4}$″ thick. Upon the inferior surface yellowish-white cheesy points are apparent. Upon section, nearly the whole organ appears to be converted into a dry, very firm white mass, which at the upper part only still allows of a thin lamella of normal gland-substance to be seen. The right supra-renal body is very firmly attached to the under surface of the liver, and from which it is but with difficulty separable; it is only $1\frac{1}{8}$″ long, $\frac{3}{4}$″ broad, and about $\frac{3}{10}$″ thick. At its anterior inferior portion some calcareous spiculæ project from the surface. Upon section here also, nearly the entire substance exhibits a dry, firm, yellowish-white, somewhat transparent character, with isolated firm white spots interspersed. Both kidneys are nearly unchanged. The microscopic investigation of H. von Rechlingshausen showed that the dry yellowish-white substance into which both supra-renal bodies were almost entirely converted, consisted solely of new-formed thick connective-tissue, so that, consequently, the case was one of *induration, or fibroïd degeneration of the supra-renal bodies.*" (p. 204.)

"*Atrophia infantum*" brings to a close Dr. Hennoch's contributions. By this condition the author implies the well-known consequences of a deficient or a defective nourishment. How frequently these results come across our own path, in a metropolis whose streets have been figuratively said to be "paved with gold," and where "no one need starve unless he chooses," it is scarcely necessary to say. The memoirs referred to at the head of this article will, if need be, furnish further evidence. To quote from Dr. Leared:

"About one-fourth of all the children born in this civilized country perish miserably in the first year of their existence. No such mortality can be observed amongst the young of our domestic animals. More favoured than the lord of creation himself, the horse, the cow, and the dog pass through the stages of immaturity unscathed. It is impossible that such lavish expenditure of life, in its most important and exalted form, was intended by the Creator. It would seem that while simple instinct proved an infallible guide to the lower animals in the preservation of their young, the free agency of man caused an indifference to the strongest of all natural obligations, and that the results of apathy and ignorance are mistaken for those of an inevitable law." (p. 3.)

"Of all causes of death, that which concerns the nutrition of the infant is the most potent. If the food supplied him is either scanty or in excess, or of improper quality, his delicate organism soon succumbs. 'Want of breast-milk' holds a prominent place in the weekly death-roll of the Registrar-General, and to this cause a large percentage of the mortality from all causes of children under one year old is ascribed." (p. 5.)

That proper maternal nursing would save a large percentage cannot be doubted; but as it is equally clear that a large percentage will continue to be "dry-nursed," what, it may be asked, should infants be fed on? This question we lately discussed when noticing Dr. Routh's treatise 'On Infant Feeding,' in our twenty-ninth volume, p. 58. Suffice it here to refer our readers to the work in question, and to the present tract of Mrs. Baines, in which she remarks:

"It would be curious to know how many of the deaths returned in the Registrar's reports under 'want of breast-milk' are those of children who were attempted to be reared (but in fact were starved) upon animal milk alone. I fancy the numbers would quite equal those of the children who die in convulsions from over-feeding. . . . . There are two extremes prevalent, both equally to be deprecated—namely, the excessive use of farinaceous food, and its entire prohibition." (p. 18.)

What with no-feeding, under-feeding, and over-feeding, society certainly does manage to get through every year a very successful rehearsal of the "Slaughter of the Innocents." A very large proportion of women who either do not at all or insufficiently or improperly suckle their offspring are led to the neglect by some social or physical necessity which they cannot easily overcome. A woman will not starve, nor let the children she may have already reared want for nourishment so long as she can labour to feed them. She will consequently go during the day to the work-room or factory, and leave her infant for others to look after. A woman who has "fallen," and is abandoned by her seducer, rises from her sick couch to ask, Where am I to go? What am I to do? How am I to live? The reply is before her: "I will sell my milk to nourish the children of such as will well reward me for it;" and she leaves her own child for others to look after. And what is the fate of it? Let us follow its fortune as graphically described by Mr. Greaves:

"In a miserable hovel situated in a dark entry in the worst part of some large town, a miserable infant lies on the knees of an old woman, who is cramming into its mouth with a spoon food of a kind which its stomach is utterly unable to digest. With the morbid appetite which its condition gives rise to, the child takes the food greedily, and then sinks into a half-torpid slumber, to wake in an hour or two, screaming with the agonies of indigestion. It is either fed again, or most probably a soothing dose of some narcotic medicine is administered, which produces a much longer interval of torpor. From this the child wakes unrefreshed, feverish, and thirsty. Its cries are silenced by more food, followed by another dose of physic. The effects of such treatment soon manifest themselves. The child, previously perhaps a very embodiment of health and vigour, begins to pine; its limbs waste, and its belly becomes protuberant. Diarrhœa sets in, convulsions follow, and after a longer or shorter period of suffering it dies." (p. 17.)

What remedies have hitherto been proposed for these evils? In connexion with the first kind, "nurseries," at which the working mothers can leave their children to be well looked after during their absence, have been established, and endeavours have been made by "Ladies' Sanitary Associations," &c., to inform those with whom infants are likely to be left, how to feed and manage them under the circumstances with as little detriment to their health as possible. Here good intentions are not all that are requisite, for children fed upon cold sausages and sweltered in a "ground-heat" of dirty blankets, are not likely to prove flourishing plants. Efforts have likewise been made to enforce the rule upon the employers of women who are mothers, that they shall allow the infants to be brought at stated times to be suckled by their parents. But need we say how utterly insufficient these endeavours have been in significantly diminishing the evil we are deploring? With respect to the second source of neglect, physiologists have been urging upon aristocratic mothers how much they lose and what danger they run, in yielding their children to be suckled by strangers, whilst philanthropists and social reformers have been arguing that the practice of hiring wet-nurses ought to be put a stop to, or at least placed under some kind of supervision. On the one hand:

"I maintain," says Mr. Greaves, "that no man has the right to diminish by ever so little the chances for life of another infant, in order to add to those of his own. It has pleased the Almighty, by the death or sickness or feebleness of its mother, to deprive his child of its natural nutriment. Availing himself of all the resources of modern science which his means may enable him to purchase, he must endeavour by hand-feeding to bring up his babe. He must not, if he would escape a fearful amount of responsibility, employ his wealth to purchase that which is the birthright of another child." (p. 19.)

On the other hand, exclaims Mrs. Baines:

"We must admit no compromise; . . . . the demand for wet nurses continues, and it consequently turns upon *single women*, so that as the demand in this direction increases, the supply will be recruited to meet it—the inference is obvious." (p. 13.)

Doubts have been expressed in some quarters as to whether the practice of wet-nursing prevails to the extent implied by such writers. Mrs. Baines refers as proof that it does so, to the various lying-in institutions of the metropolis known to recommend poor women for that office. Mr. Greaves writes:

"How frequently we see an advertisement for a wet-nurse. There is kept in a certain place in a populous town which I shall not designate by name, a register of wet-nurses. I had, not many days since, an opportunity of examining that register, and found that in the course of the last year two hundred women had signified their readiness to desert their own offspring for the care of that of others. That register has to my knowledge been open for the last twenty years: it may have existed thirty, forty, or fifty years. Let us try to conceive the amount of misery and death which it has occasioned—the number of innocent babes who have been tortured into a premature grave." (p. 17.)

That we should hold a tighter rein over all such mothers as would, either from mere inclination or mistakenly assumed necessity, deprive their own offspring of their immediate care, is apparent. But there is a class who fall into the crime in the simple purpose of getting bread to eat. And with this, such arguments as those above referred to can have but little effect. Supposing it were made illegal (except under stringent medical certificate) that Lady A—— should hire her "fallen sister," Mary B——, to suckle her youthful heir. Will it be supposed that Mary B—— will then quietly stay at home and tend her own infant? Possibly in a few cases it might thus happen, but in by far the majority, Mary B—— is only willing to be hired by Lady A—— because she is "cast out of house and home." The danger of superficially discussing subjects of this kind is precisely this—that a matter which is exceedingly complicated is made to appear exceedingly simple. The attention is diverted from full one-half of the facts that are necessary to enable us either to form a correct judgment, or to properly legislate for the future. There is always a very large number of persons who are on the very verge of error—nay, a very large number of people who are on the verge of crime. What proportion of these shall pass the boundary, and what shall be the nature of the error they shall commit and of the crime they shall perpetrate, depends very greatly upon the nature of the temptations surrounding them. If we become, for example, too stern with necessitous and "fallen" mothers, abortions, still-births, and infanticide will soon augment. If we show ourselves uncompromising with the fashionable parent, "mishaps" might possibly become more frequent in circles where they are at present only occasionally known. Mr. Greaves himself writes:

"I have known a married woman, a highly-educated, and, in other points of view, most estimable person, when warned of the risk of miscarriage from the course of life she was pursuing, to make light of the danger, and even express her hope that such a result might follow. Every practitioner of obstetric medicine must have met with similar instances, and will be prepared to believe that there is some foundation for the stories floating in society, of married ladies, whenever they find themselves pregnant, habitually beginning to take exercise, on foot or on horseback, to an extent unusual at other times, and thus making themselves abort." (p. 15.)

"I have read with great regret some remarks made by a female member of the English aristocracy, in a work which has been deservedly popular. Lady Llanover, the editress of the 'Life and Correspondence of Mrs. Delany,' ridicules 'the violent outcry of the present day against wet nurses,' and the *sentiment* of this century among the higher classes, that every mother (be her health or avocations what they may) ought to nurse her own child." . . . . "It is *not* necessary that a gentlewoman who wishes to nurse her child should go to bed at nine, rise with the sun, and walk about with it the whole day." (p. 25.)

That there is much to be deplored in all this does not admit of question. But hasty legislation or uncompromising sternness would unquestionably defeat their objects. This we very recently (in an article upon Illegitimacy and Infanticide) showed was the case in respect to a related subject.

"I have good reason to believe," writes Mr. Greaves, "that facts have recently come to the knowledge of the police in London which lead to the suspicion that in certain districts of the metropolis, an organized system of fœticide is in operation, sheltered by the allegation of still-birth. It is said that a certain number of female practitioners, when engaged by a pregnant woman to attend her, inquire whether she wishes for a living child or not, adding that their fee for the delivery of a living child is so much, for one still-born so much more." (p. 13.)

If there exist motives—and who can doubt it?—inclining women to wish for dead children and to neglect their living infants, we shall best prevent these crimes by the removal of the motives. Whilst we now write, the following case is reported in the daily papers:

"HAMMERSMITH.

"*Distressing Case of Child Desertion.*—Emma Gale, a genteel-looking young woman, was charged with deserting her illegitimate male child, aged two years, who was found crying on the step of a door in Brunswick-gardens, Kensington, on Saturday night last. Police-constable Mitchell said that on Sunday night, from information he received, he went to No. 40, St. James's-square, Notting-hill, where he found the prisoner in service. On telling her that she was charged with leaving her child in Brunswick-gardens exposed to the air, she stated that she went to the workhouse, where her child was refused admission, and having no place where she could take it, she left it in the gardens. She also said that the child had been out at nurse since it was three months old, and that the woman brought the child to the house on Saturday night and put it inside her (the prisoner's) master's door, at the same time telling her she would not keep it any longer, as she was in arrears of her payments. She asked the woman to keep the child until Monday, to enable her to find another nurse, but she refused. Herring, the porter at Kensington workhouse, said, that about a quarter-past ten o'clock on Saturday night, the prisoner brought the child to the gates, and a note. She wanted to leave the child, but he told her that he could not take it in, and that she must see the relieving-officer on Monday. The prisoner went away, and the child was subsequently brought to the workhouse by the police. Mr. H. E. Brooken, the prisoner's master, came forward to give her an excellent character. He said the prisoner left his service some time ago on her own account, and about ten days since she returned, they being in want of a servant, and satisfied with her previous conduct. He also said that when the woman brought the child to his house, he wrote the letter referred to by the porter, and he had no doubt that in a moment of desperation, not knowing where to take the child, she left it as has been described. If his worship would overlook it he promised to take her back into his service. Mr. Ingham said it was a very sad case. There was a great deal to be said for her; but the child might have died. It was a very serious case, and he should not like to decide upon it at a moment's consideration. The prisoner, who cried bitterly, said she supported the child as long as she could. She had been out of a place for some time, and had been unable to keep up her payments.

Mr. Ingham told her that if the child had died she would have been tried for murder. He had no doubt that she did not intend that it should die, but hoped that some person would find it and save the child. Mr. Brooken wished to know what would be done with the child in the interim. Mr. Edmonds, in answer to a question from the bench, said they could not admit the child into the workhouse without the mother. It was against law, and that was the reason the porter did not admit the child. Mr. Ingham said that before he could liberate the prisoner he must have an assurance that the child would be properly taken care of, and that it would not be again exposed. The prisoner was put back for an arrangement to be made, and on being placed in the dock shortly afterwards, a promise was given that the child would be properly looked after during the week. Mr. Ingham then remanded the case for a week, and took Mr. Brooken's recognizance for the prisoner's appearance."

If we do not make it worth an erring or necessitous woman's while to let her child be born alive, nor help her in her distress to rear it, we fear that "preaching," any more than "flogging" will not have any permanent effect. Dr. Leared thinks that "mothers might be taught that it is a crime to abandon their children for the sake of increasing their income, so long as the bare necessaries of life can be secured by their husbands." (p. 13.) Mr. Greaves, alluding to pregnant single women, observes:

"They are generally sent to the workhouse. Let them always go there. But once there, let every such woman remain until her child is old enough to be weaned. At present she may, and almost always does, go out at the end of the month, taking with her her infant, of whom no more is heard until its death is reported. . . . . But it may be said that a workhouse is not a prison, that the master possesses no such powers of detention as are here indicated. . . . . If the workhouse were what it might be made, a reformatory school, the forced detention for ten or twelve months might be a precious moral seed-time, promising rich fruit for time and for eternity." (p. 23.)

The truth is that there are no forms of error so completely beyond the usual recognition of the "law of kindness" as those which a "fallen" woman betrays to the world. And what must also be confessed is, that they are indebted for such banishment from our mercy mainly because of the violent arguments of the more fortunate, if not the more virtuous, of the female sex. If only a small portion of the sympathy which is the lot of the condemned murderer was accorded to the pregnant single woman, we should hear far less of still-births and of infantile mortality than is now the case. To talk, however, of according *sympathy* to such a person, we know full well will make numbers stand aghast. Many, no doubt, have perused, and possibly with tears in their eyes, the following description of the last moments of Henry Carter, the Sunday-school teacher, who was hung for a most cruel murder whilst we were writing this:

"Since his condemnation [said the 'Birmingham Post'] the prisoner has passed a great portion of his time in religious exercises and in communicating with his friends. These letters abounded with expressions of affection and good-will to old acquaintance, neighbours, and relatives, and contained many references to his hopes of salvation. They were also freely interspersed with scriptural quotations. On Tuesday last, he wrote a letter to Alice's [the girl he had murdered] mother, in which he expressed a hope that 'the Lord would forgive her *her* sins, and that he should meet her in heaven.' In answering questions from his father, he said he had made his peace with God, and had that day partaken of the sacrament."

With the false and sickly sentimentalism which can find "comfort," as the phrase goes, in such hypocrisy as this we have no share; thousands have, however, a very large one. Would that they might place their affections upon a more legitimate object of compassion and mercy! Are we going too far when we maintain the "fallen" and necessitous mother to be one?

With regard to Dr. Harrison's "familiar letters," we have scarcely any observation to make beyond the expression of the wish that the author had been pleased to present his little book of simple and useful precepts to "a young practitioner," without previously disfiguring it by such remarks upon his medical brethren as follow:—

"Professional men have written rather with a view to display their own ingenuity and learning than to benefit those engaged in the treatment of disease. They have consequently said many things not absolutely true, and represented others in false colours; they have treated of doubtful matters as if they were matters of certainty, and of difficult things as if they were easy; they have spoken of useless remedies and valuable ones in the same tone of praise; and where no medicine was to be relied on they have not had the candour to avow it." (p. vi.)

"Some medical men seem to take a pleasure in tedious manipulations and unnecessary refinements. Then, medicines are given which are disgusting to a child, though they may not taste very bad to an adult; and often they are given more from routine than because they are really necessary. A good third, if not more, of the medicines given to children might be dispensed with advantageously." (p. 9.)

"It seems odd that medical men should give medicines, and confound the effect of such medicines with the disease; yet such is really the case." (p. 29.)

"It would be well if we had more honesty in the profession as to the real value of remedies in particular cases; and then I think we should not so often send our patients to quacks and pretenders." (p. 158.)

The author's volume is one which is not unlikely to fall into the hands of the public; and the latter will certainly not receive any advantage, nor hold the profession in any higher repute, from these animadversions of the writer upon his medical brethren. Digressions of this kind in a scientific book always detract from its value in the opinion of such as are the best able to judge of the truth of the matter. Though Dr. Harrison intended his letters to be "familiar," there was not any necessity that they should be "smart," or facetious.

REVIEW II.

A Manual of Psychological Medicine; containing the History, Nosology, Description, Statistics, Pathology, and Treatment of Insanity. With an Appendix of Cases. By JOHN CHARLES BUCKNILL, M.D., London, &c.; and by DANIEL HACK TUKE, M.D., &c.—*London*, 1862. pp. 600.

IN our review in the number for October, 1858, we gave a very favourable account of the first edition of this work. It was a volume of ample size, reaching to 560 pages. This, we regret to see, in its revision, is still larger, extending to 600 pages. Should another edition be called for, as is probable, we hope its authors will do their best to reduce it to more moderate dimensions, befitting its title of a manual, which we think quite practicable by the exercise of a critical judgment, and the use of a concise style, even with the addition of new matter. The points which appear to us most admitting of curtailment are those which are in a manner introductory, such as the first, second, and third chapters. These are mainly historical, and yet little informing, except as showing that no race of men, in any time or country, has been exempt from mental disease, but in what proportion cannot be determined, owing to the want of reliable statistics; seeming, however, to justify, on the whole, the conclusion that what is called civilization increases the tendency to insanity. We use the word civilization in this qualified sense, believing, as we do, that real civilization—that which conduces to the improvement of society, to check vice and dissipation, to promote virtue and sobriety—will and must have a contrary effect, resulting in the "*mens sana in corpore sano.*" And we lay stress on this, seeing that the words of our authors apparently imply, that with civilization the risk of the spread of the disease must increase. Thus, in the opening page they state—"our conviction is that, "*other things being equal*, civilization does, on the whole, tend to render men more liable to mental disease." And in concluding the third chapter, "Modern Civilization in its bearing upon Insanity," they express nearly the same opinion. Adverting to a better, a perfect civilization, which should exactly temper the forces, moderate intellectual exertion, and banish intemperance (of which they despair), such would, we believe (these are their words), present more danger to the integrity of the great centre of the nervous system than a state of barbarism." This is a very sad conclusion, and we believe an erroneous one. We think, in regard to the etiology of insanity, they attach too much importance to mental and bodily labour, and too little to the vices coincident with that very heterogeneous condition of so-called civilized life.

Whilst we think some parts might be curtailed, others, we are of opinion, might be amplified, such as the fourth chapter, "On the Condition of the Insane in Modern Times." All that Dr. Tuke has stated on this important subject is very interesting; but much more information on it might be given by extending the details to the management of lunatic asylums in foreign countries; in some of these, showing a progressive improvement, as in our own; in others, little or no advance; the one in accordance with the progress made in science, the other in accordance with the torpor of intellect, distinctive of the peoples amongst whom the sciences are not cultivated.

The change in the treatment of the insane which has been effected in so short a period, little exceeding a quarter of a century, is one very delightful to think of, and a strong proof, as we hope it may be considered, of a civilization advancing in the right direction. It has so happened that we have seen instances of its whole progress in all its degrees. What a contrast there is between a lunatic asylum of the present period and the past! Very recently we visited a county asylum solely for the reception of pauper lunatics: the air of the rooms was untainted and fresh, the bedding scrupulously clean and good, the patients clean and decently dressed; everything in order, indicative of propriety and even comfort, and not one, either man or woman, was under any bodily restraint. Nearly a quarter of a century ago, it befel us to make a like visit to the place of confinement of the insane in the great capital of the East—Constantinople—which we could not call an asylum. It was contiguous to a menagerie; in one court were the dens of the wild beasts, in the adjoining the dens—they deserved no other name, they were so similar—of these pitiable persons, and like the beasts they were all in chains, and like them exposed to public view, and liable to be made the sport of boys and of thoughtless persons. This abomination, we believe, was shortly abolished; and it is some satisfaction to us to think that a representation we were in a position to make conduced to the introduction of a more humane system. Only a few years earlier, we witnessed in one of our own asylums, an asylum which at the time was inferior to none in the country in point of management, an instance of the old mode of coercion which we shall never forget. The individual, in solitary confinement, received us as if he had a horror of his fellow-men, venting his feelings in the most abusive, profane, and abominable language, and spitting at us, not being able to reach us, being tied down, arms and legs, in his bed; under which restraint it was said he had been for weeks as a measure of necessity. Shortly, as an experiment, the binding cords were removed, he was taken from his solitary cell and put into a room with other patients, and with the best effect, his ferocity leaving him as if charmed away.

In the comments which we may have to offer we may occasionally again use, as we have already, the term authors, inferring that though we learn from the preface that we are indebted to Dr. Tuke for the first three hundred pages, and to Dr. Bucknill for the remainder of the work, they hold themselves jointly responsible for the whole, though we can hardly think that the views of the one are the exact counterpart of those of the other. And we would here remark, that the work being a joint work, has led perhaps to some prolixity and a less perfect unity than if it were the offspring of one mind.

The chapter, the fifth, on the Definition of Insanity and on Classification, we are inclined to think, might, with some of the preceding, be omitted with advantage, or much shortened, all that is stated in it tending to show the vast discrepancy that exists amongst authors on the subject, and of what comparatively little use either a definition is or a classification; indeed, respecting the first, a caution is given. They say:

"It is not in any definition of mental derangement that the student will learn what insanity is, and in a court of law the practitioner ought never to be so unwise as to be tempted to offer one, for, as Burrows says, it is 'an *ignis fatuus*' which eludes and bewilders pursuit."

And, as regards the second, we find the classification which is now offered is materially different from their earlier one, and much simpler. It is as follows, with the preliminary remark that—

"Almost all the forms of insanity are so mixed, and blend so intimately one with the other, that when describing them we shall shackle ourselves as little as possible by arbitrary divisions—nosological limitations, which are better fitted for study than medical practice.

"Idiocy (with imbecility and cretinism).	
Dementia	primary. secondary.
Delusional insanity . .	of a melancholy character. of an exalted character. of a destructive character.
Emotional insanity . . .	of a melancholy character. of an exalted character. of a destructive character.
Mania	acute. chronic."

In the next chapter, "On the Various Forms of Mental Disease," the important portion of the work begins, and we can bestow on it unqualified praise. The space to which we are limited obliges us to pass over with merely partial notice the whole of the interesting details, which are amply given, descriptive of the several forms of the disease, following the order they have proposed in their classification.

Under the head of Idiocy and Imbecility, there are two facts which should ever be kept in mind, and which they have well pointed out: one, that imbeciles and idiots are in some instances dangerous, and can rarely be trusted; the other, that both idiots and imbeciles admit of amelioration by education. A case is mentioned, of "an imbecile who, after killing two of his brother's children, went to the father with an expression of delight, and told him what he had done." The evidence of the beneficial effects of education are most satisfactory. Herr Sægert, of Berlin, an instructor of idiots, is one witness of this. He had under him, it is stated, indubitable cases, in which, as the results of education, there was not only an improvement of the higher faculties, but also, as ascertained by measurement, of cerebral development.

Dr. Maxwell, in charge of the asylum at Earlswood, bears equal testimony to its good effect. He reports that all its inmates have improved more or less, even the most hopeless cases, "under kind treatment, good diet, and attention." His experience is so valuable and encouraging that we think it right to give it more fully and in his own words:

"Many that come in dirty, irritable, &c., not only become cleanly, but get to speak intelligently, to dress themselves properly, and make themselves useful. Other cases will do a great deal in the school; for instance, we have a case which came in spiteful, obstinate, and unable to read or write. Now he reads well, writes well, also writes from dictation, draws very nicely, can sing several songs, plays on the harmonium, and can drill, which has made him walk upright. He has latterly been in the mat-making shop, and can make the best part of a mat. Another boy has *improved* in all the above, and is learning mat-making. He possesses, perhaps, the most intellect of any of the boys; but I cannot say that he will ever be like an ordinary person. The cases most favourable are those between seven and twelve, which are healthy, can speak, and are free from fits and paralysis."

Now, if judicious education can have such an ameliorating influence as this on the minds of imbeciles and idiots, ought we to despair of its effects on mankind generally, or in other words, of the effects of a higher civilization in diminishing insanity, a civilization attainable only by education?

Of dementia some very striking examples are given. The most remarkable of these are cases produced by mental emotion. Here is an instance:

"A young lady gave her father a draught of laudanum in mistake for senna. From the time of his death, a few hours after, 'she was lost to all knowledge or notice of persons and occurrences around. Food she never took, excepting when it was placed upon her tongue. The only sound that escaped from her lips was a faint yes or no. To her the world and all things in it were a blank.' She died within the year."

Another instance is given of a young man of a timid disposition, in whom incurable dementia was occasioned by a fright from the snapping at him of an unloaded pistol. It is well to remember that though this form of mental disease is commonly the sequence of other forms, it is rarely curable: the recoveries recorded, it would appear, have mostly been in cases of an acute kind, and of sudden occurrence.

Delusional insanity is a term, as used by our authors, of a very comprehensive kind, including all false conceptions and impressions. The examples described are numerous and impressive, and every way instructive. The following are their concluding remarks on hallucinations and illusions:

"These observations upon hallucinations and illusions may be concluded by succinctly stating the points of real practical importance in regard to them. Either may exist (the former rarely) in persons of sound mind; but in that case they are discredited, in consequence of the exercise of reason and observation, or if credited, they do not influence the actions. They are sometimes with difficulty distinguished the one from the other, and indeed often merge into or replace each other; but still they ought to be distinguished by the points of difference already laid down. Either may be the cause of violent acts, and terminate in murder or suicide; their discovery in criminal insanity is, therefore, most important. Hallucinations are most frequently met with in monomania and melancholia, but are not uncommon in mania. We have several times observed them present with great vividness in incipient senile dementia, and they may be present in the latter stages of dementia, and in imbecility, without our being able to discover their existence. In children we often see remarkable examples of hallucinations, especially, so far as our experience goes, of sight. Illusions are not so common in monomania and melancholia as are hallucinations, being more frequent in mania. The senses of sight and hearing are more liable to hallucinations than those of touch, taste, and smell" [and we may remark are far less the subjects of dreams]. "Pathological anatomy has not hitherto thrown any certain light on the particular lesion associated with hallucinations. As regards their importance and prognosis, M. Brierre truly observes that monomania, when of a lively character (amenomania) and of recent occurrence, is only influenced by them; it is otherwise with melancholia, which frequently receives from them the most unfortunate impulses; they may be dangerous in mania also, from the acts they incite; their presence was observed by Esquirol to be little favourable to the cure of insanity. This observation, however, must not be taken too absolutely." (p. 155.)

Of the cases given we shall quote one as an example. A gentleman labouring under the belief that his legs were of glass, was very angry with his servant-maid for carelessly throwing down logs of wood to mend the fire before which he was sitting; she, tired of his folly, gave his legs a smart blow with one of them; provoked by the pain inflicted, he instantly "rose from his seat (from which he had never walked, for fear of breaking his legs) in a violent hurry to revenge the insult. Soon after, when his anger was abated, he was happy to find that his legs were able to support him; and his mind was freed from this absurd imagination." Another example we shall offer, which came under our own notice. A young Greek, when labouring under an attack of fever, thought himself dead. During a consultation on his case, which, according to usage in the Ionian Islands, was held in the most formal manner in the presence of the assembled family, he spoke of the absurdity of the proceedings, of the folly of feeling the pulse of a dead man, and of discussing learnedly his symptoms. He recovered, and was jocosely called afterwards "Il morto."

The examples given in the section on Melancholia, in its several varieties of nostalgia, hypochondriasis, hysteria, are hardly less remarkable than the preceding. We select the following one from the many.

"A gentleman imagined (although he had led a regular, religious life) that he had entirely neglected his duties, and that therefore he would be burnt alive as a sacrifice to an offended Deity. Three months after he was placed in confinement: he became maniacal, vociferating and swearing constantly; these symptoms passed away, and the former state of despondency returned, accompanied by a suicidal tendency. During four months, these opposite conditions frequently alternated—a phase of insanity popularly known as 'high and low.' At last, while in his low mood, he suddenly became cheerful, and spoke rationally. Undisturbed sleep, to which for a long time he had been a stranger, followed, and he recovered perfectly."

The cases of amenomania, partial exaltation, the opposite of melancholia, are comparatively rare. The following, described in the fifth section, is a good instance of it:

"G. H——, a maiden lady, aged seventy, her face bearing the furrows of many years, walks erect notwithstanding, and is dressed in the most antiquated foppery. As she walks along the streets, the children flock around her, bowing and curtseying, and soliciting sweetmeats and halfpence, with which she is always provided, and which she gives with an air of gracious condescension quite characteristic of her class. This does not arise from benevolence, but is the fruit of vanity in another form—the attempt to obtain homage from the only class willing to give it. Her eccentricities do not interfere with the peace and good order of society, and she converses with entire rationality. She has one brother equally eccentric, and another is confined in an asylum for the insane."

Of emotional insanity, characterized chiefly by impairment, loss, or defect of the moral sense, some very illustrative cases are given in the sixth section. The following is one of them in a patient in the Richmond Lunatic Asylum, Dublin, who exhibited a total want of moral feeling and principle, yet possessed considerable intelligence, ingenuity, and plausibility:

"He never," says Dr. Crawford, "had been different from what he now is; he has never evinced the slightest mental incoherence on any one point, nor any kind of hallucination. He appears, however, so totally callous with regard to every moral principle and feeling, so thoroughly unconscious of ever having done anything wrong, so completely destitute of all sense of shame or remorse when reproved for his vices or crimes, and has proved himself so utterly incorrigible throughout life, that it is almost certain that any jury before whom he might be brought would satisfy their doubts by returning him insane."

Dr. Tuke remarks on this important variety of mental malady:

"We are prevented publishing the details of many cases of this description falling under our own observation. We may mention, however, the case of a patient admitted into an asylum at the age of seventeen, labouring under moral insanity and epilepsy. He possessed remarkable intellectual vigour, united with an exceedingly obtuse perception of moral responsibility. His father stated that he had been the same 'from the cradle.' At nine years of age he endangered the life of a little boy, his playfellow; subsequently at school he was characterized by similar mental qualities, learning more quickly than other boys, yet committing many acts of violence. He was consequently obliged to leave several schools."

He died, it is stated in a note, "of disease of the spinal cord, which caused paralysis and disease of the pelvic viscera," showing, it is remarked, "the close relationship subsisting between disorders of the moral faculties and of the nervous system." Cases of this kind, they observe, have more than any other puzzled the psychologist, perplexed the advocate, and disconcerted the divine.

In the seventh section, on homicidal mania, 60 cases are given illustrative of this form of monomania, of which the following is an analysis:—35 were without marked disorder of intellect; of these, 31 were without premeditation or design; in the other 4 there were both. Fifteen showed more or less disorder of intellect. In 5 of these there was a deficiency of intellect; in 10, delusions.

Other cases are given in detail. We quote one as an example:

"A convalescent patient once induced his attendant to lend him a razor for the purpose of shaving. He sat down before a glass, and having shaved one side of his face, called him, the attendant, to see with what dexterity he had performed that part of the operation. The attendant came forward; but the patient started up, and nearly severed the man's head from the body."

Of suicidal mania, the subject of the next section, the varieties are as many as of the preceding. It is very noteworthy that children are occasionally liable to this monomania. In France, it is stated that from 1835 to 1844, on an average, every year 19 committed suicide under sixteen years of age. Of 26 cases recorded, 1 child was five years old, 2 were nine, the rest were under fourteen. A remarkable instance of an adult is related, in which the disease was attended not with a depressed but rather with an elevated and joyous state of mind.

"The patient, who was opulent, stated that he was perfectly happy, and free from every cause of suffering, with the exception of one circumstance which tormented him. This was the desire, thought, or violent temptation, to cut his throat whenever he shaved himself. He felt as if he should derive from the act '*an indescribable pleasure.*' He was often obliged to throw away the razor."

It is an important fact, that murder and suicide have a close relationship. "It has been calculated," the author states, "that in sixty per cent. of cases of murder, suicide is attempted." In Ireland, we may remark, according to the last census but one, the number of suicides during the decennial period exceeded the number of murders.

Many remarkable instances are given in the ninth section of that peculiar mental derangement designated kleptomania. Particulars are related of a family in which this monomania of an irresistible propensity to steal—for such it seemed to be—was hereditary in three successive generations. In the great majority of the cases mentioned by the author, the morbid propensity was not associated with any marked disorder of intellect.

The tenth, eleventh, twelfth, and fourteenth sections we must pass over, little more than naming the subjects of them. These are, erotomania, including nymphomania and satyriasis; pyromania, dipsomania, and puerperal insanity. The first three, like the preceding monomanias, are characterized by irresistibility. The last-mentioned, puerperal insanity, has, it is generally admitted, no very special constant symptoms by which it can be distinguished in a satisfactory manner from mania. Instructive cases are afforded, accompanied by judicious comments on all these forms of mental derangement.

General exaltation, or mania, constitutes the thirteenth section. In the way of arrangement, it seems to us that this, the most common mental disease, the best marked and recognisable, constituting about at least fifty per cent. of the whole, might have been advantageously placed before the more or less isolated morbid propensities, the monomanias, and made an introduction to them. The author's remarks on the disease, as to its various phenomena, the difficulty of defining it, the various opinions of authors respecting it, &c., if given in the earlier portion of the work, might, we cannot but think, have been a help to what follows, and might have saved some repetition. His account of mania in its acute and chronic stage occupies seventeen pages. It is comprehensive, and well adapted to impress on the mind of the alienist student the difficulties he has to contend with, and the caution and judgment that should be exercised in the inquiry. It should always be remembered that every case has its peculiarities; that, however raging the mania may be, the pulse and general health are not necessarily disturbed—that is, are not invariably disturbed—nor the intellectual faculties deranged, though commonly they are more or less so.

The statistics of insanity, including in three separate sections the causes of the disease, the proportions of recoveries and relapses, and the proportional mortality, are treated of in the eighth chapter.

A good deal that has been advanced on the causes of insanity has been almost unavoidably conjectural at best, being founded in so many instances on a too limited induction. Of the predisposing causes, a certain number of the assigned ones must on this account be viewed as questionable—such as sex, consanguinity, occupation. Of those better determined, the following may be named: heritage, old age, celibacy. Of the exciting causes, there seems to be a doubt whether the moral or the physical preponderate. The author ranges them in the following order, beginning with the physical: intemperance, epilepsy, affections of the head and spine, vice, and immorality. Next, the moral, likewise in the order of their inferred force: domestic troubles and grief, religious anxiety, disappointed affections, fear and fright, intense study, political and other excitement (joy, &c.), wounded feelings. Without some predisposition it seems questionable, we may remark, whether any of these assigned causes would be operative, inasmuch as so large a proportion of our fellow-men are exposed to the same incidents and retain their sanity unimpaired. This portion of the work might, we think, with propriety have been enlarged. The etiology of insanity, viewed in relation to prevention, is of the first importance, and parts of it admit of physiological illustration. From this point of view we had expected to have found some details respecting the comparative influence of the several kinds of intoxicating drinks amongst different peoples, but we have been disappointed. How very desirable it is that their effects should be clearly expounded;—how much greater is the danger from the abuse of ardent spirits than from that of wine and malt liquors.

The proportion of recoveries of those afflicted with insanity, according to the most authentic and reliable statistics of the disease, varies from 25 to 50 per cent.; in England, as last reported by the Lunacy Commissioners, the proportion was 35·5 per cent. It is well-established that the probability of recovery diminishes with the length of duration of the disease, and especially before the disease has been encountered by treatment.

That the average duration of life is shortened by insanity appears to be proved in the most convincing manner. A table is appended, distinctly demonstrating the fact.

Dr. Bucknill's moiety of the work—its second part—comprises the diagnosis of insanity, its pathology and treatment. It has received so much attention in our notice of the first edition, that we may be well excused for not dwelling on it at any length at present.

The chapter on Diagnosis, with which it commences, and which extends over eighty pages, is ably written, with perhaps too much of metaphor in the descriptions, and some redundancy and repetition—the latter attributable to that want of unity already adverted to. These are minor blemishes. As a whole, it deserves the careful perusal of the student. It cannot fail to bring strongly to his mind the difficulties he will have to encounter, should he choose this branch of his profession. But if he possess the qualities fitting him for entering on the study of medicine as a science—if medicine yet deserves that name—such as ordinary ability, good sense, a love of truth, and a love of labour, he need not despair, even remembering the high requirements laid down by the author as essential to the alienist expert—such as skill in physiognomy, a fine tact, profound metaphysical knowledge—that is, so far as the laws of mind are concerned. We need hardly remark that all kinds of knowledge cannot fail to be useful to such a student, but not more so than to other medical students. As to metaphysics, it may be a question whether an addiction to their study is not likely to be rather injurious than beneficial, and certainly the former, if it should lead, as it is often asserted to do, to over-refinement, to casuistry, and endless distinctions. The greater the amount of knowledge possessed, the more judgment clearly is needed to make a good use of it. How often is the evidence of the expert unsatisfactory in a court of justice, and in consequence, how little is the confidence it usually inspires! Almost every part of this chapter might be quoted to show the difficulties which lie in the way of diagnosis, and the judgment needed to distinguish the varieties of mental disease. The following quotation we offer as an example, and also as an example of our author's style:

"*Chronic mania* has to be diagnosed from malingering, from eccentricity, or from sanity. There are no non-mental diseases for which it can be mistaken. The symptoms of chronic mania present themselves in such infinite variety, that it is extremely difficult to round them within the compass of a description which will be sufficiently brief for the purpose of comparison. Chronic mania, in the sense in which we use it, is distinct from incomplete mania, is in most instances the result and the remainder of the more acute form. It represents the rudderless and shattered state of the vessel after the tornado of raving madness has swept by. The wreck is left in every variety of condition; sometimes sail enough remains to keep her head to the wind; sometimes she lies upon the waters a log, in the helpless state of consecutive dementia. In chronic mania of this kind, there is always more or less of dementia, and the loss of mental power is to a great extent diagnostic between chronic mania, which is consecutive upon acute mania, cerebral inflammation, or typhus, and chronic mania which has resulted from moral shocks or from physical causes less injurious than the above to the organic integrity of the brain. Chronic mania which has not passed through the acute stage, frequently presents a remarkable vigour of the intellectual functions, in so far as they are not affected by delusion. Patients with this form of disease not only retain the perceptive faculties in all their activity, but the memory also is found to be tenacious; and even the judgment, on matters unconnected with the delusive opinions and perverted notions peculiar to the case, may be found to be sufficiently trustworthy. It will be asked how such a case is to be distinguished from one of monomania? and, in truth, the distinction between the two is not very observable in practice, though, if we accept the current descriptions of monomania, the distinction would be easy. It is unnecessary to embarrass our present subject by discussing the existence of book-drawn monomania; for the present purpose it is sufficient to observe that primary chronic mania runs insensibly into the nearest approach to monomania with which we are acquainted. In some cases the delusions are numerous; in others they are few; in others there is but one—one, however, indissolubly connected with other grave lesions of the mental powers." (p. 337.)

Our remarks on the next chapter—the Pathology of Insanity—must be equally brief. Of it, as we said of the preceding, it deserves a careful perusal, and like it, its tendency is, we think, rather to convince the student of the infinite difficulty of the inquiry, than to inspire him with hope of attaining any useful knowledge of the organic lesions productive of insanity. In one place the author says: "The condition of the cerebral substance is the prime question in the pathology of mental disease." And in the next paragraph he adds: "To the pathologist the substance of the brain is as yet practically structureless." Further on, when treating on the pathology of general paralysis, he holds that the only proposition justifiable at present is, "That the disease depends upon some modification of the brain, whose nature we have not yet learned to appreciate." The following is the concluding passage of the chapter. It is given after details spread over 115 pages, and contains the results of his inquiries:

"Such are the gross changes which have presented themselves to us, and when the microscopist and the chemist have succeeded in demonstrating the fundamental changes of the structure which undoubtedly exists in the nervous system in such diseases as tetanus and hydrophobia, we may expect their invaluable aid in the elucidation of the true nature of the pathology of the interesting disease under our present notice." (p. 497.)

Of the gross changes adverted to, the description given by Dr. Bucknill is most ample. What is chiefly remarkable is their want of accordance; and what is least satisfactory is, that the same lesions are met with in very many instances in which during life there had been no indications of insanity. He is of opinion that atrophy of the brain is one of the most common accompaniments of the disease, often beginning with hypertrophy; the former associated with chronic, the latter with acute mental malady. But how often have we atrophy of the organ, as in pulmonary consumption and in advanced old age, without derangement of intellect! And does not the like remark apply to the great majority, if not to all lesions—"the gross changes" which have been noticed in necroscopical research. No organ, as regards this research, offers so many difficulties as the brain. Many are hardly sufficiently familiar with its appearances in its perfectly healthy state, from want of opportunities of examining it in that condition. A variety of casual circumstances after death may affect its aspect and its physical qualities, the temperature of the atmosphere, the interval between the death and the autopsy, the position of the corpse, abdominal distension or relaxation, &c. Then there is the difficulty in determining—nay, the impossibility as regards the lesions found in any particular case—whether they were concomitants of the fatal attack, or its independent precursors. Here is an instance in point. Dr. Bucknill, when adverting to the viscera which have been found to exhibit peculiarities in the insane, remarks:

"There is nothing remarkable among the insane in the pathology of the small intestines; but the large gut suffers in chronic insanity frequent and extraordinary displacements, which we are quite at a loss to explain. The most common of these displacements is that of the traverse-arch of the colon to the lower part of the abdomen, from whence again it ascends to take its proper position in the descending position."

Now, what proof is there that these so-called displacements were not congenital, and nowise owing to mental disease? It may, indeed, be a question whether they might not have conduced to the disease; and *that* even we think very doubtful, considering that such an abnormal position of this intestine is far from an unfrequent occurrence in those who have died of other diseases without a suspicion of mental derangement. In a recent work by Dr. John Davy, "on some of the more important diseases of the army,"* as many as thirty-seven examples are given of the kind, of which three only were in subjects who had been insane.

Unsatisfactory as is this chapter in its details, we must repeat that it well deserves, and will repay, a careful perusal. Its amplitude is its recommendation, and not the least valuable portion of it is that which contains the results of Dr. Bucknill's own researches. Although the subject now is obscure, and full of difficulties, yet when we reflect on what has been accomplished in physiology and pathology in recent times, we ought not to despair of success in this special inquiry. The first step to such a success, it may be presumed, will be a more accurate knowledge than we at present possess of the physiology of the brain. Until its functions in connexion with its organization are well ascertained, it can hardly be expected that their derangements can be accounted for. A quotation which the author gives from the great work of M. Esquirol, "Des Maladies Mentales," expresses a feeling probably common on the subject. It is very much ours; and we venture to entertain the hope which he indulges in:

"Thirty years ago (says this veteran) I would willingly have written upon the pathological cause of madness. At the present day, I would not attempt a labour so difficult, so much of incertitude and contradiction is there in the results of the necroscopy of the insane up to this time. But I may add, that modern researches permit us to hope for ideas more positive, more clear, and more satisfactory."

Those inquirers who are most likely to accomplish this desideratum are not, we think, the alienist physician, whose attention is chiefly confined to the insane, but those of wider research, who devote themselves to the pathology of disease generally in union with physiology.

* See our number for April last.

The concluding chapter on the treatment of insanity is ably written. We need hardly remark that, so long as our knowledge of the pathology of mental diseases is imperfect, so long must the treatment of them be tentative and uncertain. The author's remarks on the subject are all of a very judicious kind, and the practice he recommends and illustrates by cases, in a valuable appendix, as rational as that imperfect knowledge just adverted to seems to warrant.

Review III.

Reports of the United States Sanitary Commission.—New York.

In this, our third article on these important and interesting reports, we propose, according to promise, to notice those portions of them relating to the medical statistics of the great American civil war—still, it is to be lamented, raging—and review briefly the more remarkable of the several treatises which have been published by the Commission as hand-books for the use of the surgeons of the Federal armies.

The document from which we derive most information on the first-mentioned subject is a "Report on the Mortality and Sickness of the United States Volunteer Forces" (1861–1862), by Mr. E. B. Elliott, a professional actuary. The data from which his report is drawn up are stated to have been, for the mortality, the official monthly returns made to the office of the adjutant-general; but as many regiments neglected this duty, we cannot have perfect confidence in the accuracy of the estimated losses. Indeed, Mr. Elliott points out that they must have been underrated; and he makes in his calculations certain allowances on this supposition. As he appears intent on accuracy, neither to exaggerate nor extenuate, we will take for granted that the figures which he arrives at are at least approximately correct for the period to which they extend: it is the first year and half of the contest.

Our readers will probably be surprised to learn that his estimate of deaths, made with all due allowance for defectiveness of data, does not exceed 53·2 per 1000 numerical strength of the forces actually engaged in the field,* of which about 44·6 were from disease and accidents, and 8·6 from wounds received in action; and that the rate amongst the commissioned officers was less, 33·2 per 1000; that amongst the enlisted men 54.

Mr. Elliott first draws a comparison between these losses and those sustained by the United States regular army during a period of eighteen years; from which it appears that in the year of lowest mortality, 1845, the death-rate per 1000 was 9; of the highest mortality (1849, a cholera year), 80; the highest of other years (1841), 40; the average for the entire period (1840-46 and 1849-59), 26.

* According to another estimate, that made by Mr. Benjamin, of the Confederate Government, in an intercepted despatch to Mr. Mason, the losses of the Federal armies during one year, reckoning up to September, 1862, amounted to 349,500 men!

Secondly, of the British army at different periods, beginning with the war of 1803-12, and ending with the entire Crimean campaign from September, 1854, to June, 1856, inclusive, two years and a quarter. We shall give these two rates, passing over the intermediate. During the first period the deaths from wounds were 9·0 per 1000, from disease 71; during the last, the deaths from wounds were 30 per 1000, from disease 202.

Of the British army in time of peace, for various periods the death-rate at home was 16 per 1000, abroad 36.

Of civilians, at military ages, the English death-rate (1841) was 9 per 1000; the American (Massachusetts, 1855) 10.

These various rates are very instructive every way considered, and especially in relation to the conditions, the causes in operation. During the ordinary state of society when undisturbed by war, the mortality-rate is a constantly varying one, within narrow limits, owing to influences partly meteorological, partly social, which there is commonly difficulty in appreciating. But it is not so during war; then the causes are generally conspicuous, and their effects, duly weighed, can be calculated with a more than ordinary prophetic accuracy.

When we reflect on the conditions under which the volunteer forces were serving during the early period of the war under review, our surprise should cease that their losses were comparatively so low. The first circumstance in their favour was, that a large portion of the whole was serving at home, in a climate to which they were accustomed, it being a well-known fact that troops almost invariably experience an increase of sickness and of mortality when engaged on foreign service. Other, and many other, circumstances co-operated in their favour, the chief of which were that they appear to have been well-clad, amply-provisioned, and commonly well-hutted and sheltered. Of the minor circumstances of a favouring kind, not the least were the facility of desertion,* and the facility of invaliding; and we may add, we think, the humanity, the absence, for most part at least, of vindictive ferocity, with which the hostilities were conducted. Let us keep in mind, to the credit of the American people, both Federal and Confederate, that, as in our Great Rebellion, there were no ruthless massacres perpetrated, no towns taken by storm and their inhabitants delivered over to the cruel greed and lust of an infuriated soldiery; and as yet no tocsin sounded in the Southern States, with we fear one exception, inciting the slaves to rise and murder their masters and masters' families.

The tables which Mr. Elliott has appended bear on some of the conditions referred to, and are very deserving of being consulted. We shall avail ourselves of them in a limited way, stating briefly some of the principal results arrived at.

1. As to seasons: it would appear that the greatest mortality was during the winter months, December, January, February, 1861-2—

* In April, 1863, it is stated that the number of deserters from the Federal army was 150,000.

viz., 70 per 1000 of mean strength, of which 11·9 were from wounds received in action, 58·4 from disease and accident; the least in summer—viz., 21, of which 8·4 were from wounds, 12·8 from disease and accident; and the next least in the autumnal months of September, October, and November—viz., 36, of which 4·4 were from wounds, 31·6 from disease and accident. These are results well accounted for, keeping in mind that deaths from wounds are a tolerable index of the degree of activity of the engaged forces, and as well as of the fatigues and privations which the troops had to endure, as of the actions fought; this being a rule in war, that all abnormal exertion and privation must be attended with increase of sickness and mortality.

2. The second table relates to the mortality by States, the States in which the forces were raised. Four groups are given: 1st, New England States, with a mortality of 36·1; 2ndly, Middle Group, with one of 30·1; 3rdly, New England and Middle Group combined, 31·6; 4thly, Western Group, with a mortality of 95. In the two first, the deaths from wounds did not exceed 3·2; in the Western Group it amounted to 18·2. Thus confirmatory of the foregoing remark of the deaths in battle being a tolerable criterion of the intensity of the war; and we know that during the period to which these returns refer, the West was the scene where the struggle was most severely contested.

3. The next table shows the comparative sickness of the commissioned officers, and of the enlisted men during the period—that of the former, 68·8 per 1000; of the latter, 105·9, or an approach to double. This also is in accordance with campaigning experience, the officers, almost invariably as regards the exciting causes of disease, being less exposed than their humble comrades, inasmuch as commonly they are better fed, better clothed and lodged, are exempt in great measure from exhausting fatigue, many of them mounted, and those not, having no heavy pack to carry, and, which is not least important, having a larger proportion of unbroken and refreshing sleep.

4. The last table but one given, and the last we shall notice in part, is that comprising the experience of the British army during the Crimean campaign—a table this, in the momentous results which it discloses, deserving of a place in every regimental orderly-room, and in the quarters of every general officer in command. In it are contrasted in a very striking manner the different periods of the war; thus, in the instance of the two winters, the results were the following: in that of 1854–5, seven months, from September to March (both inclusive), the annual death-rate per 1000 was from wounds 43·0, from disease 622·0; whilst in that of 1855–6, seven months, from September to March, from wounds the death-rate was 14·5, from disease only 35·0.

Taking particular months of the two periods, the contrast is still more wide and remarkable. Thus in January, 1855, the deaths from wounds were 31·0 per 1000, from disease the enormous amount of 1143·0; whilst in January, 1856, the deaths from wounds were 0·5, from disease no more than 24·5. Again, contrasting the mortality (the rate per annum per 1000), in February of the two years, in 1855, the deaths from wounds were 16, from disease 963; whilst in 1856 the results were, from wounds 0·0, from disease 12.

These marked differences need little explanation. It must be fresh in the minds of our readers, that during the time of the vast mortality, actually exceeding, if continued, per annum, the total force as 1143 to 1000, that the conditions of our troops were of the worst kind; they might well be called deplorable—hard work in the trenches, famine in camp, total destitution of every comfort in the way of shelter and clothing, of light and fire, and almost a total want of good nutritive food; whilst, on the contrary, during the period of least mortality and greatest healthiness, there was a luxurious abundance of all the things before wanting, with a cessation of overwork and harassment following the taking of Sebastopol.

It is to be hoped that the historian of this memorable war, who has given such proof of unrelenting vigour in some of his biographical sketches, will not shrink from faithfully portraying the horrors of sickness and the terrible fatality occasioned by ignorance and negligence; or, in other words, by defective administrative power—ignorance in not knowing the requirements of an army in the field during the winter season in the Crimea—negligence, or want of energy (for want of energy in war deserves no better name), in procuring supplies essential to the well-being of an army so situated.

In another Report a good deal of information is given respecting the prevailing diseases and their causes, and the proportional sickness of the several armies, but it comprises only about six months of the war—viz., from June to December, 1861, and is designated "advanced incipient copy." From this document it appears that the diseases of the volunteers have been as many and varied as those of our own and of other European armies when in the field, and the most prevalent ones much the same, a large proportion of them being the ordinary camp-diseases, such as fevers, dysentery, and diarrhœa.

The proportion of sick has varied from 42, the lowest number per 1000 strength, to 192, the larger number in the army of the West, which, as already observed, was most actively employed, and on most hard and harassing duty.

The following is the percentage of diseases during the period, compared with that of the British army in the Crimea from April, 1854, to June, 1856.

	Army of the Potomac.	Army of the West.	Army of the Crimea.
Zymotic	61·1	76·4	69·8
Constitutional	1·2	6	5
Local	30·7	17·3	15·6
Developmental	3·4	3·5	·1
Violence	3·6	2·2	14·0
All cases	100·0	100·0	100·0

Of 27,000 cases returned as treated, a large proportion appear under the class Zymotic and order miasmatic; these terms indeed, as used in the

Report, are very comprehensive, the latter including all kinds of fevers, cholera, both common and Asiatic, dysentery and diarrhœa, and rheumatism and anthrax, altogether amounting to 17,886.

Of the Constitutional class the proportion is small, though this includes lumbago and phthisis, the total being only 270.

The class of Local diseases is by far the most numerous, including in its different orders as many as eighty distinct maladies, of which those of the digestive organs, 2993, constitute a large proportion, and next those of the nervous system, 1393, and of the respiratory organs, 1093; the whole as many as 6822.

Under the class of Developmental, two diseases only are returned, atrophy and debility, amounting together to 747 cases.

Of the last class, that of Violence, such as wounds, &c., the total number, it is remarkable, does not exceed 819, no great battle having been fought during the period comprised.* Included in this number, it is noteworthy there is only one case of punishment and execution.

We must pass over much that is interesting in this Report on the various circumstances commented on, affecting the health of troops in the field, such as water, rations, sites of camps, drainage, ventilation, tents, flooring, &c. On most of these the remarks are of a general kind. On a few of them precise information is given, thereby enhancing their value. Examples of the latter are the kind of tent in use and the tent-flooring in their influences. Of the two kinds of tents most in use, the wedge and the Sibley—the former lodging six men, the latter from twelve to sixteen, and better adapted for ventilation—typhus prevailed most in the first, in the ratio of 29·5 to 23 in the second. The medical statistics of the flooring of the tents, which are given in figures and in detail, are specially worthy of record. The following is a summary of the results:

"A limited examination of the diseases of the army indicates that the largest proportion of those of a typhoïd type occur with regiments sleeping in India-rubber blankets, the least with those on straw or boughs; the largest proportion of catarrhal with regiments on wooden floors, the least with those on the ground; the largest of rheumatism with those on wood, the smallest with those on boughs; the largest of malarial with those on the ground, the least with those on straw or boughs."

The Commissioners recommend, and we think with good reason, fir or cedar "spray," whenever attainable in sufficient quantity, as the best bed for soldiers in camp; and as justly are opposed to the use of board-floors, these concealing dirt and rubbish, and yet not preventing noxious exhalations. Many a barrack in our colonies, we are confident, has owed much of its unhealthiness to a planked floor, so laid as not to permit of ventilation beneath, and yet to serve as a sink for filth and a generator of malaria after the manner of the bilges of a ship.

We have marked many passages for comment or extract as bearing

* In the battle fought before Fredericksburgh in Dec. 1862—one of the severest during the war—the total loss of the Federal army was returned as 14,379—viz., 1190 killed, 9986 wounded, and 3203 missing.

on the etiology of disease; but restricted as we are for space, we must confine our choice to one or two. The following is noteworthy as showing the results of a regiment encamped on a good site, and of another adjoining on a bad site:

"Two regiments," reports the President of the Commission, "separated by a quarter of a mile only, contained in one camp not a dozen sick men; in the other, 250 more or less ill with dysenteric diarrhœa; and all because one was on a plain with decent well-water at hand, the other in a wood, with a wretched puddle of black ditch-water as the only resource for drinking and cooking."

Hardly less noteworthy in its effects on health is a bad article of food, such as are specially mentioned, pies—the "villanous pies," as they are designated—which were supplied by the sutlers. The following extracts from the report of a surgeon of a volunteer regiment, addressed to the Commission, vividly, though in a somewhat odd manner, denounces the evil, ending with a suggestion for its correction hardly practicable even under despotic rule:

"In our regiment we have the best sutlers on the Potomac; *nevertheless, they prove in actual practice an unmitigated curse.* Some of the men throw their rations away, and literally live on sutler's *trash.* Others will eat a full ration, and then go straight to the sutlers' and eat three or four villanous pies. Many of these have been fried in condemned lard a week before the soldier eats them; the result is, *camp-diarrhœa, dysentery,* and all their concomitant evils."

He adds:

"Sutlers are a twofold evil. By them the soldier is tempted to spend his earnings, which should be saved for a purpose, and is made sick in the same transaction. My observation and experience in camp prove clearly that to keep a soldier *healthy* you must confine him to plain and regular rations."

He concludes:

"If Congress would pass a *law,* the tendency of which would be to *compel* the soldiers to live on Government rations only, it would prove a blessing of infinite value to the service."

Judging from the later reports of the Commission which are before us, a great improvement has taken place in the volunteer Federal army as to health and efficiency, and not in a small degree owing to the exertions of the Commission; yet we cannot but smile at some of the eulogies indulged in as too sanguine and too highly coloured, such as the following: "That the *morale* of the army of the Potomac after the battle of Fredericksburg was equal to any in the world." The same writer, a sanitary inspector, concludes his report relating to this battle in the same eulogistic strain. We quote the passage as indicative at least of Northern faith and sentiment:

"The recent terrible conflict has left their courage unshaken, and their confidence in their leader is firm. Cheerfully performing the duties of the soldier, even new regiments are becoming perfect in discipline. Instead therefore of demoralization, they possess all the moral qualifications necessary to insure speedy and complete victory. Having confidence in the sacredness of our cause, and faith in the justice of God, our army cannot fail."

This was written on the 29th December, 1862. We shall make no comment on it, but we may repeat the saying of a great general, that Providence is on the side of the strongest battalions, with a remark we venture to make of our own, that man is presumptuous in dogmatizing on so mysterious a matter as the justice of God in human affairs, and especially in war.

Respecting the tracts which have been issued for the use of the medical officers of the armies of volunteers, our remarks must be briefer than we could wish. Of the strictly professional ones there are now fourteen before us, besides which there are five others. These latter are,—1st. 'Rules for Preserving the Health of the Soldier,' in 12mo, a fourth edition. 2nd. 'On Camp Fires and Camp Cooking; or, Culinary Hints for the Soldier; including Receipts for making Bread in the portable Field-oven furnished by the Subsistence Department.' 3rd. 'Advice as to Camping;' a reprint this from the 'British Government Sanitary Commission.' 4th. 'Directions to Army Surgeons on the Field of Battle;' a reprint of Mr. Guthrie's, who by a strange mistake is designated "Surgeon-General to the British Forces during the Crimean War." 5th. 'On Military Hygiène and Therapeutics;' a third edition, containing excellent instructions—these directed to the preservation of the health and efficiency of the armies, and in more detail and a wider scope than the rules addressed to the soldier, with the addition of suggestions relative to hospital administration in the field, and the care of the wounded after an action.

These subsidiary tracts are all well adapted for the purposes intended, and especially the first two. The rules for preserving health are designed for the use of the men as well as of the officers—every man to have a copy. The example is worthy of being followed in our army: such brief instructions might be of essential service in the way of teaching the soldier to take care of his health by making him acquainted with some of the most important requisites. In this little tract, as well as in the 'Culinary Hints,' bread and soup are laid down to be "the great items of a soldier's diet in every situation." In our army there is now some danger of forgetting this, in the recent outcry for variety. The cooking hints are admirably adapted for American use. With some modifications, a like tract would deserve a place in the kitchen or cooking-place of every British regiment. Beans form an important part of the soup of the volunteers, and also desiccated vegetables, both to be recommended—the one for high nourishing power, the other for wholesomeness. Vegetable additions, including onions and peas, judiciously used, might secure variety of flavour, as well as variety of substance.

The purely professional tracts are all ably written, and are very creditable to their authors and to the state of medical science amongst our Transatlantic brethren. Some of them, indeed, are deserving of the highest praise, and not only for the subject-matter, but also for their manner; such, for instance, is the veteran Dr. Valentine Mott's essay on 'Pain and Anæsthetics,' in which, in the brief space of fifteen pages, he eloquently and philosophically discusses the important subject, resting his argument on a few propositions,—1st. That "to *prevent pain is humane.*" 2nd. That "pain is useless to the pained." 3rd. That "pain is positively injurious to the pained." 4th. That "anæsthetics, when properly used, are perfectly safe." His reasoning is very logical, and his illustrative facts happily chosen. We could wish that this little treatise were in the hands of every military and naval surgeon; for it not only affords proof of the propriety and safety of using anæsthetics, but also lays down simple rules, following which, even the risk, as far as possible, may be guarded against. Chloroform he prefers to ether, as acting more rapidly. Opium he continues to hold in high estimation, attributing much of its virtues to its relieving pain and thus preventing the exhaustion of nerve-force. To the freer use of opium in operations by American surgeons he attributes a success, which, he thinks, exceeds the European average. Had such a pamphlet as this been issued previous to the Crimean campaign by the army medical authorities, and had the heroical anæsthetics, chloroform and opium, been supplied in due quantities, how much severe suffering might have been prevented, to say nothing of the saving of life, and how much reproof and sarcasm, justly bestowed, have been escaped! The departmental order given before the battle of the Alma, not to use anæsthetics in operations on the wounded—happily not obeyed—was a sad example of physiological ignorance directing surgical practice.

Another tract of a kindred kind—that on the use of quinine as a prophylactic—is but little inferior in value to the preceding, showing as it does, by very satisfactory evidence, that this alkaloïd is as safe as it is efficacious in preventing, when daily used, attacks of malarious fevers, whether intermittent, remittent, or continued, and however long used, without abatement of its energy or injury to health. Some of the instances given of its efficacy in regions notoriously malarial are remarkable, and are a valuable addition to those which have been more than once quoted in this Review, when noticing the Medical Reports of our Navy compiled by Dr. Bryson.

The purely professional tracts are partly medical, partly surgical. The diseases treated of are the more important camp-diseases, such as fevers, dysentery, scurvy, pneumonia. The surgical essays relate to the "Excision of joints for traumatic cause," and "The treatment of fractures in military surgery." Besides which, there is a 'Report of a Committee of the associated Medical Members of the Sanitary Commission on the subject of Venereal Diseases, with special Reference to Practice in the Army and Navy;' and another 'On the Value of Vaccination in Armies.' We have read them all, and we commend them all without detracting from the praise we have already given. Condensed as they all are, they display more than common literary research, and an acquaintance with the latest and best works on the different subjects on which they treat. That on vaccination is particularly deserving of perusal. The value of vaccination is shown by very ample statistics, in a manner, we think, to convince the most sceptical, if any such there be; and also of the propriety of a second vaccination, as a further security from small-pox, when small-pox is epidemic. The best modes of preserving vaccine lymph are described; and that of Dr. Husband, by hermetically sealing it in *straight* tubes, by which its efficacy can be preserved for several years, is most insisted on. In addition, a new preservative method is noticed—the invention of Dr. Collins, of Boston, a very simple one—viz., including the ordinary scab, reduced to powder in glycerine; glycerine, it would appear from the experiments reported, having the property of dissolving the lymph, and of keeping it without its losing its efficacy for at least many days, and, as he supposes, were air excluded, in sealed tubes for many weeks.

The assurance that the volunteer armies of the Federals are unusually free from the curse of venereal diseases, is a fact creditable to the people. The tract on the subject of these diseases is written with much discrimination, especially as regards the treatment of the several varieties of the malady. The use of mercury is advocated, but only in a mild manner, and by inunction in cases of true venereal chancre.

In the report on scurvy, an interesting review is taken of the disease as to its causes and cure, as well as of the rationale of its symptoms, embodying much experience acquired in the ample field of American practice. It is well inculcated and proved, that the disease is a blood-disease, arising commonly from complicated causes, and that under depressing mental conditions and unsanitary circumstances, it may be produced whether the diet be salt or fresh, whether meat or vegetables, and that the most efficient remedial means—and those alone are sufficient—are the opposite of the exciting causes.

Dysentery, as one of the most common and fatal of camp-diseases, has due attention paid to it, and is very ably treated. The author distinguishes its varieties and complications from the mild to the malignant, with other distinctions, such as the sthenic, bilious, and periodical. The complications he chiefly dwells on are, typhoïd fever, intermittent fever, and scurvy. Hepatic abscess he states to be of rare occurrence in the disease even in the Southern States, confirming our impression that too much importance may have been attached to the conjunction of the two. Dysentery the writer holds to be a blood-disease; and expresses an opinion, which we hesitate to adopt, that it may prove fatal before its localization is fixed, and not only before ulceration has set-in in the large intestines, but even before inflammation. Tracing an analogy between the epidemic form of the disease and typhoïd fever, he says that, as in the latter, when "of a grave type, the general phenomena predominate, and death itself may occur before any ulceration of Peyer's gland takes place; so in dysentery, a fatal issue may be quite independent of intestinal changes, but present, on the other hand, all the evidence of blood-poisoning." Such an inference, so extreme of its kind, seems to us hardly admissible. That it is a blood-disease even may be questioned. Another opinion which he expresses, to which we cannot give an assent, is, that there is not, after the healing of dysenteric ulcers, a restoration of epithelium.

Our observations lead us to believe that there is such a restoration on mucous membranes, and specially of the large intestines, analogous to that witnessed after the healing of cutaneous ulcers. The treatment of the disease in its several forms is very judiciously described. Venesection is very properly prohibited even in the sthenic kind. The remedies chiefly recommended, varying with the varieties, are opium and calomel, ipecacuanha and acetate of lead, with special precautions as to diet and hygiene.

The tract on Pneumonia displays a very minute and scientific knowledge of the disease in its various modifications and complications. As in the former disease so in this, the use of the lancet in treatment is considered dangerous; the sustaining of the strength is most relied upon, the rule being laid down that the disease cannot be cut short, and that death in the fatal cases is owing not so much to apnœa as to asthenia. Very properly, stress is laid on the rapid manner in which effusion or exudation is got rid of by absorption in natural cases. It is interesting and instructive to see how in this disease, and in dysentery and in fevers, there is the same change of practice in America that we witness amongst ourselves; and there, be it remembered, unsupported by the hypothesis that a change has taken place in the constitution of man, and that in consequence diseases in our times have assumed a more asthenic type.

The three tracts on Fevers, two of them, those on Miasmatic Fevers and Yellow Fever by Dr. Metcalfe, that on Continued Fevers by Dr. J. Baxter Upham, sustain equally the high character of the preceding Reports. And the same remark is applicable to the surgical treatises. Did our limits permit, we could wish to be more particular in our notice of them, so as to endeavour to do them ample justice; but we have already exceeded, we fear, our allotted space. We venture to express the hope that these essays will be republished: collected, they would form a volume of moderate size, and we cannot but think it would be in request.

We must not conclude without expressing our admiration of the efforts made by the Sanitary Commission in the performance of its voluntary duties.

Of all the circumstances connected with the gigantic struggle between the North and the South, the existence of this Commission appears to us the one which gives us most confidence that there is a great fund of national worth amongst the Federals, and a sustaining virtue on which, in spite of popular and mob clamour, we may rely for their not aggravating the present evil by rushing into a war with England.

Review IV.

Studies in Physiology and Medicine. By the late Robert James Graves, F.R.S., Professor of the Institutes of Medicine in the School of Physic in Ireland. Edited by William Stokes, Regius Professor of Physic in the University of Dublin.—*London*, 1863. pp. 511.

This is a valuable and very interesting volume. It consists of two parts—a memoir of the late Dr. Graves, and a selection of papers contributed by him to various periodicals, which give the title to the work.

Dr. Graves was a remarkable man; his character as a physician is briefly and forcibly and, we believe, faithfully described in the following passage by Professor Trousseau, to whom the "Memorial" is dedicated.

"Graves is, in my acceptation of the term, a perfect clinical teacher. An attentive observer, a profound philosopher, an ingenious artist, an able therapeutist, he commends to our admiration the art whose domain he enlarges, and the practice which he renders more useful and more fertile."

Fortunate in his career through life, an instance of high merit amply appreciated and rewarded, he has been equally fortunate after his death in having his eulogy and his memorial written, the one by an admiring foreigner, himself a man of eminence in his profession; the other by an intimate and attached friend, in every way competent to do him justice and make us acquainted with the special qualities of the individual, his attainments, his opinions and doings.

Brief as the memoir is, occupying only eighty-three pages, it is full of instruction, and admirably adapted to give a good bias to the student of medicine, and to impart a beneficial influence to the minds of all those who may read it—what is noble and praiseworthy being happily blended with example, or rather, we should say, emanating from the example.

Dr. Graves' 'Clinical Medicine,' which the Parisian professor says "I have read and re-read," "which I have almost by heart," "which I cannot refrain from perusing, and never leaves my study," is well-known to the readers of this Journal; it was his most important work, and that which gained him his European reputation and his fame as "a great physician," which we cannot but think will be enduring. Whilst this work showed his eminence in the strict line of his profession, his other writings, such as these now published, and others omitted, display the variety of his knowledge, his science, his eloquence, his humanity.

How much we are indebted to him professionally we learn from the enumeration which Dr. Stokes makes of his friend's Contributions to Practical Medicine; we copy it, the list itself being instructive:

"1st. The employment of food and stimulants in fever, even from its earlier periods; in other words, their use by anticipation.

"2nd. The exhibition of acetate of lead conjoined with opium in spasmodic cholera.

"3rd. The development of the laws of pathological reflex action, as given in his Lectures on Paralysis, in which he has anticipated the views of Marshall Hall.

"4th. The employment of tartar emetic and opium in the delirium and insomnia of typhus fever.

"5th. The method of operating for the evacuation of hepatic abscesses by promoting adhesions between the hepatic and parietal peritoneum.

"6th. The observation of the latent periodicity in intermittent fevers.

"7th. The demonstration of the independent action of the capillary system in health and in disease, and the practical application of this doctrine in the treatment of diseases.

"8th. The account of yellow fever as it appeared in Dublin in 1826.

"9th. The observations of symmetrical diseases.

"10th. The nature and functions of the lymphatic system.

"11th. The influence of position on the pulse in health and disease.

"12th. The description of the disease lately termed exophthalmia cachectica."

The disease with which his name is most frequently associated is fever—we should write fevers, for he considered all of the continued kind merely varieties. The editor has with much propriety enumerated the leading points of his doctrine of fever and its treatment as given in the 'Clinical Medicine,' which, too, for the same reason as that already assigned, we transcribe:

"1st. Its existence as an endemic disease in Ireland, but occasionally taking an epidemic character.

"2nd. The existence of a general and peculiar character in each epidemic, subject, however, to modifications at different periods of the epidemic, and in different places, even at the same period of the disease.

"3rd. The contagious quality of all the forms of continued fever in this country, but especially the maculated fever, the occurrence of which, however, seems to establish a greater immunity in the individual affected from subsequent attacks than is observed in the other varieties.

"4th. The necessity of avoiding routine treatment, and the importance of the anticipative use of nutriment.

"5th. The doctrine that fever in Ireland was to be attributed neither to miasmata nor to deficient food."

The importance Dr. Graves attached to good feeding in fevers, and his claim to its discovery, is aptly shown by his once suggesting to his clinical class, that were they at a loss for an epitaph for him, they might find it in three words:

"HE FED FEVERS."

His career was cut short in his fifty-seventh year by a painful malady, which he bore with Christian and philosophical resignation. He died in 1853, in full possession of his mental faculties, another happy circumstance for which, during his protracted illness, we are assured he was thankful.

He was an F.R.S., yet, strange to say, though the President for the time was his countryman, the Earl of Rosse, no obituary notice of him was taken at the anniversary meeting following his death. Referring to the Proceedings of the Year, we find his name merely given in the list of deceased Fellows. This omission we regret, and for two reasons —one, because we think he eminently deserved to have had his name honourably mentioned from the chair, and recorded amongst the illustrious men who belonged to the Society; the other, because it would have afforded the President, in pointing out the merits of Dr. Graves, an opportunity of impressing on the minds of his hearers—a large number of them medical men, and many of them teachers in the metropolis—the excellent method of clinical instruction which Dr. Graves had inaugurated in Dublin; and, taking him as an example, might have reminded them how great is the influence of one man of acknowledged eminence in giving new life and vigour to old and sluggish institutions. From this point of view Dr. Graves might be compared with the first Monro, Whytt, or Cullen, to whom the Medical School of Edinburgh was hardly more indebted for its prosperity than is the existing Medical School of Dublin for its deserved repute and high standing. All this, except what relates to the Royal Society, is well insisted on by the editor; indeed, that portion of the memoir in which Dr. Stokes gives an account of clinical teaching at home and abroad, and of Dr. Graves' method, is peculiarly valuable and deserving of attention.

Our notice of the work itself must be brief. We have read it with pleasure, and are satisfied that for its intrinsic merits it deserves to have many readers. It is not so remarkable for the information it imparts as for the logical and philosophical spirit in which it is written. Dr. Graves' physiology is of a broad and comprehensive kind. This is well displayed in the second Essay, that on "The Position of Man in the Scale of Life."

Of the twenty-four Physiological Essays, these forming the first part of the work, each has its distinct subject, and yet they are all more or less connected—man in his various relations being chiefly dwelt on. We think that one or two quotations are best adapted to convey a just idea of the author's excellences of style and matter. The first we shall give shows his observing powers; it relates to the development of the senses:

"With respect to the senses in the infant, we find that the sense of smell is but little developed. The sense of smell, like that of taste, seems to be placed by nature as a guard or sentinel over the digestive tube, to warn the individual of the fitness or unfitness of the matters to be swallowed. In the infant, where the nature of the ingesta is generally regulated by the mother, the sense of smell is scarcely required, and hence we find it more or less imperfect, and the nose very small. We have also reason to believe, from the small size of the mouth, that the sense of taste at this period is by no means acute; in fact, these two senses appear to be developed in similar proportion. But when we come to examine those of sight, hearing, and touch, we find a very remarkable difference. The infant comes into the world with the eyes, the ears, and the organs of touch fully formed for use. These are the three senses which subserve to the acquisition of knowledge, and it was necessary that they should be well developed; the others have application chiefly to corporeal wants, do not call for the same precocious expansion. With respect to hearing and touch in the infant, there is no peculiarity deserving of notice; but it is a very curious fact that all children are short-sighted. We can easily perceive this by observing children when they are looking at a book or picture. Whether this arises from the greater convexity of the eye at that age, I cannot exactly say, but I am inclined to think that this is the principal cause. We, however, always observe children keeping their eyes very close to a book, and I do not recollect that I have ever known a child that did not see clearly at the distance of an inch or two less than the adult."

The next quotation we give displays his judgment in the application of physiological knowledge to education. After considering the ordinary manner of teaching, he remarks:

"It is, nevertheless, sufficiently obvious that in endeavouring to educate youth we are not pursuing a physiological plan when we make the attempt without having accurately ascertained by observation and study the faculties which distinguish the different periods of childhood and youth, and their peculiar adaptation to the various branches of science and literature. In youth, when the perception is quick, the curiosity ardent, and the memory good, the mind should be directed to the study of the physical qualities of things and the accumulation of names; and hence natural history, biography, history, languages, and geography, should be the employment of youth. Every species of knowledge which requires for its attainment perception, imitation, attention, curiosity, and memory (faculties which are all enjoyed by the child in a very remarkable degree), may be now communicated with facility. Youth, too, is above all others the period for cultivating the discipline and education of those mysterious and powerful springs of human action, the passions and affections. Languages, however, are not to be taught by commencing with the philosophical part, which is grammar. They should be taught in the way by which a child acquires a knowledge of its own language; a stock of words and phrases should be learned and fixed in the memory by repetition, reserving the study of grammar and syntax for a more advanced period. The practical precepts of grammar may be sufficiently enforced by judicious and well-selected examples, but we should never attempt to involve a child in the useless and perplexing task of studying the ultimate analysis of language. This is the natural and most successful mode of teaching languages, and yet how generally do we find an opposite plan pursued. The moment a child begins to learn a language, a grammar is put into his hands, and he is dragged through a maze of abstract terms and unintelligible definitions, and is required to understand and explain the complex and subtle nature of verbs, pronouns, prepositions, and conjunctions. He next proceeds to the study of syntax; he is now required to explain all the difficulties connected with the concord, government, and mutual relations of words; he is expected to be able to understand all the intricacies of grammatical abstractions—an expectation supposed to be fulfilled when the suffering scholar has learned a certain number of rules by rote, which he can mechanically apply for the explanation of grammatical construction. This is termed *parsing*, which a boy may learn to perform, much to the credit of his master and the edification of an audience, while all the while he understands as little about the matter as one of Babbage's machines, which, however, performs calculations much more difficult."

Of the twelve Miscellaneous Essays constituting the second part of the work, three only are of considerable length—one, on "The state of Medicine in European and Asiatic Turkey," though not without interest, we think might have been omitted, as not of value proportional to its length. It is a review of a book by Dr. Oppenheim, a German physician who resided some years in Turkey towards the latter end of the reign of Mahmoud, the father of the late and of the present Sultan. Irrespective of its length, there are details in it respecting abortion, &c., hardly fit for the unprofessional reader.

Another, an essay on the "Influence of the Waters of the Dead Sea and of the Great Salt Lake of America on Animal Life," is an admirable specimen of criticism. Dr. Graves' account of these waters and their shores affords a good example of his accuracy and his love of truth, and also how much more interesting a description of remarkable scenes may be made by a strict adherence to the real than by exaggeration and poetical embellishment. It is curious to see how such authors as Lieut. Lynch and the Rev. Dr. Robinson and others, writing under preconceived ideas, state particulars respecting the Dead Sea which are entirely imaginary, the name seemingly being the temptation.

Dr. Graves, adverting to the popular error associated with the name, says, "Recent writers and travellers have not failed to fall in with and confirm the popular prejudice on this subject. Thus Mr. Warburton calls the Dead Sea a *corpse*, and says, with much emphasis, 'There was no shell, or fly, or any sign of life along the curving strand which rose steeply to the water's edge, and consisted of very small angular pebbles.'" Dr. Robinson and the author of 'Eothen' both indulge in reflections respecting the absence of life, not only from the waters of the Dead Sea, but from the air above and the shores surrounding it. We find also Lynch bearing similar testimony: thus, at p. 311, he states, "No bird fanned with its wings the attenuated air, through which the sun poured its scorching rays upon the mysterious element on which we floated, and which alone, of all the works of its Maker, contains no living thing within it."

"This passage" (comments Dr. Graves), "I must confess, strikes me as being more poetical than philosophical, for, in the first place, no fact recorded by either Lynch or others justifies the epithet *attenuated* being applied to the air; and, in the second place, Lynch seems to forget that he himself several times met with birds both resting on the waters of the Dead Sea and flying over it."

Dr. Graves has done good service in exposing these and other errors and exaggerations. How few are the writings which will bear such criticism? Even Dr. Graves himself, usually so accurate, we perceive in one instance—it is, we believe, a solitary instance—has not escaped being carried away by the too common tendency of the human mind to use hasty expressions not warranted by facts and incompatible with them: thus in his "Essay on the Ubiquity of Life," we find the following: "Even mephitic pools, poisonous soils, and boiling springs, teem with animals and plants." Now, had he exercised his critical judgment here, he surely would have omitted *boiling springs*, the temperature of boiling water, as has been ascertained in the most demonstrative manner, being incompatible with animal or vegetable life.

The third, the last of the essays which we shall notice, is "On the

Progress and Contagion of Asiatic Cholera." It is one of the latest of his writings, and one to which he attached great importance, believing, as he did, that the disease is contagious or infectious, and that those who hold the opposite view were misleading the public and indulging in a most dangerous doctrine. Ably as this paper is written, as we read it, it has, we think, somewhat too much the character of special pleading; whilst, in a powerful manner, he adduces the facts which had come to his knowledge favourable to the conclusion he had arrived at, he passes over in great measure unnoticed other facts, as well established, opposed to his conclusion. This is not a place for discussing the much-vexed question of the contagious or non-contagious nature of the disease. So great is the obscurity of the subject, so much may be said on either side from conflicting evidence, that we can hardly even hope that the profession is soon likely to be united in opinion respecting it. Though a contagionist in the strictest sense, Dr. Graves is not an advocate for quarantine: whilst he seems to admit its protecting power (he overlooks the counteracting influence of the contrabandist), he admits the impossibility of carrying it into effect without occasioning greater evils than even the free admission of disease would produce—viz., the interruption of commerce and its consequences. His words are:

"We must bear in mind that Great Britain chiefly depends on trade for the support of millions of her population, and that any quarantine laws which had the effect of closing all the channels of commerce (and to be efficacious they should close them all) would speedily produce such distress in the manufacturing districts as would occasion starvation so intense, that more deaths would arise from the operation of this cause alone than could possibly be caused by the introduction of any disease, however infectious."

We must not part from these Essays without expressing our thanks to the editor for having given them publicity in their collected form. High as was their author's reputation before their appearance, if they do not heighten, they will not lower his fame; they are a confirmation, so far as writings can be, of the character of the man as described in the notice of his "Life and Labours."

Review V.

The Domain of Medical Police. (Abstract of a Paper read before the New York Sanitary Association, Feb. 6th, 1862.) By Louis Elsberg, M.A., M.D.

It is well that Englishmen should now and then hear what intelligent foreigners say about matters of public administration on which very strong, if not peculiar, opinions prevail in this country. But it would be almost impossible to place medical police in a point of view more opposed to our insular notions than a German physician, Dr. Elsberg, has done for the benefit of his adopted country. Possibly, the forcible suppression of liberty in the Federal States of America may have encouraged our author to show cause for state control of a far more justifiable kind than President Lincoln's despotic efforts to crush freedom of thought and speech. But right as Dr. Elsberg may be on many points, it cannot be denied that he advocates a more aggressive and intrusive system of sanitary police than we should deem consistent with constitutional government. Still there is something to be learnt from this little pamphlet.

We are reminded that there is a science of State medicine; that it embraces a variety of departments, each of vast importance to the physical, social, and moral welfare of every citizen; that for practical purposes these subjects require classification and normal treatment; and that no public action can be wisely taken upon any one of them without considering and settling its bearings upon the rest. Unconnected measures and shuffling expedients, adopted by any government to satisfy pressing demands or to meet sudden exigencies, are sure to prove either inoperative, or even absolutely mischievous, just in proportion to their non-recognition of admitted principles of social science.

Granting, as we must, that the actions of individuals require control for the safety and health-protection of the many, we must also admit that such control ought to be exercised by judiciously-constituted authorities, invested by statute with sufficient but not excessive powers of action, and enlightened by scientific advice. No sensible man questions the still greater advantages of intelligent self-control, the result of education and moral suasion; but at the same time he never doubts the necessity of laws to protect society against the injurious acts or neglects of the ignorant and the immoral; and so long as these latter classes constitute the majority of a people, legislative and administrative power should be vested in and wielded by the wiser minority.

Dr. Elsberg begins by discussing the meaning of the word "police." Popularly, it is used for the body of men, or the civil force, by which general laws, municipal regulations, and orders of magistrates are executed and enforced. But its derivation from πολιτεία—the mutual relations of the citizens of a state, government, administration—points out its real meaning. It is, however, subordinate to Polity, and it is correctly applied to those regulations of a state, a city, or a country which have for their object the maintenance of its internal peace, good order, safety, comfort, cleanliness, and health. According to Professor Mohl—

"Police embraces all the various measures and institutions which, by the exercise of the general powers of the community, aim to prevent or remove those external influences that interfere with lawful actions, possessions, and enjoyments of individuals, which the individual cannot wholly or satisfactorily prevent or remove without a violation of the rights of others or a disturbance of the peace of the community."

Dr. Elsberg accepts this definition, and infers fairly from it the necessity for *medical* police, which he in turn defines to be—

"That science which teaches the application of every branch of medical knowledge—or, more widely still, of physical knowledge—to the purposes of police. Medicine," he continues, "bears to police, as medical police, a somewhat similar relation as it bears to law as medical jurisprudence. And the difference, too, between the latter, or legal medicine—*medicina forensis*—and *politia medica*, is exactly that between law and police. Both medical police and medical jurisprudence are embraced in the term state medicine—*medicina publica*, or *medicina politico-forensis*."

Dr. Elsberg divides his "domain" into three departments: 1. Preservation of Public Health; 2. Removal or Cure of Disease; 3. Administration of Medical Affairs. This classification differs materially from that of most writers on state medicine, even those of the author's own race. We have not space for a comparison of Dr. Elsberg's arrangement with various French and German systems; but we may observe that it is neither philosophical nor methodical, though it assumes to be practical.

The matters contained in his second and third departments are not very clearly distinguished. The medical *personnel* and institutions, which he places under the second, belong rather to the third head; while certain regulations for the interment of the dead should be included with others relating to the same subject under his first head, Sanitary Police, except in so far as they belong to Legal Medicine, for which he has allotted no distinct department, although he repeatedly and very properly refers to its inseparable relations with Hygiene.

Under his main topic, Sanitary Police, he starts with the indisputable canon, that "the most important source of effective public hygiene is thorough, minute, and extensive scientific observation of the sanitary condition of the community and the causes affecting it." He asserts, truly enough, that such "skilled sanitary inspections can be made only by persons of a medical education;" that private physicians cannot make them "extensively or continuedly enough for the purpose; and that they require specially-appointed health officers." This correct practical distinction between the preventive and the curative functions is beginning to be generally acknowledged. The duties and responsibilities, the scientific researches and experiments, the records and the reports of a health officer, especially if he were to act also as a medico-legal expert, would be quite sufficient to employ all his energies and to occupy his whole time—assuming that the area of his jurisdiction were of sufficient extent. The attempt to combine these appointments with private practice, under parochial boards and in small districts, leads either to ruinous loss of curative engagements or to an imperfect and crippled execution of official duty, and thus to a depreciation of the office in the public mind. No kind of inducement to stand well with his neighbours as a therapeutist, even if merely as a consultant, should be offered or permitted to the medico-legal and prophylactic adviser. Inferior sanitary appointments, moreover, tend to involve the medical officer in a routine of meddlesome and unseemly interferences with the commoner and minor nuisances which belong more properly to the office of an ordinary policeman. We recollect that it was gravely proposed, a few years ago, to empower a metropolitan officer of health to seize all the stinking fish in his parish!

Our author divides his main department into three heads:

(1) The total extinction of causes of disease, to which this ardent reformer gives the name of "nosophthory," or the science of eradicating disease.

(2) Protective measures against contagious diseases.

(3) Protective measures against miasmatic diseases.

His "eradication" measures are subdivided into (A) prevention of hereditary diseases, and (B) removal of injurious external influences. It is to his recommendations under A that our countrymen would take the strongest objection. A quotation from Prof. Reese's 'Report on Infant Mortality'* shows the pace at which enthusiasts will run in pursuit of an idea:

"No marriage," says he, "should be permitted between parties until the physical health of both has been subjected to professional scrutiny [!], and such alliances should be prohibited by law to those of either sex who are the subjects of those diseases which are known to be hereditary or transmissible to offspring, or such as are fatal to infantile existence. Celibacy should be required by statute of all consumptive, scrofulous, scorbutic, gouty, insane, intemperate, and especially syphilitic individuals of either sex; and this for grave reasons of State which concern the public weal. Nor will any course short of such legal prohibition of marriages adequately correct the evil."

It is but fair to state that Dr. Elsberg withholds assent to such extreme and impracticable projects of legislation, and suggests that prohibition should be confined to the following cases:

"(1) Marriages before the age of sexual maturity; the precise age to be fixed by law. (2) Marriages between persons affected with diseases that will certainly, or with very great probability, be communicated to their offspring . . . . as insanity and syphilis. . . . . (3) Incestuous marriages, in regard to which laws should accurately define the various degrees of relationship within which marriage is prohibited."

Our chief biologists are now fully agreed that not only particular forms and forces of life, but also particular defects and diseases of organs, are transmitted by natural inheritance from parents to offspring; and unquestionable statistics have now proved that the liability to certain organic imperfections—as, for example, deaf-mutism†—is greatly intensified by marriages of consanguinity. The question, therefore, is no longer whether marriages which tend to produce an infirm or insane or diseased progeny ought to be contracted, but whether they ought to be prevented by law, and if so, with what limitations and exceptions. It is a large and difficult question, and not to be settled by off-hand assertions on either side.‡ In theory, the State is as fully justified in taking measures to check the hereditary transmission of certain loathsome and horrible diseases as in legislating to arrest the contagion of small-pox, to ward off pestilence, to cleanse and sanify towns and dwellings, to regulate the construction of buildings, to control common lodging-houses, slaughter-houses, &c.,

* Transactions of the American Medical Association. Philadelphia, 1857.

† See Boudin, Prosper Lucas, &c.

‡ Valuable papers by Mr. Sedgwick and Dr. Child, in recent numbers of this Review, show how much is yet to be learned on this subject. See also Whitehead on Transmission from Parents to Children, &c. 1857.

to banish noxious and offensive trades from crowded populations, to prevent the smoke nuisance, to prohibit burial of the dead among masses of the living, to drain malarious marshes, to improve river-currents, to secure adequate supplies of pure water to towns, to provide baths and washhouses for the poor, to prevent the adulteration of food and the sale of putrescent meat, to regulate the sale of poisons, to protect the lives and health of workers in factories and mines, and to limit the employment of women and children. Yet all this has been attempted, and some of it has been accomplished by the British Parliament. In practice, however, the State can do nothing of the kind effectually, unless supported by public opinion. All such enactments, therefore, depend for success upon the admission of certain principles by the majority of the educated classes; and, granting the expediency of laws against the more flagrant abuses of the marriage contract, we hope that Dr. Elsberg and those who think with him will some day see how much greater and more immediate benefit would accrue to society from hortatory and educational measures, from authentic information as to the consequences of improper marriages, and from public instruction in elementary physiology; thus leading the people voluntarily to avoid matrimonial connexions which must bring a curse on themselves, their children, and their country. But then all this advice and instruction should come from accredited sources. It should be given systematically by persons of known ability, of scientific attainments, and of high character, and as such authorized to teach others. It ought not to be left to amateurs and philanthropic triflers. It requires diffusion by more reliable means than tales and sensation articles.

Dr. Elsberg's brief notice of illegitimacy and prostitution presents nothing new. The suggestions of Dr. Griscom, of New York, are known to many of our readers. Those who have most fully investigated the subject admit generally that the Government of this country must, sooner or later, boldly face the difficulty. If congenital syphilis, that most horrid and mysterious form of human ill, is to be robbed of its innocent victims, if an unspeakable amount of domestic misery is to be prevented, if more than half of the costly sickness of our costly army is to be saved to the nation, the "social evil" must be fairly met as a social fact, and the simplest and most obvious methods of regulating and controlling it, under scientific direction, will prove to be the soundest and the best.

Under "Removal of Injurious External Influences," our author includes those measures which are called "sanitary" in England. It is needless to go over the whole ground with him. Each of his fourteen sections would suggest matter for a separate paper. But we propose in a future article to offer some remarks upon two of these subjects, in noticing what has been either done or discussed in Parliament respecting the adulteration of food and the injurious effects of various industrial occupations.

Before taking leave of Dr. Elsberg, we may say that there is nothing original in his cursory sketch of protective measures against contagious and miasmatic diseases. He includes both as "communicable abnormal conditions" under the general term "infectious," or as we call them, zymotic diseases. These depend, he says—"(1) on the communication to the organism of some material cause of a disease (*materies infectionis*); and (2) on the susceptibility of the organism—i.e., on its capability of becoming affected thereby, so that a disease *sui generis* is the product of the infection." To remove the susceptibility of persons exposed to infection he considers of at least equal importance with the removal of the infecting germ or ferment. Hence vaccination against variola, quinine against marsh miasma, &c. Though he admits that *ochlesis* or crowd-poison is the one essential cause of typhus, he seems to be practically a non-contagionist. He adopts without giving his authority the following opinions, which may pass for what they are worth among other ætiological *dogmata.*

"Wherever the excreta of human beings, from their lungs and skins as well as bowels and kidneys, accumulate in an unrenewed atmosphere for a considerable period, typhus will be produced, both miasmatic and contagious;—i.e., not only do certain places become tainted with it, so that all persons abiding there are liable to it, but a single patient with typhus taken to a new neighbourhood may generate the disease in other persons. The excretory zymotic agent, which ordinarily requires numerous bodies to afford it in typhus-breeding quantity, is so concentrated and dynamized in the body of the patient, as to have in it the poisoning power of a whole crowd. Thus, in typhus, the morbid poison is originated by the alteration and accumulation of matters naturally produced in the body. It is not so with the contagion of small-pox, the origin of which is not thus controllable by circumstances, in the absence of its specific cause, nor is it so with the localized infection of yellow fever, or the migratory poison-cause of cholera."

We may leave him here to the tender mercies of Dr. W. Budd and Dr. Murchison, perhaps also to his own not very logical conclusion from the premisses—namely, that "by a proper medical police and by attention to the laws of public and domestic hygiene, all malignant epidemics are preventible." This he trumpets forth, with many a Yankee flourish, as the "Gospel text of the evangelists of sanitary reform!"

By the bye, the varieties of type, the numerous capitals of all sizes, the continual italics and notes of admiration which abound in this pamphlet, certainly disfigure its appearance, while they do not compensate for its feebleness of style or remedy its defects of composition.

Review VI.

Lectures on Surgery. Delivered in St. Bartholomew's Hospital by William Lawrence, F.R.S.—*London,* 1863. 8vo, pp. 632.

To criticise in detail a work by a veteran surgeon so eminent as Mr. Lawrence, would be a task which we, as reviewers, would not willingly undertake; at the same time, we consider such a proceeding to a great extent superfluous, as it must be evident that it is impossible for any man who has become far advanced in age, and who for many years has been fully engaged in practice, to keep completely acquainted with every advance in the scientific portion of our profession. Still we cannot lightly pass over even antiquated opinions when held forth by a man so experienced as Mr. Lawrence, whilst we should always remember how "*Opinionum commenta delet dies, naturæ judicia confirmat.*" It will, we think, be sufficient to enumerate the contents, to illustrate the style by rather copious extracts from such parts as appear more especially important, and to add a few explanatory or critical remarks of our own.

Mr. Lawrence commences his work with an introduction, in which he shows the absence of any real distinction between physic and surgery, and to which he appends some valuable remarks on the best mode of study. The medical man cannot be too well acquainted with anatomy; with every part of the body, even its minutest details, he should be perfectly cognizant, and not only when healthy,—he should be familiar also with the appearance of every part when diseased. For the operator, regional anatomy, the knowledge of the exact relative position of different parts, is, however, the matter of supreme importance; it is on this knowledge, and on skill gained by the practice of operations on the subject, that he should depend; for an unskilled man to operate on the living being—a practice so common at the present day, when every apothecary calls himself a surgeon—is a disgrace to the medical profession. If a man has the natural qualification for an operator—quickness of intellect, coolness of temper, steadiness of hand, youth,* patience, and above all judgment, he may by practice on the dead body almost always acquire such skill in operating that he will run no risk at his first attempt on the living; such a saying as the one attributed to Mr. Guthrie, that to learn to extract, it was necessary to spoil a hatful of eyes, is in our opinion utterly false. We urge this, because practice on the subject is not generally estimated at its true value; of course, the operating surgeon should avail himself of every opportunity to improve, and if he is worthy of his name, he will. He should despise no part of anatomy; when he can spare time for systematic dissecting, he should do so; vivisections, which have recently been so much denounced, are, notwithstanding, of service, even as a preparation for the operator. Above all, however, he should constantly practise on the subject, never satisfied till he can perform each operation with neatness, accuracy, and certainty; he should frequently make surgical dissections, cutting, as it were, windows here and there in the body, and rapidly laying bare all the more important parts, such as the great bloodvessels, nerves, and muscles. Such dissections will enable him to find his way when every other means would fail him.

Our remarks may appear trite to some, but the simple fact that no tolerable book on the subject of surgical anatomy has been published in England for the last ten or twenty years—no book that can com-

* A qualification not usually admitted either by the public or profession. Celsus, however, says, in a well-known passage: "Esse autem chirurgus debet adolescens, aut certe adolescentiæ propior."

pare with the works of Hyrtl, Engel, Jarjavay, Malgaigne, or Roser, is a sufficient proof that the subject is not appreciated in this country, and a sufficient excuse for our tediousness. Mr. Lawrence very justly says that—

"No man who aspires to a scientific knowledge of his profession can neglect the sciences of anatomy and physiology, because they afford the foundation and criterion of all medical doctrines; but correct anatomical knowledge is especially necessary to the surgeon as a preparation for his ordinary duties; without it he cannot determine the seat and nature of disease; he cannot distinguish between the affections of contiguous parts; he cannot understand the varied nature and appropriate treatment of injuries, such as fractures, dislocations, or wounds of bloodvessels and other soft parts. If you ask how much knowledge of anatomy is necessary for a surgeon, the answer is short—as much as he can get. Your study of anatomy must be general; it must embrace the whole frame—unless, indeed, you should know of any part which is out of the reach of injury, or exempt from the attacks of disease, or any region which can never be the subject of operation.

"Operations may, in some instances, be executed mechanically, by following certain rules; but if things do not go on exactly according to the description, the operator, ignorant of anatomy, is immediately confused, embarrassed, frightened. In most cases, too, unless the knife be guided by minute anatomical knowledge, operations are attended, not only with unnecessary suffering and risk to the patient, but also with the greatest danger to the reputation of the operator.

"I trust, gentlemen, that you will not be anxious to discover how small a stock of scientific knowledge will enable you to carry on the trade of surgery. Your more honourable course will be to render yourselves accurate anatomists, as the most essential step towards becoming good surgeons. The health, the limbs, the lives of our fellow-creatures are intrusted to our care, with a confidence in our knowledge, skill, and humanity; our utmost exertions in the acquisition of knowledge will not do more than enable us to undertake this serious responsibility. What kind of feelings, what conscience, can the man possess who can plunge an instrument into the human body without knowing what he may divide or injure, who can operate without that full anatomical knowledge that will enable him to meet every emergency? How could he bear his own reflections, if serious and permanent injury or loss of life should ensue, as the consequence of his ignorance and rashness." (p. 13.)

There has been much difference of opinion as to the value of lectures: the corporate bodies have for the most part seemed to imagine, that by obliging students to attend certain courses, in which such and such a number of lectures were delivered, they could drive a certain amount of knowledge into them; on the other hand, it appears clear that as the rule, lectures must be inferior to books; the lecturer may be most indifferent, stammering, and imperfectly acquainted with his subject, and yet the student must attend him; the book may be the best extant for matter or for manner, or several books may be used together, so as to combine the peculiar advantages of each. An author of some celebrity has stated of lectures:

"These, whether public or private, are surely the very worst modes of acquiring any sort of accurate knowledge, and are just as much inferior to a good book on the same subject, as that book hastily read aloud and then immediately withdrawn, would be inferior to the same book left in your possession, and open at any hour to be consulted, retraced, collated, and in the fullest sense studied."*

Of course, these remarks do not apply to lectures in which all the more important facts are demonstrated—demonstrations, as they might be more fitly called. When the lecturer on medicine exhibits to his class a case of favus or a specimen of cancer when the chemist shows the process by which a gas is prepared, or when the surgeon ties an artery, so that each student sees clearly and distinctly, there can be no question that such a demonstration is better than a book. For the medical student to learn anything efficiently, it matters little what, the first requisite is to see the facts exhibited, the second to trace them afresh for himself. The knowledge acquired from simple lectures or from mere reading is too often counterfeit, that from demonstration or self-research is the sterling coin. What General Jomini long since said of the art of war† is also true of medical practice:

"Dans tous les arts comme dans toutes les situations de la vie, *le savoir* et le *savoir-faire* sont deux choses tout à fait différentes, et si l'on réussit souvent par le dernier seulement, ce n'est jamais que la réunion des deux qui constitue un homme supérieur et assure un succès complet."

As Mr. Lawrence says:

"The wards of an hospital are the best school of medicine; and clinical study, under the guidance of a competent teacher, is the best mode of learning. You will immediately inquire whether it is not necessary to hear lectures and read books before you begin to see patients. I advise you to resort as early as possible to nature, to that source from which the masters of our art have derived their information; from which lecturers and authors must draw their knowledge. Here it may be said with perfect truth, *Juvat integros accedere fontes atque haurire.* In learning anatomy you have the facts demonstrated to you by the teacher, and you examine and explore them for yourselves by dissection. In the same way, demonstration of the phenomena of disease on the patient by the teacher, and the actual observation of them by the learner, are the only means by which real knowledge of the subject can be acquired. The facts thus presented to the senses make a stronger impression than any description, even by the ablest lecturer or writer; while the information which a person thus acquires for himself from nature can always be depended on, and is never forgotten. Between him who has only *read* or *heard*, and one who has *seen*, there is the same difference, in point of knowledge, as between a person who has merely perused the description of foreign countries, and another who has actually visited them. To secure these advantages to their full extent, instruction should be strictly clinical—that is, the symptoms of disease, and the changes it produces, should be actually pointed out and explained on the patient; their origin, progress, and connexion should be illustrated, and the indications and modes of treatment should be deduced from the facts thus immediately observed. This kind of clinical instruction can only be given at the time of visit. I have always endeavoured to explain diseases in this manner to the pupils of the hospital, and I shall continue to illustrate clinically in the wards the general doctrines which I deliver in this theatre.

* A remark quoted by Mr. Thomas Alcock in his interesting essay on the Education and Duties of the General Practitioner in Medicine and Surgery: Trans. of the Assoc Apoth., vol. i. p. 1. London, 1823.

† Tableau analytique des principales Combinaisons de la Guerre, 4th ed., p. xvi. St. Petersburg, 1836.

"After beginning to observe diseases for yourselves, you may have recourse, with advantage, to lectures and books, which may be of great use in teaching you how and what to observe; in pointing out what might escape observation; in elucidating what may be obscure and perplexing; in rectifying erroneous conclusions; and in impressing the results of observation more strongly on the memory.

"I cannot help thinking that too much importance has been attached to lectures. From the long prevalent custom of attending them, and the regulations of the public bodies which require certificates of attendance before admitting candidates to examination, the belief seems at last to have been produced that medicine can be learned in this manner. This is a great mistake. The medical sciences rest on observation, and are only to be acquired by resorting to nature. The great number and intricacy of the phenomena are additional reasons why we should examine for ourselves, and not take the facts at second or third hand. A few cases attentively observed will teach you more than any lectures or books. If you attend to nature with an unprejudiced mind, you cannot go astray. Lecturers and writers often copy from each other without resorting to the fountain of knowledge: can we wonder that they frequently mislead and deceive instead of instructing?" (p. 15.)

These preliminary remarks are followed by a chapter on the Nature and Divisions of Diseases, their Arrangement and Classification, from which we find that Mr. Lawrence is not a believer in purely functional diseases:

"The division of disease into functional and organic, according to the view that we now take, denotes a difference in degree, rather than in essential nature. In diseases called functional, the changes in the organs are of the slighter degree, which leaves little or no trace behind; while in the organic the alterations are more considerable, permanent, and dangerous. To assert that functions are disordered when their appropriate organs are in the normal state, would be the same as if a watchmaker, when our watch goes wrong, should tell us that all its parts and their connexions are perfect, but that the motion is in fault. We should be justly dissatisfied, and recommend him to examine more minutely. In the same way, when medical men are puzzled, they must inquire more deeply and cautiously, in order to find out what may have escaped observation on the first occasion.

"This is not a merely speculative point, but one of considerable practical importance; for they who believe in strictly functional diseases direct their treatment according to that view. The endeavour is to remedy the imperfection of the function, to excite the vital properties which seem to be deficient in energy, to rouse those that appear to be dormant; such treatment being calculated in many cases rather to aggravate than to remove disease. On such pathological views the treatment of palsy would be directed to the loss of power in the paralyzed parts, to the neglect of the real seat of the mischief in the brain. In the same way, amaurotic affections, regarded as weakness of sight, have too often been treated by tonics and stimuli of all kinds, with the sure result of increasing the disease of the retina and hastening its progress." (p. 27.)

As to classification, he considers that—

"it might be convenient, in order to avoid controversy, to arrange the affections of each organ on a simple plan not involving any theoretic views of general pathology. Thus, affections of the eye might be taken in the following order: First, injuries; secondly, inflammations, divided into two heads, common and specific; thirdly, consequences of inflammation; fourthly, organic changes; fifthly, functional derangement. We shall hardly find anything that can be said about the diseases of the organ that is not referable to one or other of these heads. An arrangement of this kind does not take us beyond the extent of our knowledge." (p. 32.)

Inflammation is discussed at length; but although Mr. Lawrence's account is not without value, many interesting remarks being scattered throughout it, still it is rendered unnecessarily tedious by the admission of hypothetical remarks and of useless details. The local changes which are produced by inflammation in the quantity and composition of the blood, are well exemplified by a case of violent inflammation of the hand, in which the vein was opened in each arm at the same time. About fifteen ounces of blood flowed from the inflamed limb, while three ounces were escaping from the opposite side. That from the latter had a thin buffy covering, while the former presented a thick layer of the toughest fibrine, and was cupped to the utmost extent.

The author remains firmly convinced of the great value of bleeding, nor does he believe that the inhabitants of London cannot bear it. His experience has shown him that "inflammations are as numerous among cockneys as among countrymen, and that they can only be counteracted by the same means, which are just as necessary and safe in the one case as in the other." The subject of inflammation is followed by the exposition of its effects, such as suppuration and ulceration. With reference to the latter, Mr. Lawrence agrees with the popular opinion that it is occasionally dangerous to heal up large sores rapidly, and enforces his opinion in the following words:

"I take this opportunity of impressing on your minds the necessity of watching closely the health of those in whom large sores, especially of long standing, are rapidly healed, or in which extensive outward disease of any other kind is suddenly brought to an end. The copious and generous diet, with the stimuli which have been necessary in a state of weakness to sustain failing power and restore strength, are now sources of danger, and may lead, by overfilling the system, to serious disturbance in the head, chest, or abdomen. I have seen alarming and permanent paralytic symptoms and fatal apoplexy from an entire neglect of precaution in this matter. In proportion, therefore, as old discharges or other external disease are brought to an end, the state of the circulation and the excretions should be carefully observed, the quantity of food, and especially of stimuli, being changed in conformity with the altered condition of the body. Our forefathers were probably not altogether wrong in advising that an issue should be opened in the leg when an old ulcer was on the point of closing." (p. 193.)

Passing over a chapter on Mortification, and another on Wounds, we arrive at one on Hydrophobia, Bites by Venomous Serpents, and Dissection Wounds. As to the latter, Mr. Lawrence appears to doubt the existence of any poison in the great majority of cases, where the local and constitutional symptoms are analogous to what result from similar injuries where no suspicion of poison exists. Nitrate of silver should not in general be resorted to, for in some cases its free use appears to have been followed by the worst effects, as in a case related by the author, where the active use of this caustic was soon followed by intense pain, mortification, septicæmia, and death:

"There is room for doubt in this case, whether the fatal event was owing to the original puncture, if indeed there had been such a puncture, which does not seem quite clear, or to the very energetic escharotic treatment resorted to under the influence of apprehension and alarm. The result might have been different if fomentation and poultice had been trusted to, with opium internally. A free division of the inflamed part might have been resorted to if painful swelling and tension had supervened." (p. 277.)

The physician should modify his treatment according to the general and local symptoms, of which two good examples deserve quotation:

"A gentleman of large frame, full muscular development, and in excellent health, scratched his thumb in sewing up the body of a female who had died of peritoneal disease. Most violent pain came on in the night, shooting up the limb, which swelled, and became hot, with great constitutional disturbance, marked by headache, restlessness, agitation, nausea, and shrunk countenance. He immediately lost thirty ounces of blood by venesection. On the next day the limb was greatly swelled, with inflammation of the absorbents. Leeches were applied to the part, and cold to the head. The swelling and tension of the limb, with great pain and constitutional disturbance, were all worse on the following day, when venesection was twice repeated, with numerous leeches on the limb. I saw this gentleman, an intelligent and well-informed surgeon, on the following day, the fourth from the commencement of the attack. There was inflammatory enlargement and tension of the entire limb, with great suffering. The patient believed that matter had formed in the ball of the thumb, which was enlarged and painful, and I made a deep incision at his urgent request; a small quantity of pus escaped, and subsequently thirty ounces of blood flowed, with relief of the hand. There was still intense suffering in the forearm and arm, which could only be relieved by the local abstraction of blood, for which purpose leeches were put on by handfuls, without counting, from time to time during this and the following day. There was now a complete change in the local and constitutional symptoms, a lowered pulse, with general depression. Opium was resorted to, with generous diet, and gradual but complete recovery of health and strength, which lasted to the time of his death at a somewhat advanced age."

"A complete contrast to the foregoing picture was presented in the state of a young physician who had pricked the forefinger in sewing up a body. Some pain was felt during the day; it increased on going to bed, and became so severe as to deprive him entirely of sleep. I found him on the following morning with a haggard countenance, expressive of anxiety and alarm, a small and feeble pulse, with great sense of weakness, and severe pain of the finger, without much external change. It was determined by another medical friend and myself to recommend an immediate glass of brandy and water with laudanum, such liquid nourishment and stimuli as could be taken, fomentation, and warm poultice to the part after leeching. These means were most beneficial. Suppuration, however, took place in the theca, with sloughing of the tendons." (p. 280.)

Scalds and burns, the sympathetic effects of local injuries, delirium tremens, tetanus, scrofula, gout and rheumatism, call for little remark. We may notice, however, a somewhat unusual application of the actual cautery at a red heat to nævus:

"Its transient application in this degree will sometimes enable us to accomplish, in an easy manner, effective but extremely superficial disorganization. An infant, under eight weeks of age, had one side of the face and lower jaw, with part of the ear, covered by a thin nævus, which had spread to this extent from a small spot observed near the nose soon after birth. It was confined to the skin, not forming one mass, but in separate portions, scattered irregularly over the space mentioned, and communicating by vascular ramifications. It was completely destroyed by a red iron passed quickly over the principal portions, and leaving light-brown marks behind. I saw the child a few years after, and should hardly have known that anything had been done to the face if I had not been reminded of it. Cases of nævus are not unfrequent, in which this method might be resorted to with advantage." (p. 289).

A case of delirium, treated by digitalis in large doses, is worth noting; three drachms of the tincture were administered to a female, about fifty, with a fracture of the leg, after two or three large doses of laudanum had been taken without effect; the digitalis quieted her immediately and permanently, inducing a tranquil state of the circulation, under which recovery went on most favourably.

Syphilis and malignant disease follow next, and conclude the volume; they are treated at such length as to fill nearly half its pages. On the latter subject we find nothing of particular interest; to the former we must devote a little space.

It has been very generally admitted, that syphilis could not have existed among the ancients, because it has not been specially distinguished and described by the writers of Greece and Rome. Mr. Lawrence disposes of this argument in the following conclusive manner:

"They who believe that the venereal disease did not exist in the world till the very end of the fourteenth or the beginning of the fifteenth century, and that it then broke out all at once in a mysterious and inexplicable manner somewhere or other in the South of Europe, allege the silence of preceding writers as a conclusive argument in favour of their opinion. It is true that the venereal disease is not described by name before that period. Syphilis and lues venerea were words not heard of before the sixteenth century. The great variety of affections embraced under our present notion of syphilis, and the relations they bear to each other, constitute an important subject entirely beyond the comprehension of the barbers and ignorant men to whom the care of external complaints was turned over in the middle ages; the natural history of the complaint is as yet but imperfectly understood. A long period must have elapsed before constitutional symptoms occurring weeks and months after the primary affection could have been understood as consequences of the latter, and before the train of subsequent affections, sometimes prolonged for years, could have been appreciated. In respect to this argument, drawn from the silence of preceding writers, let us remember that it has been doubted whether the contagious eruptive fevers were known to the Greek and Roman physicians. The Arabians pointed out their existence clearly, but their distinctive characters, though apparently obvious to common observation, were for a long time neither perceived nor acknowledged. Small-pox, measles, and scarlet fever were considered as mere varieties of the same disease, till near the beginning of the eighteenth century; and the diagnosis of measles and scarlet fever was not established till 1793, in the second edition of Dr. Withering's essay on the latter. The small-pox was known for centuries before its contagious nature was recognised, and even Sydenham ascribed it to the state of the atmosphere, with only a vague and doubtful reference to contagion. These considerations show us that the truth may escape the notice even of enlightened observers, and lead to the conclusion that the diseases of mankind, like their moral and physical constitution, are likely to remain unchanged." (p. 346.)

The author's description of syphilitic affections of the toes and nails is interesting and instructive. Some further information may be found in the Transactions of the Pathological Society of London;* where Mr. Jonathan Hutchinson has described and figured the affections of the nails which occur in connexion with both acquired and hereditary syphilis. Dr. B. W. Richardson has also given an essay on this subject,† and has endeavoured to show‡ that a diseased condition of the nails, much resembling that from syphilis, may arise independently, and may be treated successfully by arsenic:

"The old name of *rhagades digitorum* is applied to a peculiar and painful ulceration of the toes, affecting their intervals and opposed surfaces, of which the cuticle becomes thickened and opaque, while the cutis is partially livid, yielding a thin, brownish, and very offensive discharge. It may affect one or two intervals, or the whole series. In the worst cases, the ulceration occupies the entire palmar aspect of all the toes, and extends to the prominent margin of the sole immediately behind them; the surface, which is that of denuded cutis, being smooth, of irritable appearance, and yielding not pus, but a light yellowish, and clear exudation.

"With this affection, or independently of it, we may meet with an inflammation and foul ulceration at the root of the nail,—of the great toe more particularly,—sometimes called onychia maligna, probably from the high inflammation, the pain, offensive discharge, and intractable nature of the disease. It may also be seen in a less troublesome form in one or two of the smaller toes. The rhagades may occur alone or in conjunction with other symptoms, more particularly scaly eruption.

"I have seen the nail of the forefinger completely loosened by an ulcerative process, so that I was able to separate it without pain from its cuticular connexions. The exposed vascular surface was in a discharging state, and soon healed under simple applications. The patient, a female, had other symptoms of syphilis.

"A gentleman consulted me on account of disease in the nails of both hands, all of which were affected more or less. The nails were separated from the subjacent parts at their loose edge, the attached base being in its natural state. The affected portions had a light-brownish appearance, the ends of the fingers being somewhat reddened, warm, and tender, particularly on exertion. The separated portions of the nails were dry and fissured. Although this was such a case as I had not seen before, the suspicion of syphilis crossed my mind; and I inquired whether there were, or had been, any other symptoms, to which he immediately gave a negative reply. Not feeling quite satisfied, I asked if he had anything of eruption. He said not; but on further urging the question, he said there might be something very slight on the arms. When they were uncovered, several spots were found on them sufficiently characteristic of syphilitic origin, which he referred to a complaint contracted many years before, previously to his marriage. I prescribed mercury and warm bath, under which the heat and tenderness ceased, and the progress of the mischief was arrested. He was going on most favourably, when he grew tired of the treatment, and left it off. All the symptoms returned in a short time, and he was obliged to go through a regular mercurial course, so as to affect the mouth, with the result of a complete and permanent cure." (p. 417.)

Mr. Lawrence has frequently employed cinnabar fumigations in phagedænic affections of the pharynx and fauces, with beneficial influence on the morbid surface; the process is, however, irritating and

* Vol. xiii. p. 259. 1862. † Asclepiad, vol. i. p. 30, with a plate.
‡ See our number for July, 1862, p. 244.

painful; it is also not free from danger, for he has seen acute and fatal bronchitis excited by it.

The most interesting portion of Mr. Lawrence's exposition of the venereal disease is that in which he relates his experience of sloughing and phagedænic sores. In the treatment of the former he is much in the habit of dividing the prepuce with a curved bistoury, carried along a director, which has been introduced from the orifice to the angle of reflexion. Copious bleeding usually follows, with great relief both to the local and the constitutional symptoms. The author states the indications for such treatment in the following words:

"To determine whether the prepuce should be divided or not is sometimes a difficult point of diagnosis. The degree of redness, swelling, and pain, will not enable us to decide. The propriety of the measure depends on the condition of the sore, which we cannot see. The discharge from the orifice of the prepuce must assist our judgment in doubtful cases. An ichorous or sanious state of discharge, with fœtor, indicates sloughing; and in such circumstances the division ought to be performed. If the discharge should be purulent, even though somewhat bloody, and the glans not tender on pressure, we may be contented with leeches, tepid syringing, and mild aperients.

"In cases of primary sloughing sore on the glans, its most frequent seat, I have found incision of the prepuce necessary, even where it has not been much swelled and enlarged, nor in the state of phymosis. It has always been most beneficial, not only as a measure of temporary relief, but in respect to the progress of the sore." (p. 399.)

If the fetid discharge be attended with general swelling, redness, and severe pain of the penis, there is, of course, all the more reason for an immediate operation. The objection to slitting up the prepuce, that the cut edges may become phagedænic or sloughing, is completely unfounded, according to the author. In a large number of cases where he has adopted this proceeding no such result has ever been noticed:

"A lad of sixteen, who had recently left the country, contracted syphilis soon after he arrived in town. He immediately consulted a general practitioner, who observed nothing alarming in the case. This was on a Tuesday, and he was seen again on the following Saturday. In the meantime, he had been going into the city daily, from a distance of three miles, and returning; had been actively employed in business during the day, and living in his usual manner in the family with which he resided, the weather being very warm. The penis was now swelled, red, and painful, the orifice of the prepuce contracted, and there was a degree of feverish disturbance rendering him incapable of further exertion. Depletion by venesection and leeching, with rest in the horizontal position, and other corresponding antiphlogistic measures, were immediately resorted to, and followed up effectively. The local mischief, however, seemed to advance, and loss of the penis was feared. I saw the patient on Wednesday, when he was in a state of the greatest discomfort from severe bodily and mental suffering. The penis was swollen generally, the prepuce more particularly, while a thin, bloody, and offensive discharge, with the peculiar fetor characterising these sloughing sores, issued abundantly from the contracted orifice of the latter. The part was so extremely painful that he could not bear the slightest touch. He was enfeebled by the depletion he had undergone, and by the entire want of rest, caused partly by the constant pain, partly by mental anxiety and depression, with apprehension that he might lose the member entirely. I divided the prepuce in its whole length along the upper surface of the penis, finding that the most marked relief is experienced in such cases when the inflamed and ulcerated glans is liberated from the pressure which is caused by the swollen prepuce. Free bleeding ensued, as it usually does, from the edges of the incision. The relief thus afforded, by unloading the vessels immediately engaged in the inflammatory mischief, is an important source of additional benefit in this mode of proceeding. One half of the glans had perished, and was converted into a dark, greyish slough, separated from the living parts by a distinct line. Poppy fomentation and soft-bread poultice constituted the local treatment; and the muriate of morphine was given at night to procure rest. On the following day he was much better; the division of the prepuce had been followed by immediate and effectual relief to the part, and the opiate had procured a good night. The slough was separating. In two days' time the separation had advanced considerably, leaving a rather foul and ragged surface, to which lint, dipped in the balsam of Peru, was applied. In two days more the surface had become quite clean, and the balsam was left off, as its application caused pain. Soon after, the mortified part became entirely detached; although the slough extended deeply between the glans and the body of the penis, the urethra escaped. The sore now healed rapidly. The measures of general treatment, after the prepuce had been divided, consisted in a nutritious diet, with porter and wine, and the sulphate of quinine, in two-grain doses, three times a day. No secondary symptoms ensued in this case." (p. 465.)

Mr. Lawrence employs a mercurial treatment of phagedæna. To us, who believe in the doctrine of a dualism, and consider that syphilis has no more necessary connexion with a simple chancre than that an open sore forms its readiest point of entrance into the system, and that the ordinary soft sore is only a virulent ulcer of the genitals, a purely local affection, and never the cause of constitutional symptoms, it seems clear that phagedæna, which in the mass of cases attacks the latter form of venereal disease, cannot generally require mercury. We do not deny that occasionally such treatment is beneficial, for we ourselves have in several cases seen phagedænic ulcers stopped by the use of that mineral. On this point, however, the author speaks so strongly, that his very words demand insertion:

"I have seen many cases in which the prepuce has been slowly eaten away, nearly to its attachment, by a process so little painful that the patient has hardly sought for a remedy. In all of them, the regular and mild use of mercury in the hospital has soon stopped the progress of the disease and effected a sound cure. I have seen a case in which the entire prepuce has been destroyed in this way, the use of mercury having been absolutely forbidden. I have known of a similar result where the glans was the seat of the disease. When the phagedænic sore is proceeding actively, destroying the affected parts by an ulcerative, not sloughing, process, without surrounding inflammation or material constitutional disturbance, the active use of mercury, both locally and internally, is indicated. At an early period of my practice, when doubts and difficulties had been raised and uncertainty prevailed respecting mercury, I saw a seafaring man from Wapping with a large, quickly-spreading ulcer of the glans, eating deep, with foul phagedænic surface. It spread for three or four days under my fruitless efforts to do good by soothing treatment. At this time the patient was fortunately seen in consultation with me by the late Mr. Thomas Blizard, an adherent to the ancient faith of mercury, which he immediately pronounced to be the only remedy for saving the remainder of the glans; it was immediately resorted to internally, and by friction, with black wash to the part. The mouth was quickly and profusely affected, and rapid cicatrization ensued.

"As the phagedænic and sloughing sores are allied forms of disease, the one passing by insensible gradations into the other, we may be at a loss to determine whether to administer mercury or to adopt a mild and soothing local treatment. When the sore presents a blackish or dark-brown slough, it is a case for soothing local measures, perhaps with some direct antiphlogistic treatment; if the surface be grey, white, or phagedænic, mercury may be advisable. In a doubtful case, if there is local inflammation, with febrile constitutional disturbance, try antiphlogistic means; if these fail, employ mercury decidedly; should that aggravate the symptoms, opium should be used internally and locally, with iodide of potassium and sarsaparilla." (p. 600.)

Although we have thought it only right to quote Mr. Lawrence's opinion at length, we must add that free destruction of the phagedænic surface with the actual cautery at a white heat appears to us more to be recommended than mercury; the local use of nitric acid in sloughing phagedæna, which is mentioned with favour by the author, is far less effective than the hot iron, and now that chloroform has deprived the latter of its horrible character, there is no reason why it should not again come into general use.

Such appear to us the more important portions of this work; from them we have made rather free quotations, because we considered it due to Mr. Lawrence that his opinions should as much as possible be related in his own words. We may now express our general impression that although the author has thought fit to write on the principles of surgery, it is to the portions of his work that treat on the practice, that it will owe its value. More than thirty years have elapsed since Mr. Lawrence published his lectures in the 'London Medical Gazette,' yet we find on comparison that the matter in every important particular remains unchanged; minor alterations abound, the essence is the same. Many parts are so antiquated or imperfect, that the younger student will not read this book with advantage: on the other hand, the practitioner will refer to it as the record of the experience of an illustrious surgeon.

To conclude, we must express our regret that the author has not drawn the conclusions which so naturally result from the considerations exposed in his introduction. These conclusions could scarcely be better explained than in his own words:*

"Whatever view of the subject we may take, the same conclusion presents itself to the mind with irresistible evidence—namely, that there is no natural distinction between surgery and physic; that they are merely parts, and inseparably united parts, of one science and art; that the practical principles rest in both on the same scientific foundation; that the two branches of the profession must, in most instances, adopt the same proceedings, because they have the same purposes to accomplish, while their occasional differences are merely unimportant modifications in the means of arriving at the same end. The distinction then turns out at last to be a matter of usage, and no longer maintainable, except by those who content themselves with the mere surface of things, or by others, who may be determined to find reasons for upholding established custom, whether right or wrong, when their own interests are involved.

"These remarks apply only to the study of the medical profession, which must be learned as a whole. The various parts elucidate each other so mate-

* Lancet, vol. xi. pp. 3, 5, 633.

rially, that he who confines his investigations to one department cannot understand even that thoroughly. I do not contend that every one should practise all parts; and I am fully aware that the field is far too extensive for one individual to cultivate the whole minutely with a view to further improvement. No doubt that one who has received a general medical education may improve a particular department, if he should have his time fully occupied with it; and that circumstances of taste, convenience, situation, and public opinion may thus lead to subdivisions in practice with advantage to the public and benefit to science. This point, however, must be left to the free choice of individual practitioners and patients.

"If I were to propose a legislative enactment on the subject of the medical profession, it should be short, and confined to the single point of preventing uneducated and unqualified persons from practising. I would appoint an examining body, to be composed of the most eminent professional characters, without any regard to the present artificial distinctions, and empower them to make an efficient examination of all candidates: the testimonial granted after such an examination should authorize the individual to practise any or all parts of medicine, and none should be allowed to practise without it. This plan, which is recommended by its simplicity, has been already adopted with the best success in other countries."

Review VII.

A Clinical Memoir on certain Diseases of the Eye and Ear consequent on inherited Syphilis. By Jonathan Hutchinson, F.R.C.S.—*London*, 1863. 8vo, pp. 259.

Indefinite and uncertain notions as to the prevalence of inherited syphilis have existed and have been the source of perplexity in the investigation of the large class of infantile disease known as "strumous." That a close alliance holds between these and constitutional syphilis has long been the conviction of the profession; but the extent and the true nature of this alliance has not been so easily made out. Mr. Hutchinson's attention having been directed to certain features observable in the truly syphilitic cases, he has succeeded in dispelling much that was "vague conjecture," and has thereby considerably limited his belief in the actual amount of inherited syphilis.

A great portion of the volume before us is occupied with demonstrating the syphilitic origin of interstitial keratitis, "scrofulous corneitis" of Mackenzie, who speaks of it as a form of inflammation specifically different from every other ophthalmia, but a form of disease not very frequently met with unless at an ophthalmic hospital. Then Mr. Hutchinson states that at the Metropolitan Free Hospital, where the average daily admission of new surgical cases is between twenty and thirty, he has not had to treat more than one case a year. Mr. Hutchinson would, of course, not be likely to overlook any examples of the disease. Dr. Mackenzie had observed that the subjects of this affection are also often the subjects of a peculiar hoarseness of the voice, of swollen lymphatic glands, and of nodes on the tibia, and other "scrofulous symptoms." Mr. Hutchinson expresses surprise that Dr. Mackenzie should have come so near to, and yet have missed the true nature of this ophthalmia, and avows his belief that "interstitial keratitis of typical form" never occurs but in the subjects of inherited taint. What this "typical form" is, it is the author's present object to show. In order to establish his position, he goes with much care into the examination of the symptoms and history of a hundred and two cases, which he has also tabulated so as to show their distinctive features, and to which he has appended explanatory and illustrative comments. The narratives of these cases are illustrated by coloured plates, which exhibit the morbid appearances of the eye and the peculiar malformations of the teeth which characterize the syphilitic taint. Mr. Hutchinson assigns the following reasons for his belief in the syphilitic origin of the typical form of keratitis—reasons which may also be transferred to other forms of disease :

"1. That in certain instances patients whom I knew beforehand to be the subjects of inherited disease have, whilst under my observation, been attacked by it.

"2. That in a large number of other cases I have obtained from the parents of the patient a free confession as regards themselves, and a distinct history of specific symptoms in the child during infancy.

"3. That in almost all cases the subjects of it present a *very peculiar physiognomy*, of which a coarse, flabby skin, pits and scars on the face and forehead, cicatrices of old fissures at the angles of the mouth, a sunken bridge to the nose, and a set of permanent teeth peculiar for their smallness, bad colour, and the *vertically notched edges of the central upper incisors*, are the most striking characters.

"4. That in many cases one or more of the following suspicious forms of disease have either been coincident with it or have occurred previously: ulcerative lupus, nodes on the long bones, psoriasis on the face, otorrhœa, chronic enlargement, and subsequent atrophy of the tonsils, ulcers in the throat, a thickened condition of the parts under the tongue, and chronic engorgement of the lymphatic glands.

"5. That the effect of specific treatment in mitigating the severity of these inflammations, and in shortening their duration, is sometimes very marked; whilst mere tonic and dietetic plans are of comparatively little avail.

"6. That it is often either accompanied or preceded by iritis.

"7. That it is often followed by certain changes in the choroïd, which are frequently seen in heredito-syphilitic patients." (p. 30.)

The permanent teeth, according to the author, present reliable symptoms of a syphilitic taint, if the patient be of age to show them :

"*The central upper incisors of the second set are the test teeth,* and the surgeon not thoroughly conversant with the various and very common forms of dental malformation will avoid much risk of error if he restrict his attention to this pair. In syphilitic patients these teeth are usually short and narrow, with a broad vertical notch in their edges, and their corners rounded off. Horizontal notches, or furrows, are often seen, but they, as a rule, have nothing to do with syphilis."

Of teeth of this description Mr. Hutchinson unhesitatingly affirms his belief in their pathognomonic character. Other indications of a syphilitic taint are pointed out by Mr. Hutchinson ; but their application must be made, as he very judiciously enforces, with the greatest care, otherwise "mistakes, leading to painful and much-regretted consequences, may ensue from too hasty reliance upon misinterpreted symptoms." Thus among these indications are the condition of the skin, the shape of the nose, and the contour of the forehead; but from the description of these given by Mr. Hutchinson it will be seen that it would not be safe to rely much upon such not uncommon features:

"The skin is almost always thick, pasty, and opaque; it often shows little pits and scars, the relics of a former eruption, and at the angles of the mouth are radiating linear scars running out into the cheeks. The bridge of the nose is almost always broader than usual, and low, often it is remarkably sunk and expanded. The forehead is usually large and protuberant in the regions of the frontal eminences; often there is a well-marked broad depression a little above the eyebrows. The hair is usually dry and thin, and now and then (but only rarely) the nails are broken, and splitting into layers. If the eyes have already suffered, a hazy state of the corneæ, and a peculiar leaden, lustreless condition of the irides, with or without synechiæ, may be expected. If, however, the eyes have not yet been attacked by syphilitic inflammation, they will present no deviation from the state of perfect health and brilliancy. The occurrence of well-characterized interstitial keratitis is now considered by several high authorities as pathognomonic of inherited taint. It is almost invariably coincident with the syphilitic type of teeth; and when these two conditions are found together in the same individual, I should certainly feel that the diagnosis was beyond a doubt." (p. 205.)

In treating of diagnosis, Mr. Hutchinson has not omitted to point out wherein these syphilitic cases differ from, and wherein they resemble what are ordinarily termed strumous affections, more particularly as regards the complexion and the condition of the teeth; the subjects of struma, as the author observes, having usually large, white teeth and a florid complexion. In very few syphilitic cases did suppuration of the glands of the neck occur, and not one was the subject of phthisis. The disease affects by preference the eldest living child of a family—a circumstance to be expected under the syphilitic hypothesis, but wholly inexplicable under that of struma. Nodes, ulceration of the palate, and erosive lupus, affections more closely connected with syphilis than with struma, are also met with in these cases.

The distinction between syphilis and struma was distinctly pointed out by Mr. Erasmus Wilson, in his treatise 'On Syphilis, Constitutional and Hereditary; and on Syphilitic Eruptions, 1852.' In the preface to that work Mr. Wilson says—"What is syphilis in the parent may be scrofula in the child; but the latter is no less a modification of syphilis. The syphilitic eruption on the skin of the parent may be consumption in his offspring. There are, besides, other and more remote diseases which have appeared to me to take their origin in hereditary syphilis." At page 166 of the same work we find also the following explicit statement:—"By a careful observation of the phenomena of syphilis, I have been led to the conclusion that many, if not all, of those affections of the skin which have been denominated scrofula and lupus take their origin in syphilitic poison." Mr. Hutchinson has, in a foot-note to his "Introduction," observed that

Mr. Wilson has asked the question—"Is not struma of syphilitic origin?" But it appears to us scant justice not to have given at the same time the above replies of Mr. Wilson. Inasmuch as correct diagnosis is essential to curative treatment, the science and practice of medicine owes the debt of recognition to him who shall have cleared away the obscurity that has hitherto hung over the large class of ailments loosely and indefinitely named "strumous," and which, for want of a key to their real nature, have so constantly puzzled and defied the practitioner. Mr. Hutchinson ignores the syphilitic origin of the common tubercular forms of lupus (*L. exedens*, and *L. non exedens*). Mr. Wilson has, we think, traced these to the syphilitic taint as closely as Mr. Hutchinson has the diseases of the eye of which he treats.

The following extract from Mr. Hutchinson's work conveys a caution which cannot be too constantly borne in mind in the investigation of the true nature of a case which may offer doubtful points:

"The establishment, or otherwise, of a diagnosis of inherited venereal taint must always be treated as a matter involving great and peculiar responsibility. It is often one of great difficulty, and requiring the cautious use of much special knowledge. In most cases the surgeon is precluded either by moral obligations or by motives of kindness from asking any direct questions, even such as may excite suspicion. If it is the mother of the patient to whom such questions are put, it is very possible that they may be the means of inducing her to suspect that which she had never before dreamed of, and which, true or otherwise, may poison the happiness of her life. There can be no duty more imperative in the exercise of our profession than that of abstaining from needlessly exciting in the minds of our patients suspicions as to conjugal purity. In a general way, there is much less need of caution in seeking information from the father of such a patient than from the mother. Still no one would willingly be guilty of the cruelty of leading a father, however correctly, to attribute the sufferings of his child to his own faults who had not previously suspected the connexion." (p. 203.)

Mr. Hutchinson has drawn up a series of seventy-three "aphorisms and commentaries," in which he has embodied the principal characters of hereditary syphilis, the laws of its transmission from parent to offspring, and the more remote effects thereof. To one aphorism (No. 53) we venture to demur: "The diseases remotely dependent upon inherited syphilis are throughout specific and peculiar. With due care they may *easily be distinguished from all other forms of scrofula.*" We have marked these last words because they seem to us to contradict the preceding sentence, and to be contradicted by the array of cases that the author has detailed, and the elaborate argument he has found requisite in support of his views. The difficulty, not the facility of diagnosis of these affections, constitutes the merit which we think due to Mr. Erasmus Wilson and to Mr. Hutchinson.

Besides iritis and keratitis, the author traces to a syphilitic origin inflammation of the choroïd and retina, cataract, and inflammation of the vitreous body, aquo-capsulitis, amaurosis, deafness, and diseases of the ocular appendages. Each form of disease is illustrated by descriptive cases. In deafness connected with hereditary syphilis, Mr. Hutchinson attributes the defect to disease of the nerves, or of their distribution in the labyrinth, adequate change in the membrane of the tympanum being absent, and the deficiency symmetrical.

In this memoir the author has, we think, achieved the task he has set himself. He has helped to dispel "vague conjecture," and to clear the way for accuracy of diagnosis and precision in the treatment of a class of hitherto obscure diseases. For such a labour successfully carried out no faint thanks are due.

Review VIII.

A Practical Guide to the Study of the Diseases of the Eye: their Medical and Surgical Treatment. By Henry W. Williams, M.D., &c.—*Boston*, 1862. pp. 317.

"In offering this treatise to the profession, and to those who are about to enter it, the author does not assume to set forth all which is known in respect to diseases of the eye. He has endeavoured to supply a want which his relations with junior practitioners and students have shown him to exist, and to prepare a work which shall afford, in a form as simple and concise as possible, a practical and serviceable knowledge of these diseases, and of their treatment, to those whose opportunities for special study have been less extensive than they could have desired." A good idea of this book may be formed, when we say that it very closely resembles in its size, general arrangement, and even style, the well-known and excellent little work by Mr. Dixon. We think such works useful, provided they are employed by the student as introductions to the study of eye diseases, and by the busy practitioner as a means of keeping up with the incessant progress of science. For these purposes they should be written in a clear and simple style, they should treat fully the more important points, whilst at the same time they should neglect, or at all events only enumerate, the rarer forms of disease, and other minor matters; finally, they should always give references to the more important general treatises, and even to some of the many valuable papers scattered through the journals. In such works originality is of no importance, and occasionally even injurious, as the discoverer has almost always a tendency to lay too great weight or to dilate at too great length on the particular portions of the science to which he has himself specially directed his attention. Another and a difficult demand is, that the subject shall be brought up to the existing state of knowledge, and here the writer is always in great risk of either not estimating recent investigations at the value they merit or of accepting mere hypotheses, inaccurate observations, as facts. Difficult then is the task which our author has undertaken, and we may at once state that he has only partially succeeded; still his work contains some interesting matter, which is worthy of further notice. There is an interesting chapter on remedies, and their application, the use of ointments, the danger of employing lotions containing lead, on the nitrate of silver, and so on. Artificial eyes also are treated in a separate chapter. On the treatment of abscess of the lachrymal sac, the author makes the following interesting remarks :

"Formerly, if the application of a leech and warm fomentations did not afford relief, no other resource was believed to be left but to puncture the abscess, and end the patient's sufferings by giving exit to the distending fluids. I have pursued another course with great advantage. If the application of warm fomentations does not soon diminish the sensitiveness, and relax the parts sufficiently to allow of evacuation of the sac by pressure, I introduce a fine probe through the punctum into the sac, which, by bringing the canal into one straight line, generally allows the pus to escape along the side of the probe, or immediately upon its withdrawal, especially if gentle pressure is at the same time made. The discharge of even a small portion of the accumulated matter gives immense relief, by taking off the tension of the parietes of the sac, and the probe may often be inserted a second time, and the evacuation completed with little pain. Where the parts have become so exquisitely tender that the slightest touch cannot be borne, it is desirable to produce insensibility by the administration of ether by inhalation, before proceeding to the introduction of the probe. It is but rarely, however, that ether is required. Should it be impossible to find the opening and introduce the probe, the punctum and canal may be laid open, or the sac itself entered from this direction with a very narrow knife, rather than to allow the abscess to take its own course, or to relieve it by puncture through the skin. Having afforded relief to the urgent symptoms, we may wait for the subsidence of the exalted sensibility of the parts before attempting measures looking to a radical cure." (p. 69.)

In the after treatment of these cases, and in obstructions of the nasal duct generally, he has adopted the plan proposed by Mr. Bowman. On the corneitis arising from hereditary syphilis, he says : "The affection is rare, even in large ophthalmic institutions, and I have seldom met with it ; but my experience confirms the conclusions of Mr. Hutchinson, whose admirable papers on the subject are a valuable addition to ophthalmic literature."

The distinction of iritis as a separate disease at the beginning of this century had an important influence in advancing ophthalmic medicine ; and for many years the treatment of that affection with mercury has been considered as one of the most decisive proofs of the efficacy of that medicine in causing the absorption of inflammatory products. Venereal ophthalmia had been roughly described at an earlier period, and in accordance with the opinions of the time the internal administration of mercury was considered necessary for its cure ; in other cases where no such origin could be assigned, it appears to have been little employed. In this country Dr. Farre* was probably the first to systematically employ and recommend this mineral as a means of arresting simple non-syphilitic inflammation of the iris and deep-seated textures of the eye, and of removing some of the morbid products. Other means, such as turpentine or bark, have been since recommended as occasional substitutes or auxiliaries, yet they appear to have met with no general favour. A sceptical view

* London Medical Gazette, new series, 1839, vol. ii. p. 496. J. C. Saunders : Treatise on some Practical Points relating to the Eye, second edition. London, 1816.

in respect to mercury seems, however, to have gained ground in recent times; even in 1847,* Hasner wrote on the treatment of iritis, to the effect that—

"Mercury, iodine, oleum terebinthinæ, polygala senega, are recommended in iritis, without any definite reason. There is, however, one means which, as it dilates the pupil, must act specifically on the iris. Its employment should never be neglected; belladonna, rubbed into the circumference of the orbit, prevents the chief danger in iritis, that of closure of the pupil. I have also once succeeded by an emetic, in freeing the pupillary margin from the capsule to which it adhered by a recent exudation. If, besides, the eye is protected from the light, and diet with a mild antiphlogistic treatment is employed, there will be but little danger, and the inflammation will generally soon diminish. On the other hand, in inveterate cases a whole army of medicaments, mercury, iodine, &c., will be of no avail. I cannot recommend the turpentine, which has been praised by Professor Flarer, and which is in good repute in England. I have tortured many cases with it (for its unpleasant taste cannot be prevented by any corrigent), but have found no result."

Another means that has been long employed is opium—generally, however, only in small doses, merely with the view of promoting the action of the mercury. Recently, Mr. J. Z. Laurence† has published a paper on "The Antiphlogistic Powers of Morphia, illustrated by its use in the treatment of Acute Inflammations of the Sclerotic and Iris," in which he endeavours to show that it does more in these cases than simply alleviate the pain—that it really has a distinct influence in relieving the inflammation. So far as we know, the only one of the early writers who assigned any special value to opium in ophthalmia was Sydenham; he says: "Observe—ophthalmia may often resist both bleeding and purging, however often repeated. In this case, give a paregoric of an ounce of syrup of poppies each night. This will effect a cure single-handed."‡ In recent times, however, we find that Mackenzie, Ruete, and others, have only mentioned it as of some value in alleviating the severe neuralgic pains so often present in iritis.

Dr. Williams§ also dissents from the common practice. He says:

"I believe this disease to belong to the class known as self-limited, having a tendency to recovery in about three weeks, provided the morbid processes do not reach the point of producing extensive adhesions of the pupil, but having also a tendency to produce serious organic alterations, and to have a longer duration, where these processes are excessively violent and are permitted to go on unchecked. If this opinion be well founded, one important point in treatment has a high precedence above all others—the maintenance of dilatation of the pupil. In fact, without undervaluing other remedies, indicated at times by the general condition, I am confident that atropia and opium—the former to enlarge the pupil, the latter to control the pain attending the disease—are the essentials in the management of this affection. It follows that I attach little

* Dr. Jos. Hasner: Entwurf einer anatomischen Begründung der Augenkrankheiten, S. 140. Prag, 1847.

† Medical Times and Gazette, 1859, vol. ii. p. 651.

‡ Works of Thomas Sydenham, translated, &c., vol. ii. p. 261. London, 1850.

§ We believe we are correct in stating that Dr. Williams published, some six years ago, a paper in the Boston Journal, in which he advocated the same treatment with opium which he now employs.

value to mercury in the treatment of this affection, though it has formerly been regarded as almost a specific, and has been often cited as an instance of the power of remedies in arresting morbid processes. . . . . Observation of many cases which came under my notice where the patient had been treated with mercury to the fullest extent, but nothing had been done to dilate the pupil, and where the result was most disastrous; and reflection on the mode of action of the constitutional and local remedies, induced me to vary from the treatment I had always previously pursued according to the approved method, in certain cases which came under my care, where the patients were in such a condition as contra-indicated the use of mercury unless as an imperative necessity. These were treated by anodynes and tonics, and the pupil kept fully under the influence of belladonna, and they terminated far more favourably than the severity of the attack and the state of the patient had allowed me to expect. Encouraged by excellent results in these cases, I was led to try a similar plan in other instances; and I am confident that I never saw better results ensue, in the hands of others or myself, from the old method. If it be in our power, then, to spare the patient the infliction of the grave inconveniences and protracted convalescence often occasioned by the free use of mercury, and to relieve him as well and as quickly by other remedies, we may gladly accept the alternative." (pp. 126, 128.)

Very large amounts of opium or morphia are sometimes required to subdue the agonizing pain and procure sleep. A dose, judged to be a suitable one, should be prescribed, and if requisite, repeated hourly, till the patient is relieved.

We ourselves are inclined to believe that a somewhat intermediate course between these extreme views, and especially a careful consideration of particular cases, will be the plan most conducive to success in practice. In this respect we strongly recommend our readers to peruse what Graefe* has written on this subject: it is, we consider, certain that the great point is to keep the pupil fully dilated; all other treatment should be subsidiary to this.

An interesting case is mentioned of spontaneous dislocation of the lens into the anterior chamber, which receded after the patient had been lying on the back at night On one occasion, the presence of the lens in the chamber caused so much irritation that the pupil contracted strongly, and the lens could not recede till atropia had been employed to dilate it.

The latter parts of this work on diseases of the fundus oculi, on amaurosis, &c. &c., are not nearly so good as the earlier chapters; we suspect the author has not yet acquired much familiarity with the ophthalmoscope. We have now, we think, pointed out all that is of any practical interest, and may conclude by saying that although this is evidently the production of an intelligent and experienced physician, it is yet, as an introduction, scarcely equal, perhaps in some respects inferior, to works already before the medical public. In two respects it is, however, very commendable—the author's style is clear and pleasing, the printing excellent.

* Three Memoirs on Iridectomy, &c., published by New Sydenham Society, pp. 251-257. London, 1859.

Review IX.

An Introduction to Mental Philosophy on the Inductive Method. By J. D. Morell, A.M., LL.D.—*London*, 1862, pp. 464.

It is a fortunate and auspicious circumstance when a subject of so much importance and interest as that of mental philosophy engages the attention of one so competent to do it justice and encounter its difficulties as is the author of the work before us; and doubly fortunate when the task he is engaged in is a labour of love, the study of it a delight, and the aim the benefit of his fellow-men, both as conducive to a knowledge of self, the *nosce te ipsum*, and of human nature, and its improvement by means of education, that in principle resting on psychology.

From the preface we learn how wide is the foundation on which Dr. Morell has laboured. He quotes the authors from whom he has derived assistance; these, the latest and best, English, German, and French. The main peculiarity of his work is its construction on the inductive plan, after the manner of the natural sciences, not the natural history method, but associating it with physiology, which hypothetically viewed may be considered the correlative of psychology. He informs us that he has always kept before his eyes a wholesome horror of creating an overgrown and unwieldy book, studying brevity and compression as much as possible, laying the chief stress on the great points to be kept in view, and leaving the minor results and applications for the present very much in abeyance. For this we are thankful, especially as he has not sacrificed clearness to brevity. And we have to express our thanks for another attention—the avoiding, as much as possible, of new terms, two only being introduced by him: these, *residua* and *ideation;* the first, one of great significance, borrowed from the Herbartian psychology, the second, sparingly used, first coined by Mr. James Mill. Another great recommendation is that his philosophy has not parted company from common-sense, but, on the contrary, has reconciled them—at least so we think—showing how there is a certain truth in the views of Hume, Berkeley, and Reid, what is the significance of the great war-words of the metaphysicians, matter and mind, time and space, the absolute, the infinite, what are the kinds of human knowledge and what are its boundaries.

In the Introduction, the author in the first chapter lays down the plan of his undertaking—that of investigating the great problem of "man as a living, instinctive, active, feeling, and thinking being." The guiding rules which he has selected being of almost universal application in scientific research, we do not hesitate to quote:

"1. Every *real* science (as opposed to those which are purely *abstract* and formal—such as logic and mathematics) must be based upon *facts*, which in some sense or other lie open to actual observation.

"2. Our induction of facts within the range of the science of which we are treating, must be as *large* and as *varied* as possible.

"3. As the phenomena which lie open immediately to our observation are usually the results of a number of simpler agencies, they should be carefully *analyzed*, for the purpose of discovering, as far as possible, the more elementary facts of which they are constituted.

"4. Hypotheses may be properly employed, while investigation is going on, as necessary tentative efforts to grasp the general law to which the facts conform; but we must ever be ready either to transform or abandon such hypotheses, according as the subsequent teaching of the facts may require.

"5. The natural course of all science is a gradual progression from one degree of generality to another, the less general result being included in the more general, until we arrive at what are termed 'The Universal Laws of Nature.'

"6. The different sciences are closely connected and affiliated, so that what is established in one becomes afterwards of essential consequence as data for the investigation of another. No real science, therefore, can stand simply on its own facts, isolated from all the other results of scientific research.

"7. The principal instruments by which we are enabled to analyze phenomena and discover the simpler and more universal agencies by which they are upheld is *experiment*.

"8. And lastly, where the possibility of experiments is precluded by the nature of the case we must generally be guided by *analogy*."

In the second introductory chapter he treats of the Facts of Psychology. After reviewing them, and describing their several kinds, he concludes with the remark which we think he is well justified in making, that—

"Taking into consideration all the sources of mental observation which lie before us, we shall hardly be inclined to complain of the paucity of facts to which we can appeal;" adding: "Our greatest embarrassment arises from their multiplicity, which is so great and varied that we can only hope as yet to make a commencement in the work of reducing them to scientific order and subjecting them to a sound and exhaustive analysis."

Following the synthetic method, as best adapted for his purposes, he begins with the "Fundamental Distinctions of Vital Phenomena"—this the title of the chapter following the introduction. He holds that vital force, nerve force, and mind force are all correlated, vital force taking the lead, from whence, as from a germ, all the mental phenomena are evolved. The first fact characteristic of life is, he insists, individualization; in unorganized masses, there being no unity in which each part is subservient to the whole, every animal existence standing forth as something distinct and separate from the world of nature around, and possessing a living force of its own, to merge again into the elements of nature on the death of the individual.

The next characteristic that he lays down is the dependence of the living being upon the physical forces by which he is surrounded, thus differing essentially from things without life—life being like a lamp that is perpetually burning, and needing therefore perpetual sustentation.

A third great fact of organized existence is that of growth, every living thing having its beginning in a germ—a single cell—bearing not the slightest likeness to the perfect animal into which it is to be developed. We quote what follows as a good example of the author's logical method and power of generalization:

"Now, let us see if we can *combine* these distinctive attributes of vitality into *one* idea, so as to bring to light some general fact or law which may be applicable to the *whole* sphere of organic existence. The first attribute we mentioned was individualization. This is, in other words, the power of *self-maintenance,* the capacity of *resisting* and *repelling* all which would otherwise tend to disintegrate the organism, and reduce it to the common elements of nature. The second attribute was the dependency of all organic and living existences upon the physical forces around them. This is, in other words, the power of attraction and assimilation, the capacity of *selecting* what is conducive to life and well-being from nature, then of appropriating and incorporating it; and lastly, by this means, of making it part of our own individuality. The third attribute was that of *growth,* which is a process in which both the above-mentioned powers are combined, for in growth we see the development of the individual, as an individual, carried on by means of nutrition drawn from without, continued life resulting from a balance, as it were, of these two forces.

"We find, accordingly, that there are two great facts or laws pervading the whole sum of organized and conscious existence, the law of attraction and the law of repulsion, the law of *assimilation* and the law of *separation.* We see this in the plant. The elements of nature perpetually act upon it, and would soon absorb all its sap if left to the natural operation of the physical forces. But the *vital* principle *reacts,* and converts those very physical forces, which would otherwise consume it, into nutriment and health. The case is the same with animal existence. Here, in connexion with the vital force, we have the power of assimilation on the one side, drawing from external nature everything that is necessary for support, for growth, and for continued existence, and the powers of repulsion, of secretion, and of excretion on the other, which avoid, throw off, and eject everything prejudicial to life, everything, therefore, which would interfere with the maintenance of the being's individuality, or cause it to return to the unorganized elements of nature.

"Rising from the vital to the nerve-force, we find here also the same twofold law in operation, for all nervous processes are carried on by a double power of action and reaction. Every nerve of special sensation has the property of assimilating and propagating certain impulses from without, and then of exciting a reactive force, which expends itself in motion communicated and repulsion effected in reference to the world without.

"And then, lastly, we find the same law in another form, pervading all the operations of the mind-force, from the lower instincts up to the highest exercise of reason. For what is instinct but the power of adaptation to external circumstances—i.e., of selecting what is conducive to well-being, and repelling all that is noxious to it? And what is *reasoning* but the power of separating and distinguishing, as a necessary preliminary to the assimilation and complete appropriation of *truth?* To point out all the different forms, however, in which this twofold law works throughout the whole economy of living nature would be to anticipate much of our succeeding analysis. We must content ourselves at present with having indicated it as the most general universal fact of *life,* whether physical or mental."

In the second chapter he opposes the dualistic theory that mind with its functions is one thing, the body with its functions another; holding that in the primary cell from which the individual man is developed, there is the nascent spark, the active principle, the vital force, the soul, which, acting unconsciously, adds cell to cell, shapes the tissues into organs and limbs, and adapts the body to perform the functions of life, constructs the wondrous network of the nervous system, and gives it power to vibrate to the influences of the world without.

In his third chapter, on "Precocious Mental Activity," he brings forward facts all tending to prove that the human mind is not a *tabula rasa* upon which experience has to write all the characters; but that every individual has its own distinctive type, bringing with him into the world mental tendencies and characteristics derived from parents and ancestors, with vital substrata which operate prior to consciousness. The conclusions he arrives at are:

"1st. That the vital forces and the mind forces are one and the same at their root; 2ndly. That all our conscious life rests upon the basis of unconscious life, out of which it grows; and, 3rdly. That there is such a correspondence between vital and mental activity, that the laws of one will help us to throw some light on the other."

In the fourth chapter the author advances a step farther, to "Primordial Activity accompanied with Consciousness," having its source in the cranio-spinal system, under two distinct factors,—external impulses acting through the organism, and a nervous centre receiving the impulses and producing consciousness initiating reaction. He illustrates this by means of the sense of smell. He says:

"An intensely strong and pungent scent suddenly affecting the nerves will produce acute pain. The *action* being so great in relation to the power of *reaction*, no other than a painful result can follow. A moderately strong scent, on the contrary, one which can just fill and satisfy the power of reaction, will often produce the most lively pleasure. But if, thirdly, the effect upon the nerve is very slight, so as not to satisfy the power of the organ, then *desire* is awakened and maintained till satisfaction ensues."

Instinct he defines as purely sensory—motor actions flowing *spontaneously* from the constitution of the nervous system and the unconscious law of intelligence which guides and impels it. He says:

"If we experience desire—*desire* as a consequence of that peculiar combination of the two factors which we have above indicated, an instinct is awakened which prompts us to seek for satisfaction and to perform those particular actions which are most likely to lead to it. If we experience *pleasure*, then we have an instinct aroused which leads us to grasp and keep inviolate the means of perpetuating it. If we experience *pain*, then our equally strong instinct is aroused to relieve and avoid it."

"Instinct," he further remarks, "is reason; but reason in its undeveloped, semi-unconscious and wholly involuntary form;" and that man is as instinctive a being as any of the lower animals, a very large portion of his life and activity—reflex actions emanating from the sensory ganglia—being always instinctive to the end.

"The reason" (he adds) "why we notice the instincts in man less than in the animals is because our volitional intelligence comes gradually to play so much more prominent a part in the whole process of human existence." And, "as the formation of the simple cell in the structure of the animal economy is *in kind* the very same effort of plastic power, which, by repetition and accumulation, frames all the organs of the body, so" (he holds) "are the first instincts which the mind developes under the stimuli of the outer world, the primary movements of an intellectual power, or *mind-force*, which, by a similar process of steady development, constructs all the faculties of our mental constitution."

Concluding this chapter, he says:

"We have now the problem of psychology fairly before us. We know the first elements of our mental constitution and the primary laws by which mental development is carried forward. Our task will be to show how out of these elements and by the action of these laws the whole of our faculties are successively constructed."

In the fifth chapter, supplementary to the preceding, he considers the doctrine of individuality. Four hypotheses he criticises, these, at the present time, widely current throughout Europe in different schools of philosophy. 1. The materialistic, in which all the facts of psychology are viewed as being simple functions of certain forms of organized matter. This he objects to, irrespective of other reasons, that no one knows what matter is; matter admitting, perhaps, of being reduced to *force,* and force to *spirit:* that the material forces from which mind is supposed to emanate are, as far as all our experience goes, uniform and constant in their operation, while in every single mind we have a separate and distinct individuality.

2. The hypothesis that mind is a special manifestation of *the Absolute Thought.* This he objects to, individuality being its disturbing element.

"Thought, it is said, is absolute; if absolute, impersonal, and having no special relation to the individual. But what is the fact? Everything tends to show that there is a real and not a sham individuality, and that the thought immanent in me and you and every one is far from being a mere wave in the ocean of infinite reason, welling up for a time and then sinking down to be for ever lost again in the ocean of the Absolute."

3. The ordinary dualistic hypothesis that mind and body are distinct existences with a temporary and partial connexion, but still carrying on their respective functions independently of each other. To this he opposes the difficulties of the correlation existing between the vital, the nervous, and the mental forces; he holding that all the facts brought forward in the preceding chapters refute it.

4. The hypothesis of Individualism, that man is made up of two elements, material and spiritual, which completely interpenetrate each other; body and mind standing to each other in the relation of *matter* and *form,* their union constituting the individual—the *personality,* every individual taking his place in the whole plan of creation as an independent unit, having a real essential existence of his own; and this from the first cell-germ to man in his complete maturity. In this conception, the author is of opinion that all the facts of the case are summed up in an intelligent manner, affording a scientific impression of the practical common sense of mankind. And he adopts it, he says, not that he "would put it on a level with the facts and laws of mind, of which we can assure ourselves with perfect accuracy," but as helping to hold the phenomena together, and "as a theory which may be modified or perfected indefinitely by the course of future investigation."

Of the following chapters our notices must be more concise and partial; yet in continuing our analysis we shall indulge ourselves in making, as we proceed, some extracts on points which are specially interesting and important; and for the sake of brevity, as heretofore, we shall give them without comment, our desire being to make our readers acquainted with the nature of Dr. Morell's very original and able work, and not to criticise it; indeed, a critical examination of it could hardly occupy less space than the work itself.

The second part—on the "Nature and Development of Perception"—commences with the consideration of sensation, properly so called. This the author holds to be an ultimate and indecomposable fact. He describes it as excited by stimuli *ab extra* acting on the nervous system, limited to the spinal cord, producing motion without consciousness, and when extended to the sensory ganglia at the base of the brain awakening consciousness in connexion with reaction—*motion* of some kind being the universal stimulus, the general link of the world with the senses.

Perception next is examined. He defines it to be not altogether subjective, as is sensation, but something more, mental action being superadded, constituting not a special and peculiar faculty, but "the entire activity of the mind as employed in interpreting the primary intimations of the senses." He holds that all perceptions are really *acquired*, and that their series in accumulation constitute, as it were, the cellular tissue of the mind, gradually consolidating into the higher forms of perceptive power to the exclusion of innate ideas. From the genesis he proceeds to the "Indestructibility" of our perceptions. His conclusion is, not that a perception made continues to exist in the mind, except as it were tacitly, admitting of "being brought back into consciousness by any sufficiently active suggestion," from the fact of its perpetuity following the analogy of everything that we see in the material universe, no particle even of physical force being ever lost.

This view leads to the consideration of the nature of *Residues*, which, as the name implies, are somethings that remain, traces left on the substance of the nerves, capable of being revived, and which, according to the frequency of the impressions made, are proportionally increasing in strength. To this doctrine of residue the author attaches great importance, especially in the way of education. And hardly less to his next subject, "*the Law of Similarity*," by virtue of which, "*identical and similar residues blend together, so that one single mental image is formed out of the whole*, after the manner of morphological cell-formation—this productive of organs, that of definite habits and mental activity.

Under the head of "Simple Perceptions," he treats of the senses, including the sense which is first in order of time, and is most elementary—"the general sense of bodily existence, cœnæsthesis, that indefinable consciousness which, with colour, sound, pressure, heat, scent, taste, constitute the groundwork on which the whole immense fabric of our perceptive life is built."

His general conclusion as to the perceptive faculty is:

"That it does not involve any peculiar mental operation essentially different from all others, but is simply the mind working according to its universal laws in this particular sphere of its intellectual development. Whatever is contained in thought, of however advanced a character, is contained *germinally* in perception. Perception involves in its unexpanded form all the elements of logical thinking; and the powers of comparison and separation, of seeing similarities, and judging differences (in which we shall see all logic consists), is here already at work, forming the mental law which underlies all our intellectual operations, alike in their lowest and highest sphere of action."

The third part is on the "Nature and Development of Ideas." The difference between a perception and an idea the author defines to be this: "that in the one case the actual object on which the mind is occupied is *present* to us, while in the latter case it is *absent*; residence, from the first elementary perceptions up to the present moment, entering into the process of *ideation;* accordingly, he holds the origin of ideas to be simply that they are neither born within us nor impressed upon us from without, but are the product of the mind's free activity operating in connexion with the world around, and that they act and react, blend together, and are associated. When an idea becomes so fixed in the consciousness as to be proof against all counteracting influences, the result he points out is *insanity*, and that *fixed ideas* are the most frequent symptoms of incipient monomania. He instances the blending of ideas in the manner that the highest religious ideas in Catholic countries are blended together in the service of the mass. As to the association of ideas, the general conclusion he arrives at is:

"1. That *frequency of connexion* between any two events in nature tends to strengthen the mental association between them, and thus to give rise to the feeling that future occurrences in the same order are probable. 2. That *uniformity of connexion*, in the same way, tends to create an irresistible association between the ideas, and thus gives rise to the feeling of *certainty* in relation to their future sequence in the same order."

Language, the exponent of ideas, he holds to be "not a thing preconcerted and completed, but *a power* which is always in the course of active development—not an ἔργον but an ἐνεργεία," and originating, as he thinks, in involuntary vocal expression connected with reflex action, the *interjection* being its genesis.

Memory he treats of under the head of "Reproduction of Ideas," not admitting it to be a distinct faculty, but based upon the universal fact of the persistency of our mental impressions, and as a volitional act taking its start from the spontaneous classification of ideas. It is involved in language, without which we could exercise no direct control over ideas.

The concluding chapter of the third part is on the "Understanding and Imagination." Neither do these terms, according to our author, designate any separate and peculiar faculties. He accepts the ordinary meaning of the words—understanding, as the power of *comparing, distinguishing, judging*, between two or more things; imagination, as the power of *creating* and retaining the images of things in the mind, and of bringing them vividly into consciousness, and combining them into new forms. To understand an object, he says, "is to know its connexions in nature, and to see it in combination with everything else of a cognate character," after the manner of the man of science; whilst to imagine "is to clothe the bare skeleton of human thought with all the embellishments of external dress and minute expression," after the manner of the poet.

The fourth part, on the "Logical Processes of the Mind," we shall pass over, quoting merely the remark with which he introduces it—that these processes do not "involve any distinct and peculiar faculty fundamentally different from those outward activities which have been already analysed and explained;" but with this peculiarity, "that as we ascend further up the scale of mental development, the processes become more *explicit;* i.e., instead of being involved in the spontaneous effort of the intellectual instinct, the elements of which they consist are drawn out into a series of distinct volitional acts of intelligence."

The three concluding parts—those "On the Human Reason," "The Development of the Will," and "On the Feelings"—are not the least interesting portions of the work; to the majority of readers they will probably prove most attractive, and to those not far advanced in psychological studies most instructive.

Human *reason* he defines as simply exhibiting the great law of intelligence in its highest intensity, forming the "*truth-organ* of the soul," truth being but a just apprehension of the relation of things in the universe to which we belong; and he holds as essential to its exercise the co-ordination of all the intellectual processes, a power lost in those who have lost their reason—the insane.

In investigating the psychology of human reason, he first treats of knowledge, of which, in some cases, truth is said to be the reflex; next faith, next opinion.

The results of his analysis he thus sums up:

"By reason we mean the power of co-ordinating all the other intellectual processes so as to give rise to *human convictions*, and to enable us to adapt ourselves to the universe in which we live. Of these convictions the first and most important are those which rest upon indubitable *objective grounds*, and which, therefore, we term *knowledge* 'par excellence.' Those convictions which rest upon universal consent, but which can produce no objective proofs, we term *natural beliefs;* while, lastly, those which rest on the subjective impulses and promptings of our individual nature are to be considered only in the light of *personal convictions*. Evidence of the first kind is in every way irresistible, any opposite convictions being either impossible or absurd; that of the second kind is open to speculative doubts, but is always practically unquestioned; that of the third kind may carry any amount of force to the individual himself, but can never be rendered valid to any other mind differently constituted to its own."

Of the first kind he says:

"Putting aside the ordinary and passing facts which the senses bring home to us—i.e., the particular knowledge of daily life—the truths of a general character which have been established on the evidence implied by personal knowledge do not reach far. They are limited to a few of the results which the most perfect of the sciences have elicited; and, even in the case of these, they do not go down to the ultimate ideas on which all such sciences rest."

Under the second he ranks what lies beyond the region of logical analysis, and as having no indisputable objective grounds. He remarks:

"We cannot be said to *know* anything of time, or space, or matter, or force; we cannot point to any objective necessity which compels us to admit their real existence . . . . yet we believe in their real existence."

Further, this kind of belief comprises the very many convictions of which our knowledge is imperfect; for instance, according to him, a theory compared with a law of nature. Of the third kind, he instances *religious faith, that,* he says, lying out of the region of *knowledge*, in the strict sense of the term. He adds:

"The very term 'faith' implies this. The chief object of religious faith is a Supreme Being—the great Cause and Creator of all things. The infinite, however, as we have shown, cannot be grasped by the finite as an element of *knowledge* any more than the objects indicated by all other ultimate intellectual ideas. Thus, we do not *know* the real objective existence of space, or matter, or force; and for the same reason, and owing to the same limitation of our faculties, we do not possess any positive *knowledge* of an infinite cause."

Dr. Morell's extended remarks on this most important subject of belief, especially as connected with religion, are deserving of a careful perusal and study, and we regret that our restricted limits do not allow of our doing more than thus briefly adverting to them. They are well adapted to teach humility, the opposite of arrogance and dogmatism; and toleration, the opposite of religious persecution.

The will, he does not of course consider as anything distinct from the mind in its operations. He says: "It is merely the mind itself viewed in relation to *effort* and *action*, instead of intelligence and reason."

His summary of the analysis of human reason we had marked for quotation, but we are checked by its length; we restrict ourselves to the last portion of it:

"5. Lastly, we say that an action is *free* when it is prearranged by an intelligent purpose, and its execution can be either suspended or carried out according to our personal determination. Freedom does not, therefore, consist in acting without motives, but in the power we possess of modifying our motives, and either elevating or depressing the moral plane of our voluntary activity. Thus we have a foundation on which human responsibility and practical morality can alike be securely based."

"The feelings," constituting the last part of the work, he introduces with some remarks on the obscurity of the subject as a branch of psychology, and how, from the time of Aristotle to the present, it has been viewed under different aspects, as to their origin, the elements of which they consist, and the relation they bear to the understanding and to the will. After noticing the opinions of the many inquirers who have engaged in the discussion, the first conclusion he arrives at is of a negative kind—viz., that the feelings constitute a special class of mental phenomena which cannot be accounted for by regarding them either as any modification of an idea, or as any form of mere sensorial pleasure or pain, or any combination of these two elements into one. The positive results of his inquiry he enunciates as follow:

"1. The emotions are fundamentally different from ideas, and cannot be brought under the same definition.

"2. But still they are so far related to ideas that, if we had no ideas we could have no emotions, in the higher sense of the word.

"3. Emotion depends on the *tension* of our ideas—i.e., on the special mode in which the residua affect each other, and pass in and out of consciousness.

"4. The *material* of our ideas does not *necessarily* enter into the process by which our emotions are originated, although there *may be* emotions which only originate in connexion with ideas of a certain class.

"5. The tension in our ideas is ordinarily accompanied with pleasure or pain.

"6. Pleasure arises when the vital energies are brought into full and adequate play; pain, when they are either checked or over-stimulated.

"7. In the case of emotions, properly so-called, the ideas from which they spring give birth to pleasurable or painful feeling according as they are seen to affect our *interests*, personal or relative.

"8. Each kind of emotion, besides being ordinarily pleasurable or painful, possesses also a *speciality* of its own, which arises from the peculiar modification which it indicates of our common sensibility.

"9. The emotions are the intermediate agencies through which the intellect acts upon the will; and thus it is that they mainly govern our practical life."

The classifications of the feelings, he remarks, have been as many as the speculations on their nature. He proposes the following, which, he thinks, is as complete as our present knowledge of psychological processes admits of:

"I. *Feelings which depend solely on the flow of our ideas through consciousness.*

"1. *Those dependent on bodily causes; as* health, vigour, high spirits, on the one side; or weakness, languor, low spirits, on the other.

"2. *Those dependent on mental causes; as* expectation, satisfaction, entertainment, on the one side; or disappointment, ennui, doubt, impatience, weariness of mind, on the other.

"II. *Feelings which stand in connexion with the nature and material of ideas themselves.*

"A. Those which stand in connexion with the contemplation of natural phenomena, or æsthetic feelings.

"B. Those which stand in connexion with the contemplation of our fellow-men, or sympathetic feelings.

"C. Those which stand in connexion with the human action, or moral feelings.

"D. Those which stand in connexion with truth and destiny, or religious feelings."

Dr. Morell's remarks on these extend over two chapters. All of these are interesting, especially those on the moral and religious feelings—the first in connexion with human conscience, the second in connexion with human faith.

"Every man," he says, "forms his ideas of good and evil from the phenomena around him. He learns gradually to *separate* actions which have any kind of moral element in them from others which have not; and in the same way he comes, by a like gradual process, to divide them into the two classes of right and wrong. And that this is the mode in which our moral ideas are formed is confirmed, he thinks, by the fact that there is no positive standard of morals anywhere to be found. Conscience he holds to be the union in our complex state of moral ideas, moral emotions, and moral activity, as, he says, "the whole of our nature, intellectual, emotional, and volitional, is involved in every act which conscience dictates."

Regarding *faith*, that great essential of religion, he offers the following remarks, which, notwithstanding their length, we shall quote, they are so eloquent, and, as we think, so just :

"IV. The only class of special emotions left are those which accompany the contemplation of human truth and human destiny—I mean the *religious* emotions. We are placed here in the midst of a universe, of which we see and comprehend only an infinitely small portion. The human reason, not satisfied with the knowledge which it is able to acquire, longs to go beyond the region of the known into that of the unknown, and thus to complete by its own subjective efforts what cannot be ascertained on clear objective grounds. In the presence of these great problems, the human mind finds out its own weakness and dependence. Interests of infinite moment start up in connexion with the purpose of life—the destiny to which it tends, the eternal future, and the infinite power on which the whole reposes. The first *feeling*, then, which naturally arises in the human breast from the tension and struggle of these great ideas, is the feeling of *helplessness* and *dependence*. This is the starting-point and foundation of the religious emotions. That which we *know* becomes a part of the whole mass of scientific truth, and is removed at once by this very fact out of the region of religious faith or feeling; that which we do *not* know and *cannot* comprehend, but which, nevertheless, stands closely related to our happiness and our destiny, can still become the object of our *faith;* and, as such, produces the feeling of helplessness and dependence from which religious life, subjectively considered, takes its commencement. As the objects of our faith become clearer to the mind, other feelings mingle up with the first emotion; love, joy, confidence, hope, all unite their influences as the beneficence and goodness of the Deity are more and more realized in the world without and the soul within; and thus that complex state of feeling involving humility, awe, veneration, love, gratitude, joy, in the presence of the Infinite and Eternal, is gradually evolved which we term *religious feeling*.

"If the ideas we form of the Infinite, and our relations to it are dark, gloomy, and oppressive, the feelings take a similar hue, and religious gloom, melancholy, and even despair, may possess the mind when crushed under the sense of its present dependence and the darkness which the future presents."

In the author's concluding chapter, he treats briefly of the desires and passions, these cognate with volition and feeling; the one originating in the motor system, the other in the sensory. Desire he defines to be a complex state, made up partly of feeling and partly of volition, the desire of a thing being accompanied by a feeling of its worth. Analysing it, he states :

"First, there is a feeling of pleasure arising from some elevation of the common sensibility, or of our mental forces; secondly, this pleasure is associated with the object (whatever that may be) which produces it; thirdly, the absence of this pleasure, when expected, leads us to *desire* the obiect with which it is associated, and to which we look for its gratification; and lastly, by the force of repetition, this desire grows up in extreme cases into an habitual tendency, which influences the entire character."

The passions he defines as "simply intensified and permanent desires—desires which more or less decidedly control the reason and the will ; some of which, like the feelings, are indefinite and subjective, others which are attached to specific and assignable objects, constituting two classes, the subjective and the objective. He ends his comments on them with the offering of an excellent practical rule for their government, expressed in the motto "obsta principiis," adding :

"While the desire is moderate, and the tendency undeveloped by long repetition, reason, and conscience, and volition can perform their part; but once let the *passion* become dominant, and the reason will be warped, the conscience seared, and the will led captive by its power."

Appropriately, the last chapter of the work is on "Human Character." In the world of mind he justly remarks, contrary to what is observed in the laws of nature, sameness is a phenomenon wholly unknown, there being something specifically different in every human being, and hence the primary foundation of the variety of human character. Yet, as he very properly enforces, more depends, as regards the nature, the quality of character, on adventitious circumstances which influence the mental growth, and are brought into action in a well-conducted education; *education*, in the widest sense of the word, being, as he holds, the great regenerator of human society; as "to it we must owe the intellectual habits we form, the power which the reason and the conscience have over the will, and strength we possess to regulate the desires and subdue the passions."

Our extended notice of this work we would wish to have considered as a proof of our estimation of it, and that we indulge in the hope and belief of its author, as expressed in the words with which the volume concludes, that "a more deep and practical psychology must contribute [to education], as it alone can expound the theoretical laws and principles on which all true human education must proceed."

Review X.

1. *Handbuch der Systematischen Anatomie des Menschen.* Band ii. Lieferung 1, *Eingeweidelehre: Haut, Verdauungs und Respirations Apparat.* Von Dr. J. Henle.—*Braunschweig*, 1862.

Handbook of the Systematic Anatomy of Man. Vol. ii. Part 1. *Viscera: including the Skin, Digestive and Respiratory Apparatus.* By Dr. J. Henle.—*Brunswick*, 1862.

2. *Lehrbuch der Anatomie und Physiologie der Sinnes Organe.* Von Prof. D. Adolphe Fick, Zurich. Heft 1.—*Lahr*, 1862.

Treatise on the Anatomy and Physiology of the Organs of Sense. By Prof. D. Adolphe Fick, of Zurich. Part 1, 1862.

3. *Die Unmerkliche Wasserverdunstung der Menschlichen Haut.* Von Dr. Victor Weyrich, Prof. der Medecin zu Dorpat.—*Leipzig*, 1862. pp. 250.

On the Insensible Perspiration of the Human Skin. By Dr. Victor Weyrich, Professor of Medicine at Dorpat.—*Leipzig*, 1862. pp. 250.

4. *Lehrbuch der Chemie.* In drei Banden. Band iii., *Physiologische Chemie.* Von Dr. E. F. von Gorup-Besanez, Prof. der Chemie an der Universität zu Erlangen.—*Braunschweig*, 1862.

Physiological Chemistry. By Dr. E. F. von Gorup-Besanez, Prof. of Chemistry at the University of Erlangen. In three volumes. Vol. III.—*Brunswick*, 1862. Art. *Sweat.*

5. *Zur Anatomie und Physiologie der Haut.* Von Prof. K. LANGER. —*Wien*, 1861.

On the Anatomy and Physiology of the Skin. By Prof. K. LANGER.—*Vienna*, 1861.

6. *Das Saugader System vom Anatomischen Standpunkte.* Von Dr. LUDWIG TEICHMANN. Mit 18 Kupfer tafeln.—*Leipzig*, 1861.

The Lymphatic System from an Anatomical Point of View. By Dr. LUDWIG TEICHMANN. With 18 Copper Plates.—*Leipsic*, 1861.

7. *Die Lymphgefässe und ihre Beziehung zum Bindegewebe.* Von Dr. F. von RECKLINGHAUSEN. Mit 6 Lithographischen Tafeln und 7 Abbildungen in Holzschnitt.—*Berlin*, 1862. pp. 98.

The Lymphatic Vascular System in its Relations to Connective Tissue. By Dr. F. von RECKLINGHAUSEN. With 6 Plates drawn on Stone, and 7 Woodcuts.

8. *Lehrbuch der Physiologie für Akademische vorlesungen und zum Selbststudium.* Von Dr. OTTO FUNKE, Professor der Physiologie an der Universität Freiburg. Vierte umgearbritete Auflage. Band I.—*Leipzig*, 1863.

Text-Book of Physiology for Lectures and for Students. By Dr. OTTO FUNKE, Professor of Physiology in the University of Freiburg. Fourth Edition, thoroughly revised. Vol. I.—*Leipsic*, 1861.

IN the following pages we propose to give a short sketch of the chief works that have recently been published on the structure and functions of the skin. Of those placed at the head of this article, it may be observed that a very complete account of the structure of the skin is contained in Henle's general anatomy, and also in Fick's excellent treatise, which, however, deals chiefly with the skin as an organ of touch. Weyrich's book, like so many other German works, is a monument of patient and persevering observation, and is also an attempt to introduce a new method for the investigation of the secretory functions of the skin, which may hereafter, perhaps, be made available in the discrimination of disease. Hitherto the various forms of cutaneous affections have been classified in obedience to certain views of their anatomical character. How unsatisfactory this arrangement is to those skilled in this department of surgery may be gathered from the following statement, made so lately as May in the present year by Dr. McCall Anderson, the Physician to the Glasgow Dispensary for Skin Diseases,* in the course of a lecture upon eczema: "Defective as any classification of skin diseases must be, there can be little doubt that the anatomical classification is the most objectionable of all, for in this way are many dissimilar diseases brought together in one group, and violence is done to the symptomatology of many of them, owing to the necessity of placing them under one of the elementary lesions."

And again: "There can be no doubt in my mind that the best classification of skin diseases is one founded, not upon the elementary

* Medical Times and Gazette, May 9th, 1863.

lesion, but upon the nature of the affection." In accordance with these views, Dr. Anderson proposes to consider the affections formerly so widely separated as lichen, impetigo, and prurigo as merely varieties, modifications, or stages, under different conditions, of one and the self-same disease—eczema—in which the elementary lesions are respectively pustules and papules. If it be found that such a method of classification holds in practice, and that diseases apparently so various are really amenable to the same general principles of treatment, we shall indeed have made a great step in advance. Now, although it appears to have been well ascertained that some of these diseases arise from contamination of the blood, as in the whole group of the syphilida; others from direct irritation, as erythema (intertrigo) and scabies; others from disorder, or perhaps sympathy with the alimentary tract, as urticaria from eating shell-fish, acne, and various eruptions consequent upon the presence of worms; yet it must be admitted that we have no information respecting the amount of alteration in the cutaneous secretions which precedes or accompanies such affections. This information it seems that the instrument invented by Weyrich, and presently to be described, is calculated to supply. It furnishes us with the means of ascertaining the relative amount of fluid exhaled by this organ in different parts of the body, and at different periods. There can be no question that considerable alterations in the functions of a secreting surface equivalent to fifteen square feet (Krause), and of an organ endowed with so high a degree of sensibility as this, must seriously affect all the other functions of the body, and the comparatively slight attention which has been hitherto directed to this subject can only be attributed to the difficulties which observers have experienced in drawing definite or accurate conclusions with the ordinary means at their command. The sweating skin of rheumatic or of hectic fever, the parched and dry surface it presents in diabetes, could indeed scarcely be overlooked, but slighter modifications of its functions, which may nevertheless be of importance in the causation of disease, have attracted but little notice from the practical physician. Yet it is probable that some such trivial changes, affecting a limited portion of the skin, may be the cause of that most common of all affections, a "cold."

Some means of affording an explanation of the phenomena of disease have indeed been furnished by the investigations of recent observers. Bernard has shown how paralysis of the sympathetic occasions a relaxation of the smaller vessels, thus permitting the passage of a freer current of blood and a more active transpiration of fluid through the skin. Thus, if the sympathetic of one side be divided in the neck of the horse, that side of the face and head will become bathed in perspiration; it may be conjectured that in the rapidly succeeding alterations which occur in ague and some remittent fevers, we witness the effects of an alternate paralysis and stimulation of the sympathetic centres produced by the action of some poison circulating in the blood.

In technical terms, the skin has been described by Prof. Huxley* as

* Cyc. Anat. and Phys., Supplement, p. 476.

"the outermost plane of indifferent tissue in the animal body, with its external and internal area of metamorphosis collectively, these being simply the expressions of two processes of growth in opposite directions and their lines of contact." The external layer or epidermis is termed the ecderon, the internal layer or cutis the enderon. Anatomically, and in the higher animals, it may be looked upon as a sac enclosing and exactly moulded to the surface of the body, into the external orifices of which it dips, and there becomes continuous with the mucous membranes lining the several canals. The chief of these canals are the alimentary and respiratory tracts opening by the mouth, anus, and nose; the ocular conjunctiva, the canaliculi and nasal duct, the genito-urinary tract opening by the vagina in the female and urethra in the male; and lastly, the cul-de-sacs formed by the external auditory meatus. The mode in which the passage takes place from skin to mucous membrane varies in different instances, being sometimes sudden, as at the edges of the eyelids and orifice of the male urethra, sometimes gradual, as at the orifices of the lips and nose, and at the entrance of the female generative organs. The chief cause of this difference in each case appears to be dependent on the one hand upon modifications occurring in the epidermoïd or cuticular layer of the skin, which becomes thinner and more transparent when it passes into mucous membrane, allowing the pink tint of the deeper-lying vascular parts to be perceived through it, and also permitting the transudation of a mucous fluid, whilst upon the other hand the glandular structures differ in the nature of their secretion, producing an oily secretion on the surface of the skin, and a watery one upon the surface of the mucous membrane.

Henle divides the skin as usual into three layers: an external non-vascular layer, the epidermis; a papillary layer, rich in nerves and vessels, the cutis; which passes by imperceptible gradations of structure into the third layer, represented by the subcutaneous connective-tissue, in the areolæ of which fat is stored up, and in which the glands and hair follicles are chiefly situated. It is in this third layer that bursæ mucosæ are occasionally found, as over the olecranon and patella. The epidermis, or ecderon of Professor Huxley can scarcely be accurately separated from the cutis by any mechanical means, maceration in water, or putrefaction, or the application of blistering agents merely effecting a separation of the superficial from the deeper and more delicate layer of the epidermis itself, which is well known as the rete mucosum. The external layer of the epidermis is formed of flattened, strongly-adherent scales, while the deeper layer is composed of nuclei, which are in proportionately closer apposition the nearer they are to the cutis.* In the deepest layers a cell membrane cannot, in all in-

* Recklinghausen has recently employed a weak solution of nitrate of silver for the purpose of defining the boundary lines of the epithelial cells. The silver appears to be deposited not in the cell-wall, but in the intervening substance, the cells remaining quite free from colour, and their nuclei, especially after the application of a carmine solution, still recognisable; but with stronger solutions a discoloration appears, diminishing towards the centre, the nucleus long remaining clear.

stances, be distinguished around the nuclei, and they become reduced to minute points, which are either irregularly scattered or are arranged in layers. In some parts the nuclei in the deepest layers are rod-shaped, with their longest diameter arranged perpendicularly to the cutis. Henle is opposed to the view of these deep-seated nuclei arising from endogenous cell-formation, as admitted by some authorities. He describes the pigmentary layer of the coloured man as consisting usually of a single layer of small, flat, polygonal, nucleated cells, more or less filled with pigment, which lie immediately upon the cutis. It is still doubtful whether the superficial layers of the epidermis in these races contains pigment, and this observer, at least, can discern no difference between those of the coloured races and of the white man. The colour of the skin in the areolæ of the nipples, about the arms, and in other parts of the body of the white man is partly due to the presence of pigment cells, and partly to detached portions of pigment intervening between the cells. The thickness of the epidermis, which is on the average $\frac{1}{25}$th of an inch, may amount to $\frac{1}{8}$th in parts exposed to heat or friction, the increase being chiefly due to the external layer. The cutis consists of partly rounded and prismatic, and partly flattened bundles of areolar tissue, which are imperfectly separated into layers by sheets of elastic fibres, both the layers and sheets being for the most part arranged parallel to the surface, and often crossing one another at right angles upon the same plane, like the arrangement of the straws in a straw mat, whilst these can again be seen on a perpendicular section to be crossed vertically by bundles of areolar and fibrous tissue, accompanying the excretory ducts of the perspiratory glands, the hair-follicles, &c. In the papillæ, the fibres become very fine, and the texture of the cutis very close, the external surface of the enderon being formed by a continuous layer of fine-meshed elastic lamellæ.

Smooth or organic muscular fibres exist in two forms in the skin, either as flattened bundles united into layers, as in the skin of the penis, scrotum, and nipple, or in single bundles, passing obliquely through the skin and connected with the hair sacs (arrectores pilorum). The cutis also contains the hair sacs, and the sebaceous glands opening into them, and the ducts of the perspiratory tubes. Arterial, venous, and nervous branches are apparent between the layers of areolar tissue, and lastly, there is a free distribution of lymph vessels.

The cutis varies in thickness from 0·24 mm. on the glans penis, and 0·56 mm. on the prepuce and eyelids, to 1·69—2·25 mm. on the back and face, and even attains the thickness of 2·80 mm., or nearly one-eighth of an inch on the sole of the foot and palm of the hand. As a general rule, it is thinner on the inside of the limbs and fore part of the body. In children it scarcely exceeds half the thickness that it attains in adults. It is also thinner in females, and in the Caucasian, or fair-skinned races.

As regards the lymphatic vessels, Teichmann, in his beautiful plates of the injections of this system, has shown (Pl. vi. fig. 3) that two layers may be distinguished, at least in the thick cutis of the sole of the foot: a superficial one, consisting of a minute plexus of small vessels, with oblong, narrow meshes, lying parallel with the surface, and a deeper layer, with wider and more polygonal meshes, connected with the former by vertical branches. In the scrotum this division into two layers is not perceptible. The superficial layer sends a central lymph vessel into many, if not all the papillæ of the skin, and these terminate at some distance from the point of the papilla in a simple blind extremity.

Proceeding from the deeper layer are vessels which contain valves, and which, passing through the subcutaneous areolar tissue, come to lie upon the subjacent fascia. It is important to notice that the subcutaneous areolar tissue and fat have no proper lymph vessels. The tubes belonging to the sweat and sebaceous glands, and the hair follicles penetrate the meshes formed by the lymph vessels without apparently having any peculiar relations to them. The character of the lymph vessels in particular parts, as in the scrotum, differs much at different ages: the meshes in the newly-born being very regular, whilst in adults the individual vessels are strangely dilated and ventricose, so that it would be almost impossible to recognise them as belonging to the same organ, a change which probably results, as Teichmann observes, from the alternate contractions and relaxations of the muscular fibres which are so abundantly present in that part.

The line which separates the cutis from the epidermis is finely wrinkled or corrugated, or, in other words, both these structures have teeth, which mutually interlock, the teeth of the corium being represented by the papillæ. The papillæ appear to be divisible into two chief kinds — the vascular and the tactile; though intermediate varieties, as when a vascular loop enters a tactile papilla, or when a tactile corpuscle appears as a kind of outgrowth on a vascular papilla, are occasionally met with. The vascular papillæ are usually somewhat longer than the tactile, and their number is considerably greater. Meissner counted 400 vascular to 108 tactile papillæ in a square line on the ungual phalanx of a finger; although the number of the latter is there relatively somewhat greater. On a similar extent of surface of the second phalanx he only found forty, and on the first phalanx only fifteen tactile papillæ. They are frequently arranged in groups of two, three, or four together.

The number of the tactile papillæ is relatively less on the sole of the foot than on the palm of the hand, and, in many instances, none can be discovered on the dorsum. The simplest mode in which the nerve-fibres have been seen to terminate in the skin has been described by Krause, and appears to be the rudimentary condition of the tactile and pacinian corpuscles. The "club-shaped terminal bodies of Krause" are found in the conjunctiva (two to the square line, though often also in groups of three or four), in the lips, at the bases of the papillæ filiformes and fungiformes of the tongue, in the velum palati, and in the glans penis and clitoridis. The nerves terminate here in free extremities, which are either perfectly simple or club-shaped, or form a little convoluted knot, contained within a small oblong sacculus or space, destitute of any special investing sheath, the cavity being simply bounded by a condensation of the connective-tissue of the part. Up to the point where the nerve penetrates the sacculus, it possesses a double contour; but after its entrance only a single border can be distinguished; the nerve, in other words, becomes a pale fibre.

Arnold has very recently denied the existence of these terminal knobs of Krause, and the results of his investigations are thus given in Canstatt's 'Jahresbericht,' just published, for 1862. He remarks:

1. That no importance can be attached to the terminal knobs of Krause, since they do not represent the true mode of termination of the nerves, either in the conjunctiva of men or of animals, as it may clearly be shown that nerve-sheaths and dark-edged fibres are prolonged from them, whereas a knob which really contained a terminal fibre could never give off a dark-edged fibre, again running out into a knob-like structure with a clear sheath.

2. The knobs of Krause are made in the process of preparing the tissue of the conjunctiva for microscopical examination.

3. The several constituents of the knobs of Krause are simply those of a dark-edged primitive fibre, in which the connective tissue-sheath corresponds to the neurilemma, the terminal knob to the modified medullary substance, and the terminal fibre to the axis cylinder of the entering nerve-fibre.

4. The real termination of the nerve-fibres in the conjunctiva bulbi, as well as in the connective tissue of the cornea, both in men and animals (mammalia), is in the form of a network of pale fibres.

These statements are strongly contested by Krause and Frey.

Fick considers it still to be doubtful whether the nerve penetrates the sacculus or does not rather lie on its outer side. On the palm of the hand, sole of the foot, and some other parts, these simple bodies become somewhat more complex, and are termed the corpuscula tactus, or Meissner's corpuscles; they occupy the points of the papillæ, which they completely fill, and even distend; their length is about two or three times greater than their breadth, and they present slight indentations in their sides, giving the impression of their being tightly bound round, or of their pursuing an undulating course. They are firm and resistent of pressure; an attempt to compress them shows that they are composed of a thick external membrane and softer contents. The investing sheath is fibrous, whilst the contents are finely granular, and consist of a clear matrix, with fine granules imbedded, which are but little affected by the ordinary reagents. The characteristic feature of the tactile corpuscle is the presence of firm single contoured lines, or striæ, which run obliquely, often diverging from several points, or quite irregularly and in straight or curved lines, beneath the external sheath. The length of these striæ is various, sometimes extending over the whole breadth of the papillæ, and sometimes terminating before they have reached half that distance in pointed or knobbed extremities. They appear to be cylindrical, and their number is very variable. There is much difference of opinion upon the nature of these streaks. Kölliker,*

* Kölliker: Zeitschrift f. wissens. Zoologie, Band iv. p. 43.

Nuhn,* Bidder,† and Huxley‡ consider them to be spindle-shaped cells, elongated nuclei or fibres, and in reality some modification of elastic tissue. Meissner§ and Wagner|| regard them as the terminations of the sensitive nerve-fibres distributed to the papillæ. Others take an intermediate view. Thus Ecker¶ and Leydig,** on the one hand, regard the lines in question as the prolonged nuclei of the neurilemma of the nerve-fibres that terminate in the corpuscle, whilst Ohl, Gerlach,†† and Krause‡‡ recognise in these lines nerve fibres in some places, and in others the peculiar connective tissue corpuscles. Gerlach distinguishes two kinds of lines when the corpuscles are treated with coloured solutions, which colour the cell nuclei, but have no effect upon dark-edged nerve-fibres. According to him, the greater number of the lines become coloured, and are therefore nuclei, whilst few comparatively remain untinted, and run in an obliquely winding direction. Ohl and Krause, on the other hand, maintain that the nuclei are isolated, and are distributed, most irregularly, often lying in the long axis of the tactile corpuscles between the nerve-fibres. The opinion that elastic fibres take part in the formation of the transverse markings of the corpuscula tactus is easily disposed of by the fact that they become pale, and disappear entirely in solutions of soda or potash. It is more difficult to determine whether we have before us elongated cell nuclei or fragments of fine nerve-fibres. It is certain that the streaks have neither the looped course nor the dimly granular appearance of ordinary connective tissue corpuscles, but rather something of that peculiar glistening appearance characteristic of the finer nerve-fibres. The indirect proof by which Meissner supports his view is of great importance, for he found that in paralysed members the corpuscula tactus, together with the nerves, were atrophic, and converted into rows of fine granules (fatty degeneration); and Krause brought about the same condition in apes, the only animals whose fingers are provided with tactile papillæ like those of man, by the division of the nerves of the arm. In these cases the sheath of connective tissue, with its nuclei, remained completely defined and unaltered, as did also the column of the whole corpuscle with its finely granular contents. After eight weeks scarcely any trace of the oblique fibres remained, and in the majority of cases only pale oval vesicles, with finely granular contents, were found at the tips of the papillæ. The nerves of the skin then running like the creeping roots of plants beneath the papillæ, shoot up twigs perpendicularly into them. There enter into each tactile papilla, for the most

* Nuhn: Illustrirte Medicin. Zeitung, Band ii. p. 80.

† Bidder and Kupffer: Untersuch. uber der Textur des Rücken-marks, p. 27. Leipzig, 1857.

‡ Quarterly Journal of Microscopical Science, vol. ii. p. 3.

§ Meissner: Beitrag zur Anat. und Phys. der Haut. Leipzig, 1853.

|| Gottinger Nachrichten, 1857, No. 19. ¶ Icon. Physiolog., Plate xvii.

** Leydig: Müller's Archiv, 1856, p. 50.

†† Gerlach: Microscop. Studies, p. 39. Erlangen, 1858.

‡‡ Krause: Die Terminal Körperchen der einfach sensib. Nerven, p. 67. Hannover, 1860.

part two, sometimes only one, but occasionally three or four primitive fibres, which are branches of the fibres contained in the sheath of the cutaneous nerves; and after reaching the papillæ often themselves divide, for the most part dichotomously; they then proceed either straight or in curves to the lower end of the corpuscle, or run by its side for a short distance, and then suddenly disappear; or they may even run to the upper end of the corpuscle, and then suddenly bend backwards into its substance. The nerve may often be traced to a point from which the stripings on the corpuscle appear to radiate; and at this point the nerve becomes diminished, and appears to give out its terminal branches in a verticillate or whorled manner. The structure of the Pacinian corpuscles, which is well given in all the text-books, appears to have received no particular attention of late years. The vascular papillæ enclose each of them one loop of the cuticular capillaries, the extremity of the loop extending to the tip of the papillæ. The wall of the vessel is simple, structureless, without nuclei or epithelium. Their diameter varies from 0·02 to 0·05 millimetres. Somewhat more highly developed papillæ exist than on the hand and foot, in the glans penis and nipples, whilst elsewhere they are more sparsely scattered, and are smaller.

Whether those varieties of papillæ which in the hand are known as vascular and tactile nervous papillæ occur elsewhere on the surface of the skin is still doubtful. Krause found the tactile papillæ extremely rare on other parts, as on the forearm. They have been found by Kölliker, Krause, and Henle on the lips, though they are there somewhat pale; they are stated to be smaller, and less rich in transverse markings than those of the fingers and toes.

There are two kinds of pores on the skin—viz., the openings of the hair-sacs, which are commonly recognised by the presence of the hairs, and which, when these drop out, close up to fine points; and 2ndly, the glandulæ sudoriparæ, which, whilst they are for the most part invisible, are perceptible to the naked eye on the hand, and in the external meatus of the ear, when these parts are actively performing their functions.

As regards the hair-follicles, the depth to which they extend depends upon the size and strength of the hair which they contain; sometimes, as in the case of the finer and more delicate pubescence of the body, only just dipping into the cutis; whilst in the whisker and scalp hairs they penetrate to a considerable depth in the subcutaneous areolar tissue. In all instances they form a more or less acute angle with the surface of the skin, which determines the inclination and direction of the hair-shaft. Henle's description of the hair-follicles is, that their internal surface consists of an involution of the epidermis, the two layers of which descend to the bottom, that corresponding to the rete mucosum being two or three times thicker than the other. At or just below the entrance of the duct of the sebaceous gland into the follicle of the hair, the external layer of the epidermis becomes suddenly much thinned, and is continued as two or three laminæ only of flattened cells to the bottom of the sac; the cells which are in contact with the hair itself being fusiform and arranged longitudinally. External to these layers, corresponding to the epidermis and rete mucosum, is the proper tissue of the cutis, the bundles of elastic tissue in which increase in number and become much finer, whilst the connective-tissue bundles assume a circular direction around the hair-follicles. A distinct fibrous investment can, however, only be distinguished around the coarser hairs. When present, it consists of three layers: an external thin one, composed of fine longitudinal connective-tissue fibres; a middle one, composed of several laminæ of circular fibres, closely resembling the circular muscular coat of the intestines and vessels, the fibres containing staff-shaped nuclei, but so far differing from that coat that it is impossible to separate the individual muscular fibre-cells; lastly, the third, or most internal layer, is composed of a very transparent membrane of great tenuity, which undergoes no change when treated with acids or alkalies. Within the substance of this last may be distinguished a simple layer of circular cylindrical fibres, which either run parallel to one another, or anastomose at acute angles. These fibres are so fine and closely set, that six or eight may be counted in a depth not exceeding $\frac{1}{2500}$th of an inch.

The root of every hair presents two stages of development: during the growth of the hair it is hollow, but when the hair has attained its full length it becomes solid. In the first stage it consists of a soft, gelatinous mass, with a broad base, in which are numerous round cell-nuclei, and in dark hairs, pigment molecules, seated upon a conical papilla or wart, termed the hair papilla, from which it can with difficulty be separated. The papilla proceeds from the cutis, is homogeneous in structure or indistinctly fibrous, and is penetrated by vessels and nerves, from which the materials of the growing hair are derived. In the solid condition the root of the hair may rather present a simple conical or rounded end, or it may terminate in a series of ragged fibrous prolongations, attached irregularly to the papilla and the bottom of the hair-sac.

The shaft of the hair is usually cylindrical, though flattened in the woolly hair of the negro's head. It is straight or curly, and of various colours. White hairs result from the irregularity of the surface and the air enclosed in the cells. The main body of the hair consists of cortical longitudinal fibres, which in their chemical nature are allied to the epidermis. Each fibre contains a long, delicate, and darkly-coloured nucleus. In some cases these fibres constitute the whole of the hair; in others the hair has a central axis or pith, having sometimes the appearance of a clear and homogeneous, at others of a darkly granular cord. It is usually composed of cells, the entrance of air into which occasions the white glittering appearance of white hair. Reichert and Steinlin* regard the central axis as a prolongation of the hair papilla, which forms a spiral and closely coiled spring in the interior of the hair. No medullary substance can be distinguished near the point of the hair, nor is it present in very fine hairs, nor in the hair of children under six years of age.

Hairs are almost uniformly invested by a thin epidermoïd coat, consisting of flat non-nucleated cells arranged in an imbricated manner, the edges of which can readily be seen as fine lines crossing the shaft transversely, and can be detached by the action of sulphuric acid.

* Zeitschrift f. rat. Medicin, ix. p. 288.

Henle considers it to be still a doubtful question whether after birth new hairs are developed from new sacs, or whether the stronger hairs of adults are derived by a process of growth from the follicles of the infant, though the frequency with which hairs are found coiled up beneath the epidermis seems in favour of the former view. He mentions as a curious fact that, in old age, brown and white hairs may be found proceeding from the same follicle. The changes which the hair undergoes before it is cast off have been too well described by Mr. Paget to require repetition here.

Every hair follicle has, as a general rule, a sebaceous gland attached to it, which varies but little in size, whether it is connected with the larger or smaller hairs. Occasionally two or three open into one hair-follicle, and sometimes they are absent, especially when the hairs are close set. The epidermis scales can be traced into the ducts of the gland, sometimes extending into the acini; but more commonly the flattened cells gradually give place to those corresponding to the rete mucosum; and these often cease, so that the innermost parts of the gland are composed only of the involutions of the membrane corresponding to the connective and elastic tissue of the cutis. Henle regards the larger sebaceous follicles occasionally present in the nose as an abnormal condition. The number of the hairs in a given space is very various in different parts of the body. Henle, quoting Withof, states that in a quarter of a square inch there were, in a moderately hairy man, 293 on the crown of the head, 225 on the back of the head, 211 on the frontal region, 39 on the chin, 34 on the os pubis, 23 on the forearm, 19 on the back of the hand, and upon the anterior part of the thigh 13. In delicate-skinned and fair-haired people the hairs are more closely set than in dark-haired persons.

The only remaining appendages of the skin, the nails, may be shortly dismissed. They consist of epithelial scales, modified for the special purpose of giving support to the extremity of the fingers, and thus ministering to the sense of touch. The cutis of the bed of the nail is composed of fibres, which, being arranged partly in a longitudinal and partly in a transverse direction, form a rectangular network, the inferior or deep surface of which is in connexion with the periosteum by means of areolar tissue, in the meshes of which but little fat is contained. The bed of the nail is free from glands, and the follicles or lacunæ, which were described by Rainey* as opening at the posterior part of the bed of the nail, appear to be merely parts or prolongations of the nail-roots surrounded by clavate papillæ, which remain behind when the nail is torn out. Hence it becomes intelligible how Reichert† found them filled with horny cells, and how Virchow‡ described them as being destitute of any investing membrane. The papillæ are most distinct in the upper or posterior part of the bed of the nail; here the nail also attains its greatest ultimate thickness. Anteriorly, in that portion of the bed which corresponds to the free part of the nail, the papillæ form longitudinal folds or waves, which are from fifty to ninety in number, and about one twenty-

* Hassall: Microscopical Anatomy, vol. i. p. 543. † Müller's Archiv, 1850.
‡ Verhand. der Würzburg. physikalisch. med. Gesellschaft, Band v. p. 86.

fifth of an inch in height; the valleculæ which separate them are about twice as broad as the ridges themselves. Near the extremity of the nail the ridges are less distinct, and separate papilla and groups of papillæ again appear. The vessels are very numerous, and give the red tint to this part of the nail; but the mode of termination of the nerves is unknown. The youngest portions of the nail consist, as in the analogous case of the epidermis, of small nuclei, in close apposition with one another, with indistinctly separable cell-walls. Then follows an important layer of polygonal nucleated cells, gradually increasing in size. These two layers fill up the valleculæ, and surround and surmount the ridges and papillæ, and ultimately pass into the flattened scales of the nail proper, from which, however, they are separated or defined by a dark line in the greater part of its extent. Henle concludes his description of the nail by observing that new material is only added to the nail at its posterior border, and at the upper or included part of the anterior surface of the nail track. From these points it is pushed forwards, but receives no new elements; and although new islets of nail may be developed from the fore part of the bed after destruction of the root, yet this is only an indication of the adaptability of the body to abnormal conditions, and is less remarkable than the growth, which has occasionally been witnessed, of rudimentary nails upon the middle and even metacarpal phalanx of the finger after the loss of the terminal phalanx.

Dupuytren observed that if a round instrument, as an awl, penetrated the skin, the wound was not round, as might have been anticipated, but linear. He made this observation in a young man who had stabbed himself to the heart, with intent to commit suicide. The question arose, whether the wounds had been inflicted with a penknife or with an awl, and experiments were made upon the dead subject to elucidate the matter. Dupuytren immediately found that wounds made with round instruments split the skin linearly in different directions in different parts of the body, but neither he nor Malgaigne, who repeated and extended these experiments, were able to deduce any general law. In a recent meeting of the Viennese Academy of Sciences, Professor K. Langer read a paper to show that by making a great number of punctures with a round instrument in different parts of the body, lines of cleavage could be deduced corresponding to the natural direction of the fibres, a point that might, he anticipated, be made available in operative surgery. The instrument he employed was a sharp conical awl, about one inch in length and one-twelfth of an inch in diameter at the base, larger instruments having been found to give similar results; this was oiled and introduced perpendicularly. He found that the best results were given with fat children, that the linear direction of the wounds was very constant, especially on the face and in the neighbourhood of joints, though the forearm, tibial region, and belly showed considerable variations in the linear direction of the fissures. The wounds were very rarely ragged, though in certain spots they were rectangular or triangular, the sides of these figures accurately indicating the chief direction of the fibres surrounding the puncture. It appears from this that the skin is not extended over the body as a kind of felt, but possesses a net-like arrangement of fibres, the meshes of which are usually diagonally elongated, forming rhombic figures. The narrower the meshes, of course the more nearly parallel do the fibrous bundles run, and the introduction or puncture of a sharp round instrument simply separates these fibres, producing the linear wound or fissure.

The general directions which the splits or fissures thus produced take up may be said to be that of girdles encircling the body and passing spirally down the limbs. Hence it follows, as Langer observes, that no muscle in contracting has directly to overcome the tension of the chief fibrous bundles; and further, that all folds of the skin which occur in consequence of muscular contraction indicate the line of cleavage, or in other words, the direction of the principal cutaneous bands.

Professor Langer also made some interesting remarks on the tension and elasticity of the skin. That the skin is in a permanent state of tension in most parts of the body is shown by the retraction of the edges of a cut, which is simply an effort of the skin to regain its medium thickness, length, and breadth. There are, however, certain exceptions to this rule, as in the case of the scalp, of the palm of the hand and sole of the foot, where the tendency to retract, if any, is extremely small, and it is obvious that the amount of retraction will, in all instances, be affected by the length and depth of the wound, the firmness or looseness of the subcutaneous areolar tissue, the muscularity or obesity of the subject, and lastly, by its position and the direction in which it is made.

With the view of determining these variations, Langer applied a kind of punch, with a diameter somewhat exceeding an inch, to various parts of the body, and having cut through the skin, accurately measured the retraction of the outer and inner lip (which was always the greatest), from the diameter of the punch. Occasionally, the retraction was uniform, but far more commonly an oval form was assumed, the longer diameter of the opening and the shorter diameter of the contained figure indicating the direction of the chief tension.

The amount of retraction—the punch being a circle of 30 mm. diameter = $1\frac{1}{5}$th of an inch—was—

———	At the Sternum.		Over Cartilage of Third Rib.		Beneath Axilla.	
	Outer lip of wound.	Contained figure.	Outer lip of wound.	Contained figure.	Outer lip of wound.	Contained figure.
In longer diameter ...	32	22	35	24	40	25
In shorter diameter...	32	22	24	21	30	18

The skin of the face showed little tendency to retract, probably from the firm nature of the subcutaneous areolar tissue. From experiments made with strips of fresh skin, Professor Langer ascertained that its elasticity, even when heavily weighted, was exceedingly perfect, pieces of skin 25 mm. (1 inch) in length, weighted with 10, 30, and 510 grammes, became elongated successively to 35 mm., 38 mm., and 42 mm., and on removal of the weights recovered almost exactly their original length, and from these measurements it appears that the elongation does not progress proportionately to the weighting, but becomes continually smaller, and may hence be represented not by a vertical but by a curved line, the curve rising rapidly up to 10 or 15 grammes weighting, and then very slowly rising with increase of weight.

If we regard the skin as a secretory organ, we find that it acts as an emunctory for the blood by discharging a watery fluid, sweat: an oleaginous material, which differs according to the region from which it is discharged, and certain gases.

The watery fluid is, by common consent, divided into two parts, though but few and imperfect attempts have hitherto been made to show whether any or what differences exist between them: the first of these is the insensible perspiration, which is thrown off by the whole surface of the skin by what appears to be a simple process of evaporation; whilst the other, sensible perspiration or sweat, is the product of the true secretion of certain glands, which rolls off the forehead, or may be seen oozing as bright points on the palmar surface of the hand during violent exertion, or as the result of certain states of mental emotion. The sudoriparous glands are simple tubes composed of an external indistinctly fibrous membrane with scattered nuclei, and of an inner layer of columnar epithelial cells. The blind extremities of these tubes are arranged in pairs on the hand and foot, and lined by columnar epithelium, the blind extremities of which form small coils seated in the deepest layers of the corium; the duct opens on the free surface of the corium, and according to some authors, is continued by a spiral canal to the surface of the epidermis, where it opens by a little valve-like aperture, whilst others consider that the spiral or corkscrew-like canal is only the expression of an irregular passage through the epidermis produced by the failure of the formation of the epithelial cells over that part of the corium at which the duct opens. According to Krause's estimates, there are in the

Palmar surface of the hand } Plantar surface of the foot }	2700	glands in the space of one square inch.
Back of hand	1500	„
Forehead, neck	1300	„
Breast, belly, arm . . .	1100	„
Cheeks, thighs	5–600	„
Back	400	„

Giving a total of nearly 2,400,000 in the body, and a total volume of about eight square inches.

Wilson's estimates are somewhat higher, giving as many as 3528 per square inch for the palm of the hand.

Meissner is of opinion that the so-called sweat-glands really secrete an oily material, and are therefore rather analogous to the sebaceous glands. He observes that they are in greatest number in those parts where, as in the sole and palm, the sebaceous glands are entirely absent, or where, as in the axilla, the skin is exposed to continual friction, and he also points to the analogy of their structure with those of the ceruminous glands of the ear, and to the evident presence of fat molecules in their secretion. However this may be, it is impossible to obtain any chemical analysis of the insensible and sensible perspiration separately. Indeed, there is always an admixture of sebaceous matter and of epithelial scales. He is also of opinion that the true perspiration is merely a kind of serous exudation from the surface of the corium, and especially from the papillæ; but this appears to be negatived by the absence of an albuminous substance in sweat, and by the circumstance that there are numerous papillæ richly supplied with blood under the nails, which certainly do not permit of any transudation. The sweat, as described by V. Gorup-Besanez, is a colourless but somewhat turbid fluid, possessing a peculiar odour, characteristic of the individual and of the region from whence it is obtained, and of a distinctly saline taste. It contains no proper formed elements, but there is always an admixture of epidermal scales. Its reaction is acid to test-paper, though it becomes alkaline after long exposure to the air. Its reaction is also stated to be in some measure dependent upon the length of time during which the secretion has been actively performed, that portion which is discharged after the secretion has been for a long time continuously excited being, according to Gillibert and Favre, neutral, or even alkaline.

Funke, on the other hand, believes that so far from any evaporation taking place from the general surface of the body through the horny epidermal scales, these actually prevent and limit the loss of fluid and the consequent reduction of temperature.

Those materials which have been shown to be constant constituents of the sweat are water, fat, volatile fatty acids, formic, acetic, butyric, and probably also propionic acids, urea, and inorganic salts, amongst the chief of which are the chlorides of sodium and potassium, the alkaline phosphates and sulphates, earthy phosphates, and iron. The salts are therefore similar to those of the blood. The following table from Gorup-Besanez will give an approximative idea of the composition of the sweat.

	Anselmino.				
In 1000 parts.	I.	II.	Favre.	Schottin.	Funke.
Water	995·	987·5	995·573	977·40	988·40
Solid residue	5·	12·50	4·427	22·60	11·60
Epithelium	0·10	0·25	—	4·20	2·49
Fat	—	—	0·013	—	—
Lactates	—	—	0·317	—	—
Sudorates	—	—	1·562	—	—
Extractive matters	1·45	3·62	0·008	11·30	—
Urea	—	—	0·044	—	1·55
Chloride of sodium } Chloride of potassium }	2·40	6·00	2·230	3·60	—
			0·024	—	—
Phosphate of soda } Alkaline sulphates }	—	—	trace	1·31	—
Alkaline sulphates	1·05	2·62	0·011		—
Earthy phosphates	—	—	trace	0·39	—
Salts (total)	—	—	—	7·00	4·36

Other constituents of the sweat, which are either not constant, or whose presence has not been accurately determined, are the salts of ammonia, which, though often recognisable, are probably the result of decomposition, the hydrotic or sudoric acid of Favre, which is said to contain nitrogen, and certain pigmentary substances of various colours. In disease, urea and uric acid have been found by Wolff and Hamernjk, albumen and grape-sugar by Anselmino in rheumatism, and the colouring matters of bile by others; but much doubt hangs upon their discrimination, and no physiological and pathological inferences of importance rest upon them. When taken into the stomach, benzoic acid (partly converted into hippuric acid) reappears in the sweat (Meissner), as do also succinic and tartaric acids, iodine and iodide of potassium, though Lehmann and Schottin deny that the two last can be recovered.

It is interesting in passing to notice the extraordinary increase of urea contained in the sweat in Bright's disease and in cholera, in the latter disease being discharged in such quantities as actually to form in some case a thin, sparkling crust over the skin on the drying-up of the sweat (Schottin). Virchow* found in the perspiratory fluid of phthisical patients, suffering from colliquative sweating, numerous epithelial scales belonging to the sweat-glands, in a state of fatty degeneration. Funke calculated the normal quantity of urea at 1·12 per 1000 parts of the whole sweat in one case, and at 1·99 in another. Picard estimated it at 0·88 per 1000.

From the above somewhat discordant analyses we may conclude that the sweat is one of the most watery of the secretions, and it may perhaps in some measure serve to reconcile the differences if we remember that the nature of this secretion probably varies—1, with the rapidity; 2, with the duration of the secretory act; 3, with the surface from which the fluid is collected; and 4, with the nature of the food and drink previously taken.

Thus it appears that the amount of organic matter, as of free acids and of the salts of the volatile acids, diminishes whilst the salts increase with increased discharge of sweat within certain limits. The amount of urea, however, increases unless the discharge is very great and long-continued, when it considerably diminishes. And again, Funke found in the sweat of the foot of one person 13·7 of solid residue per 1000 parts, with 4 per 1000 of salts. In the sweat of the axilla, which was exactly similar in appearance, there were only 2·4 per 1000 ashes. Schottin also found in the ashes of the sweat of the arm 27·5 per cent. of sodium, and 15·7 per cent. of potassium; whilst in the sweat of the foot there were 29·88 per cent. of sodium, and 11·78 of potassium.

Lastly, Meissner observed that vegetable food diminished the proportion of urea contained in the sweat.

The absolute amount of loss by insensible perspiration per diem was estimated by Seguin at a little more than two pounds avoirdupois, or $\frac{1}{67}$th of the weight of the whole body, and nearly double the quantity which is given off by the lungs. Valentin† calculated that the loss by evaporation from the skin, was to the loss by the lungs as 3 : 2. These statements closely accord with those of an old English experimenter, Rye,‡ who estimated the total loss by the skin at $\frac{1}{85}$th of the weight of the body per diem; but considerable variations appear to exist in different individuals; thus, Funke, in experimenting upon himself and two of his colleagues, MM. Brunnen and Weber, on one occasion, when ex-

* Archiv fur Path. Anat. Band xiii. p. 288.

† Report f. Anat. u. Phys. Band viii. p. 389.

‡ Rogers' Essay on Epidemic Diseases. Dublin, 1734.

posed to a temperature of 80° F., and with violent exertion, collected in a small caoutchouc bag placed round the arm, from one of the experimenters 240 grs., from another 104 grs., and from the third 463 grs. of fluid, a difference which was perceptible in all their experiments, though not generally so strongly marked. Funke found that the lower extremities discharge less sweat, *cæteris paribus*, than the upper. The extremes were equally remarkable. In Funke himself, the quantity hourly excreted from the same arm in summer time varied from about 67 grains to 740 grains of fluid; in Weber, it varied from about 102 to 354 grains, and in Brunner from 47 grains to 215 grains.

Before passing on to the consideration of Weyrich's Treatise, we may just allude to the fact that a certain quantity of carbonic acid is eliminated by the skin, amounting, according to Hannover, to $\frac{1}{38}$th, according to Scharling to $\frac{1}{50}$th, and according to Regnault to $\frac{1}{100}$th of that given off by the lungs, and therefore probably varying from one hundred and fifty to three hundred grains per diem at the outside. It is not improbable that some other excretory products are discharged from the skin, since it is difficult otherwise to explain the cause of death proceeding from shaving and varnishing the whole cutaneous surface of the lower animals. Bouley* found that death occurred in three horses subjected to this treatment, from a kind of slow asphyxia, on the seventh, ninth, and tenth days, the lungs and viscera being found after death gorged with dark blood.

Weyrich's observations and inquiries had for their object—

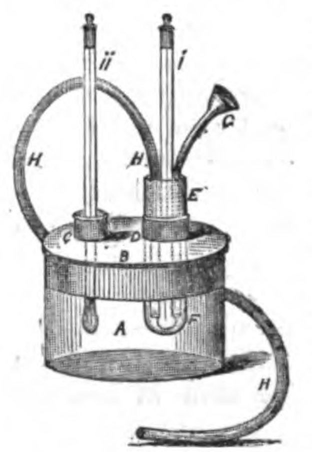
1. The determination of the average and of the relative amount of the cutaneous transpiration from various regions of the skin in healthy adults under ordinary conditions.

2. The determination and explanation of variations from this average produced by external and internal causes.

Now, in order that any apparatus for the scientific determination of the amount of the cutaneous transpiration in either health or disease should come into general use, it is clearly requisite that it should combine facility of application with accuracy in its results, and these, Weyrich believes, may be most easily attained by the employment of an instrument constructed on the principle of the condensing hygrometer for which we are indebted to our own countryman, Daniell. The theory of this instrument is sufficiently simple. When water is exposed to air, a certain quantity of its vapour is taken up, varying with the temperature, and if the supply of water be unlimited the air soon becomes saturated with aqueous vapour. If now the temperature falls, a precipitation of the water takes place in the form of dew, mist, rain, hail, or snow, but if the temperature be raised, a larger quantity of aqueous vapour will be taken up. The temperature at which the precipitation of the vapour first begins to take place is called the dew-point. Such precipitation may be always observed when a glass of ice-cold water is brought into a warm room, and practically the dew-point is always determined by cooling down a metallic and brightly polished ball in contact or in close contiguity with a delicate thermo-

* Bouley: Recueil de Med. Veter., pp. 5, 805. 1850; quoted in Longet: Physiologie, vol. i. p. 970.

meter. The cooling is effected by the evaporation of a little ether. The dew-point being thus ascertained it is easy to ascertain, either by certain algebraical formulæ constructed by Regnault, or by tables which have been published by Gyot, the exact amount of aqueous vapour which is present in a given volume of the air. Thus, it has been determined by the latter experimenter that one cubic metre (= 35·28 cubic feet English) at 32°, when saturated with aqueous vapour, contains 74 grains. At 50° F. it contains 140 grains, at 61° F. 208, and at 70° F. 285 grains. A certain pressure is of course exerted by this vapour. At 32° it is equal to a column of mercury 4·6 mm. high, at 50° of a column of 9·165 mm., at 61° of 13·536, and at 70° of 18·495. Both the amount of aqueous vapour and its pressure, it will be observed, rapidly increasing with the increase of temperature.



The apparatus employed by Weyrich will be best understood from the following diagram, where A is a strong glass vessel about two inches in height, rather less than $2\frac{1}{3}$ inches in diameter at the base, and slightly tapering towards the summit; the edges of the vessel are ground true, so as to be air-tight when pressed with moderate firmness on a plane surface; to the upper orifice is cemented a metal cover, B, perforated near the centre by two openings, into which two short metal tubes are soldered, C and D. Through the smaller orifice, C, a thermometer stem is passed, the bulb being within the vessel, so that it can be elevated or depressed at will; the larger opening, D, has a glass tube, E, adapted to it, the extremity of which projecting into the vessel is fitted with a tightly fitting, but brightly polished, silver or copper electro-gilt cap, F. This tube, with its cap, is also freely moveable in a vertical direction through the brass tube D. This is constantly referred to as the "metallic capsule;" contained within this capsule, and projecting from the upper orifice of the glass tube, E, is a thermometer, *i*, whose bulb does not quite touch the bottom of the capsule, and also two tubes of glass or metal whose diameter may lie between 1·5 and 2 mm., and which reach as far as the bulb of the thermometer. One of these, G, runs up and terminates in a funnel, and through this a graduated quantity of ether can be allowed to flow upon the bulb of the thermometer; the other tube ends by being connected with a caoutchouc tube, H, two or three feet long; these two tubes and the thermometer stem are kept in position by passing through a piece of cork. Through the latter tube a free current of air can readily be made to pass. The capacity of the bell glass, when all the parts are in position and ready for use, is about 94 c. c., or nearly 6 c. i. English. It is scarcely requisite to notice that the thermometers should be as sensitive as possible.

In determining the choice of locality to which this apparatus could be conveniently applied, Weyrich found that he was necessarily limited to those parts on which the instrument could rest securely and air-tight. He found that its area comprehended from $\frac{1}{500}$th to $\frac{1}{600}$th of the whole cutaneous surface, or about $\frac{1}{570}$th of a man of average size. In the course of a number of preliminary observations, he found that under similar conditions of temperature, pressure, light, exposure, &c., the portions of skin which are symmetrical upon the two sides of the body are equal in the amount of perspiration discharged, but a comparison of different parts gave very different results. The same portions of skin were also found to discharge different amounts at different times, but those regions which gave the most constant results, or presented the smallest variations in the amount of perspiration, were the infra-clavicular region, the upper part of the back, the external surface of the thigh, and the upper part of the calf of the leg. The sole of the foot and the palm of the hand presented great variations, and were often considerably above the average, especially in those whose skin was delicate. Ultimately, for various reasons, he chose the infra-clavicular region. Weyrich appears to hold the same opinions as Meissner, in regarding the perspiration as a general transudation taking place from the whole capillary network, and permeating the whole surface of the epidermis, whilst the perspiratory glandulæ only take a greater share in this function than the sebaceous follicles on account of their secretion being more watery. It was in the next case requisite to ascertain the length of time which elapses before the air in the bell-glass becomes completely saturated with aqueous vapour, and upon trial he found that though five or six minutes is requisite for this purpose, yet that, in consequence of the chief addition of aqueous vapour taking place during the first minute, it might practically be considered as complete after three minutes, and this accordingly was the unit of time employed in his observations. The determination of the variation of temperature in the air under the bell-glass was ascertained by thermometer No. 2. The enclosure of a portion of the cutaneous surface and of the air covering it by the instrument, must necessarily interfere with the convection of heat, and seriously diminish the amount of insensible perspiration; and this must have the greater effect the longer the experiment lasts; but, as Weyrich observes, the rise of temperature in the contained air, as indicated by thermometer 2, affords some compensation for this.

The mode in which the observations were made with this apparatus was the following:—1. The temperature, and then the proportion of aqueous vapour contained in the air of the room, were accurately ascertained. The thermometers were then brought to equality, and the polish of the metallic capsule observed to be perfect. The bell-glass was then applied to the skin for two minutes forty-five seconds, a little ether injected, and the dew-point rapidly determined, from which the calculation of the amount of aqueous vapour contained in the included air was made. The frequency of the pulse and the temperature of the axilla were noted, and lastly the external meteorological conditions, temperature, barometric pressure, weather, &c.; the latter points he obtained from his friend Kämtz, of Dorpat, for the hours of seven A.M., two P.M., and eleven P.M. of each day.

As Weyrich's observations were made upon himself, it is as well to remark that he is thirty-nine years of age, of a healthy constitution, rather thin, moderately strong, of a lively disposition, five feet nine inches in height, fifty-six inches round the chest below the nipples, and of a vital capacity equal to 326 cubic inches, 143 pounds in weight, and with no unusual disposition to perspire. His daily occupations appear to have been professional and moderate in amount, his food simple, and his habits regular. His study, in which his observations were made, was large and roomy, artificially heated, and he was invariably in the sitting posture. His experiments were continued over one year and a half, but those of the first half-year were rendered comparatively useless in consequence of the fracture of his thermometer, which he replaced with one of much greater delicacy.

The causes which induce variations in the amount of perspiration may be reduced to two main divisions, internal and external, the former being physiological, the latter physical. Amongst the external or physical agents may be mentioned, as amongst the more important— 1. The temperature of the air, for the higher the temperature the greater the amount of aqueous vapour which can be held in suspension. 2. The pressure of the air, for the smaller this is the greater is the amount of evaporation. 3. The relative moisture of the air. 4. The state of rest or motion of the air, the latter being singularly favourable to evaporation. Lastly, other accidental circumstances, as sunlight, clear sky. In fact, it is evident that those conditions which influence evaporation in general must necessarily affect that process as it takes place from the human skin. Krause made a series of careful experiments upon the amount of evaporation that took place in a dead body in air artificially dried by SO_3 and CaCl, and found that on the first or second day the evaporation for twenty-four hours from fifteen square feet of surface at 95° F. was $=$ 7363 grammes ($=$ 113,684·72 grains), but from the fourth to the sixth day, only 3537 grammes ($=$ 54,611·28 grains). A few observations made by Weyrich show that the insensible perspiration which is given off within a definite period a day or two before death, in the act of dying, and shortly after death, stand in the relation of 3·95 : 5·61 : 1·86, the agony of dying being probably accompanied by a great relaxation of the pores of the skin.

The chief conditions which were found by Weyrich to influence the amount of perspiration were the following:

1. *Season of the Year.*—According to the experiments of Sanctorius, which were made by successive weighings of the whole body, no food having been taken in the interval, nor any urine or fæces discharged, and subsequent estimation of the relative loss by the skin and lungs, less fluid is eliminated by the skin in cold than in warm weather, and the quantity varies inversely with that of the urine. There can be little doubt of the correctness of this general statement in those whose bodies are freely exposed to the weather; for it is obvious that external cold, constringing on the one hand the cutaneous vessels, and consequently increasing the quantity and pressure of the blood in the internal vessels, will supply the conditions favourable for the active discharge of their function by the renal organs, and diminish

on the other hand the secretory activity of the skin. The results, however, obtained by Weyrich do not altogether accord with this view—at least for those who are protected from external cold by in-door occupations—since he found in one set of experiments that the highest average was attained in January (3·86 mm.), then in February, and then in March, and that there was a gradual fall through the succeeding months to July. In a second table, however, this result does not come out quite so clearly, and, upon the whole, he is inclined to believe the effect of season to be secondary, maxima and minima occurring in months whose average temperature is much opposed.

2. *Influence of Variations in the Pressure of the Air.*—This constitutes a factor of seasonal influences from which none can withdraw themselves, and which is equal, *cæteris paribus*, for all those resident at the same height above the sea. Vivenot has, however, shown that the barometric pressure is not the only element to be considered as influencing the economy in a physiological point of view, but that the vapour-pressure arising from the quantity of water held dissolved in the air is also to be carefully regarded, since the barometric pressure may be the same, though the relative quantity of watery vapour, to which probably our various feelings of comfort or discomfort, exhilaration or depression, are due, may be very different. Vivenot's observations led him to conclude* that residence in rarefied air, as on high hills, caused increase of evaporation, increased frequency of respiration, and acceleration of the pulse; congestion of the superficial vessels; sensations of fatigue in the joints, possibly arising from diminished pressure of the ends of the bones against one another; and, lastly, diminution in the quantity of urine. Weyrich's observations, however, fail to show any definite influence between the pressure of the air and the amount of insensible perspiration. In regard to the condition of the weather, he observed, as might have been anticipated, that perspiration is favoured by a bright serene sky, and diminished in sullen and overcast days.

3. *Temperature.*—From a consideration of all his experiments, Weyrich concludes, 1, that the skin is subject to the ordinary laws which govern the evaporation of fluids; and 2, that whilst at mean temperatures there is a mean amount of perspiration, with increase or decrease of temperature there is a corresponding increase or decrease in the amount of perspiration. Under mean temperature he includes from 55° to 70° F. Variations within those limits produce little effect, but beyond them it would appear that for every 1° C. = $1\frac{4}{5}$° F. below 55° F. there is a decrease of perspiration equal to from 1 to $1\frac{1}{2}$ per cent. of the whole amount which is discharged between 55° and 70° F., whilst for every $1\frac{4}{5}$° F. above 70° F. there is an increase on the whole amount equal to 2 per cent.

4. The influence of period of the day is believed by Weyrich to be such that the perspiratory process is most active in the morning and at noon, and the excess of the diurnal over the nocturnal perspirations

* Virchow's Archiv, Band xix. p. 492. 1860.

is about 20 per cent. of the latter value; but the influence of food is very great, and may modify to a remarkable extent the secretory process, as is shown by the fact that Sanctorius, in opposition to Weyrich's and also to Valentin's researches, considered that the nocturnal perspiration was greater in amount than the diurnal—a clearly erroneous conclusion, attributable to the fact that the old philosopher was in the habit of taking a hearty supper before retiring to rest. Valentin's observations on the influence of the period of the day on the urinary and cutaneous secretions are of considerable interest. From eight observations made at night, an average loss of weight by perspiration was obtained of 570 grains per hour, which, compared with the quantity of urine eliminated in the same time, gave a proportion of 0·591 for the urine and of 0·409 for the perspiration; consequently the nocturnal perspiration was to the nocturnal urine as 0·692 : 1. But Valentin determined the relation of the urine to the perspiration, on the whole, to be as 1 : 0·839; whence it follows that the nocturnal perspiration, compared with the diurnal, shows a diminution of from 20 to 25 per cent., a result which closely accords with Weyrich's.

5. *Influence of Food.*—This is admitted on all hands to elevate the amount of perspiration. After each meal the rise may be represented by curves whose sharpness either of ascent or descent are not exactly equal either in extent or duration, the ascending curve being sharpest for breakfast, while the extent and duration are least, the maximum being attained in about an hour. The ascending portion of the curve of the evening meal (substantial tea) is somewhat longer, the maximum not being attained till after the lapse of two hours, though the absolute elevation is greater; and lastly, the curve of ascent after dinner is much the greatest; both in intensity and extent, attaining its maximum at the fourth hour after that meal. Tea, coffee, and alcoholic liquids greatly increased the perspiratory power; the latter in the form of a glass of punch or a couple of glasses of good wine, amounting to nearly double the fasting average (80 per cent. increase).

6. *Exercise.*—Severe mental or bodily exertion occasioned invariably an increase in the amount of perspiration; when sweating occurred, the rise was more than double, 116 per cent. over the mean amount, but after sweating, there occurs, as a kind of compensation, a diminution equivalent to 26 per cent. of the mean amount. These conclusions accord well with those of Speck and Lehmann, Böcker, and others; mental exhaustion or depression of spirits produces in like manner a diminution equivalent to 10 per cent. of the fasting average.

7. Lastly, Weyrich made a few experiments upon the following points. *a.* The effect of a mustard poultice. *b.* The effect of cold. *c.* The effect of covering the skin with oil. *d.* The effect of a soft brush. Symmetrical points of the body were examined for the sake of comparison, one being in the natural state and the other under treatment.

a. Mustard poultice applied for thirty minutes gave, on five trials, an average rise of 67 per cent. over the mean.

b. Snow or ice applied for some time occasioned a fall of 51 per cent., when the skin was pale and benumbed, but when reaction had taken place, it was 16 per cent. above that of the opposite side.

c. When warm oil was gently but abundantly rubbed in for two minutes, so that the skin was obviously oily, there was a constant rise of about 27 per cent. beyond that of the opposite side.

d. When the skin was lightly rubbed with a soft brush for five minutes, there was an increase over that of the opposite side of as much as 80 per cent.

There remains yet one other important remark made by Weyrich, with which we shall conclude this portion of our subject. It has commonly been admitted that the urinary and perspiratory functions are antagonistic, and indeed, vicarious of one another. In Weyrich's observations, however, there was not that agreement with this theory which might have been expected. On the contrary, it would rather appear, that under similar conditions, avoiding extremes, both excretions rise and fall together, but that when extraordinary circumstances come into play to disturb the equipoise, in all probability the law of antagonism and vicarious action comes into force. No indication, however, can in any case be drawn of the perspiratory power or amount from estimates of the amount of urine discharged.

Review XI.

Annual Reports of English County Lunatic Asylums.

The Reports of County Lunatic Asylums constitute a class of medical literature to which we have for two or three years past desired to call the attention of our readers, as a promising source of information respecting the state and progress of opinion relative to the treatment and management of the insane and to the pathology of their malady. In our wish to fulfil this task earlier, we have, however, been disappointed, owing to the defective condition of our collection of such reports. To remedy this defect, we have for the few past years requested, by notice in these pages, to be furnished with these annual returns by the authorities of the several asylums of the country, and our request has, we would thankfully acknowledge, been acceded to on the part of a few with pretty constant regularity, but on the part of the majority, in a fitful, irregular manner. Of few asylums, therefore, are we able to follow out the history, to form any accurate notion of the movements of their populations, or to deduce any statistical results from them.

The appearance of medical reports, penned by the resident superintendents of county asylums, is due to a requirement of the Lunacy Acts, calling for an annual return of admissions, discharges, and deaths, specifying recoveries and such other particulars as the medical officer may choose to add. The official meagre details will therefore form the substratum of asylum reports, and not many years ago, such was all that was supplied by many of our public institutions for the insane, the whole being comprised, along with the returns of receipts and of expenditure, the cost of board, clothing, &c., on a sheet of paper. Such barren and brief reports have of late years been replaced by others on a larger scale, and now each county asylum publishes an annual pamphlet, varying much in bulk and in the character and value of the contents, the cost of its publication being chargeable to the county rates.

The oldest attempt to render an asylum report something more than a calendar of admissions, discharges and deaths, of receipts and expenditure, was made by the authorities of the Middlesex Asylum at Hanwell, the medical details being supplied by the then resident physician, the late Sir W. Ellis, and the remarks of this officer will always be of interest, as foreshadowing several of the improvements in what is termed the moral treatment of the insane, which were subsequently so largely elaborated in the same institution under the energetic and persevering efforts of Dr. Conolly. Sir W. Ellis was one of the first to fully recognise the value of employment for the insane, and it is interesting to watch the development of the modern system of management from this beginning under the one physician of Hanwell, until its full outburst and maturity under his successor. The reports of Dr. Conolly, issued during his residence at the Hanwell Asylum, especially his earlier ones, show us the successive unfolding of his views, the obstacles and difficulties entailed by the very nature of his labours, as well as those thrown in his way by various individuals then armed with a little brief authority, and the manner in which he met and overcame them. These reports will therefore always be read with interest by the student of the history of non-restraint. They contain much of what he afterwards put forward in his well-known and valuable treatise 'On Asylum Construction and the Management of the Insane.'

The reports of few asylums go back to this, as we may term it, historic period in the treatment of insanity. In fact, the majority of the existing asylums have been established since Dr. Conolly's doctrines have become generally accepted, and their medical officers have consequently had the comparatively easy task of applying and developing them. And looking to the reports made by the several superintendents, as well as to those published by the Commissioners in Lunacy, there are good grounds for satisfaction in the manner in which the non-restraint doctrines have been carried out to the great benefit of the insane.

Dr. Conolly's numerous followers in the practice he advocated, were greatly encouraged by the history of his own perseverance and success, as it was universally set forth in the Hanwell Reports, whilst their number was constantly being recruited from the band of students who followed the clinical teachings of their master in the wards of the metropolitan asylum. The abolition of mechanical restraint became the primary object of almost every superintendent, and the various reports detail their difficulties in accomplishing this end, note cases in which they were for a time partially baffled in carrying it out, and record expedients adopted in lieu of coercion, and suggestions for improved forms of clothing, beds, and other articles of furniture, &c.

Again, the altered views regarding the treatment of lunatics entailed changes in the construction and fittings of asylums, and in the responsibilities and duties of officers and attendants; and all such matters came within the compass of the annual reports. Other standard topics are the condition—often most vile—in which patients are sent to asylums, the histories of sudden and violent deaths, of suicides, and of injuries, accounts of escapes and of any unusual sickness, defects in the laws, &c.

After reading and comparing the older with the most recent reports of asylums, the fact stands clearly forth, that not only has non-restraint been accepted as the principle of treatment in its entirety, with one or two exceptions only in the country, but also that most of the expedients originally substituted in the room of coercion have been laid aside or greatly reduced in the extent of their application, and many exceptional kinds of dress and furniture become disused. For instance, when the strait-jacket, belts, and bands were put away, the difficulty of dealing with refractory, violent, and destructive patients required in some way to be surmounted, and the first means resorted to for the purpose were seclusion in a cell, particularly in a padded room, and strong tick dresses, bordered with leather, and fastened by ingenious button-locks. Again, for those of dirty habits, trough-like bedsteads, lined at the bottom with lead, and perforated to allow the escape of the urine into a vessel or fixed drawer beneath, and canvas stretchers in lieu of mattresses, were contrived to obviate annoyances and to obtain as much cleanliness as seemed practicable. At the present day, however, seclusion is looked upon as an evil only second to coercion itself—not, perhaps, to be entirely abolished, but to be resorted to only exceptionally; and the asylum superintendent now prides himself on the few instances of seclusion to be noted during the year. And not only has the number of patients secluded, and the frequency of seclusions become much reduced, but also the length of time during which seclusion is practised. Probably the difficulty of dealing with many patients, as felt when first mechanical restraint was abolished, led to an unnecessarily frequent resort to seclusion; but this evil has been materially diminished by an excellent regulation made by the Lunacy Commissioners, that a register should be kept in every asylum of every instance of seclusion and of its duration. Instead, therefore, of seclusion being adopted to calm excitement, scope is given to the patient's energy and activity in the extended airing-courts and grounds which, unlike those of former times, are not cut up into numerous small courts, assigned to as many groups of patients, and made bare, deprived of everything which was thought might harm the patient or be injured by him, but are thrown together into large spaces, laid out and planted as gardens, to arrest and divert the mind and suggest ideas of order and quiet.

Along with seclusion, the ingenious special devices in the way of clothing and furniture have very much gone out of fashion. Exceptional arrangements prove provocative rather than curative of exceptional conduct and habits. Careful arrangements of bed and bedding, to obviate the inconveniences of dirty habits, are suggestive of the continuance of such habits; the wearing of strong dresses is to many an insane mind a challenge of strength to tear them; and in place of all such schemes of mechanical prevention, attention, watching, diversion, and employment are found to be the proper substitutes. The less a lunatic is dealt with as an exceptional being, so much the better, and so largely is the truth of this principle now felt, that the grand desideratum with most psychological physicians is how best to assimilate the condition of the insane with that of ordinary life; how best to restore to the lunatic the "*vie de famille*" when his malady renders the severance from his family essential. This question has been discussed in the reports of some superintendents. Dr. Bucknill, the present Visitor of Chancery Lunatics, in his report of 1855, refers to his having successfully tried the plan of boarding selected patients among cottagers in the neighbourhood of the Devon Asylum, of which he was then superintendent, and in a subsequent report (1858) gives an account of the removal from the asylum of a considerable number to a house at Exmouth, under conditions of life much more nearly resembling those of an ordinary home. With the same object in view, as well as for the purpose of providing additional accommodation for the increased number of insane in the county, detached cottages have been erected in the grounds of the Devon Asylum—a plan which appears likely to be pretty generally adopted. Thus, Dr. Lockhart Robertson, in his Fourth Report of the newly-erected Asylum for Sussex, speaks of the time having arrived for the building of cottages on the asylum estate, in which married attendants might reside, having a small number of patients under their charge. Non-restraint is throughout Great Britain an accepted principle in dealing with the insane, and an accomplished fact; the great problem that has arisen in its place is, whether it is necessary and right to continue to build large establishments of the usual model of asylums for the aggregation of the insane under a strict discipline and conditions of life widely different from those of their previous existence, or whether we may not bestow our lunatics in colonies of a certain sort, distributed in cottages grouped within a limited area, and connected with a small central institution, serving as an infirmary and as the head-quarters of the medical and general administration? What the sentiments of asylum superintendents may be on this question, their reports do not enable us to judge; but the proposition above quoted, to build cottages within the grounds of asylums for some of their inmates, points to opinion as somewhat setting in that direction, and to the giving way of the old notions of entirely secluding lunatics in specially-constructed and adapted buildings, almost entirely cut off from intercourse with the sane members of society.

This last subject—the intercourse of the insane with the sane—has been mooted by several of the writers of reports before us. The general impression is that such intercourse is desirable as an aid to cure where cure is possible, and as a preservative against the natural tendency of chronic insanity to lapse into dementia. In asylums as at present constituted, there is a difficulty in the way of allowing visitors in the wards; there are few people competent as visitors; absurd notions and prejudices are still rife among the public at large relative to the insane and insanity, and before the wards of asylums can be thrown more open to visitors these must be overcome, or at least, there need be a much larger class of judicious, clear-headed people than is to be found at the present time. Nevertheless, the practice now prevalent in asylums tends to promote an increasing intercourse of the sane with the insane, and to extend just notions respecting them. The inmates of asylums are no longer the prisoners they formerly were; very many of them are taken for walks in the neighbourhood, and at times for excursions to a greater distance—even to places of public resort—and latterly, at the Northampton Asylum, under the care of the Superintendent, Dr. Wing, some patients were treated to a visit to Llandudno, in North Wales. Moreover, they not only are taken out among the public, but visitors are much more freely admitted among them. The visits of relatives are encouraged; official and medical visitors are frequent in the wards; teachers from without, mechanics to overlook their labour or to carry on work in the building, are often mingled among them, as well as visitors, who take part in their frequent gatherings for amusement. In these and other ways the influence of the sane mind is to some extent brought to bear upon the disordered intellect of the occupants of our asylums as they are. By the scheme of cottage distribution, this influence would become much more largely and continuously operative, and as happens in the insane colony at Gheel, in Belgium, the sane and the insane would become so intermixed that the peculiarities of the latter would often elude notice.

One effectual way of bringing the insane mind under the influence of a sane one, is that of teaching, the subject-matter of teaching being of subsidiary importance, and regulated by the condition and wishes of those to be taught. The employment of schoolmasters and other teachers has been adopted in some asylums, but on the whole has, in our opinion, been much neglected. We presume the difficulty is to find suitable instructors. A mere pedagogue for a teacher must be a failure with asylum classes. The teacher need first to learn something of what lunatics are before he can properly teach them; and it is indeed scarcely fair to introduce a well-meaning but uninitiated man to a class of lunatics, and expect him forthwith to profitably instruct them. To educate, or rather to discipline the insane mind, to bring it into the ordinary groove and channel of thought, and to lead it to forget its vagaries, is not the task of an ordinary schoolmaster, but of a man who has made the insane a study, who feels for them, who knows as well how to curb as to encourage and cheer them.

Doubtless it is the absence of such properly qualified instructors that has rendered teaching generally a failure. At Hanwell, a schoolmaster was engaged for a year or two, and suddenly set aside. The committee (in 1853) informed the master his services would no longer be required. No reason is assigned for this discontinuance of the school; indeed, it occurred in the face of the schoolmaster's report for that year "that the numbers attending the school have considerably increased;" that at the evening meetings "the room is always filled with the superior class of female patients and attendants. The same result applies to the day-room of No. 2 ward, on the male side, when similarly engaged. Singing classes have been fully attended, and . . . . our lectures are a never-failing source of interest." The school having thus attained (if we can receive this evidence, printed by authority) a high and increasing state of usefulness, the magnates on the committee, without any concurrence on the part of their medical officers (whose opinion should surely be paramount in all that relates to the treatment and management of the diseased inmates), suddenly and inexorably extinguish both school and schoolmaster. Such an act is inexplicable, particularly as in previous reports the committee had vaunted their school, and their far-seeing sagacity in being among the foremost in establishing such an institution in an asylum. In the last report, for 1862, we are glad to observe a partial revival of the attempt to teach, a patient on the male side acting as schoolmaster; and we are further pleased to find among the recommendations of the Commissioners in Lunacy, "the organization of a school in the female division."

The schoolmaster need be more abroad in the asylums of this country. The occasional lecturer, the conjuror, and other performers, are at times enlisted for the instruction or amusement of asylum inmates—a plan frequently followed out by Dr. Hitchman of the Derby Asylum. Drilling and gymnastics are more common; we could wish that they were in vogue in all asylums.

Amusements are afforded to the patients in all the asylums. Within doors there are periodic parties for music, dancing, and singing, the two sexes usually intermingling. Now and then there are private theatricals. The introduction of such exciting amusements appears to meet with general approval; but there are some few lookers-on who doubt its utility and expediency, especially on the large scale attempted in a few of our colossal establishments, in the presence of a mixed company of visitors. To certain minds such proceedings savour too much of glorification of the liberality and other qualities of the committees, and seem to make the unfortunate inmates too much of a show, and, in the case of some among them, of a laughing-stock. In some cases, certainly, patients are too indiscriminately admitted to share in such festivities, due regard being had to their mental state, their previous position in life, and their moral feelings and emotions. This error obtains principally in the very large asylums, where the medical men cannot assume individual supervision and control, nor have that personal knowledge of their individual patients necessary to judicious selection, and where, consequently, much is left to the will and pleasure of attendants, who may, to enhance themselves in the opinion of their employers and superiors, seek to introduce as large a number of those under their charge as possible, with little or no reference to their fitness or unfitness for the entertainment.

The exercise of various mechanical trades, employment out of doors in farming and gardening, and outdoor amusements of all sorts, are prosecuted in every asylum with much benefit and comfort to their inmates. Of the utility of such measures there is no difference of opinion, and in every report we find credit taken for so much work done, represented as worth a certain amount, and consequently diminishing by that amount the total cost of maintenance in the institution. In this matter, however, whilst on the one hand there may be neglect—as the Lunacy Commissioners complain of in one or two asylums—so, on the other, the labour of patients may be too rigorously enforced. Employment is most desirable for the insane, and it is equally desirable to keep down the rate of cost of maintenance in the asylum; still the primary object, the cure of patients, must ever be kept in view, and neither should their minds be irritated nor their bodies overtaxed by the rigorous exaction of work. Such evil is most to be apprehended in large asylums, and especially in the case of quiet and meek patients—a class certainly not infrequently imposed upon—for, as in other matters, much must be left to inferior officers and attendants where the supervision of individual patients is rendered impossible to the medical officer by reason of their number. At the North Riding of York Asylum employment is made the paramount business of the institution. This is a mistake, and the Lunacy Commissioners have at various times drawn attention to it, by pointing out its prejudicial influence upon the general management of the asylum and the welfare of the patients where such a practice prevails.

From the ardour with which employment is pushed in many asylums, particularly where an economically disposed committee aim at reducing the cost of maintenance to the lowest point as a primary object of their administration, there is danger that such institutions may degenerate into literal *workhouses*, whose inmates will be regarded rather as so many dependents from whom as much as possible of their cost must be reimbursed by their labour than as the victims of disease brought together to be cured or relieved. We are indeed ready to admit the existence of a majority of chronic and in all probability incurable cases in every asylum, for whom, generally, employment is the very best thing; still we repeat that, if made too much of—if it become the ruling principle in an asylum—the interests of recent and curable cases will probably be sacrificed to it.

There is another direction in which the pursuit of economy in asylums appears to us to operate prejudicially—that is, on the medical officers and the visiting justices, reacting mutually on the two. The superintendent who rates employment too highly, who pushes it as far as he can and vaunts its pecuniary value, fosters the belief among his committee that the physician's duties are secondary, and those of the manager and director paramount, and that if opportunities for useful and healthy occupation are afforded, along with proper hygienic and dietetic arrangements and means for amusement, the inmates of the asylum can do with just that amount of medical oversight as shall afford a sufficient security in case of accident and casual disease. Under such circumstances the physician depreciates his calling, and damages the interests of the insane as the subjects of disease demanding the study and assiduous care of a medical man. This fallacy we would indicate farther reacts upon committees in making choice of superintendents for asylums, and in their valuation of the services of medical men. A committee which has learned to look upon employment as the chief object and end of asylum administration, and as of paramount importance in the treatment of the insane, will come to regard the functions of their superintendent as a physician as being of very secondary importance to those he is called on to exercise as the general controller and manager, and will therefore, in selecting one to supply the office, look rather to the qualifications of a candidate as a judicious manager, a competent agriculturist, and generally as an able organizer and promoter of employment, than as a medical man who has an intimate acquaintance with insanity, and who, whilst not neglecting moral treatment, makes medical treatment and accurate medical supervision the objects of his special study. It is this same over-estimate of the value of employment and of routine organization that has led to the depreciation of the value of the services of medical men to an asylum, as instanced by the inadequate rate of remuneration paid them in some institutions compared with that given to the non-professional officers, as the steward and matron, and still more by the allotment of such large numbers of patients to the care of one medical superintendent—a feature in English asylums animadverted upon by the psychologists of every country in Europe as monstrous and absurd.

It is impossible to present anything like a summary of the miscellaneous medical jottings, observations, and suggestions scattered through the many reports of asylums now before us. We have attempted to sketch a general outline of the progress and present state of opinion respecting the management of the insane in asylums, but few medical superintendents have struck out for themselves any line of research and followed it up in consecutive reports, so as to furnish us with materials for an abstract of their views on any points in the pathology and medical treatment of insanity, or indeed in the management of the insane. The subject-matter of the reports of the medical superintendents generally is heterogeneous, composed of scraps of opinion and of observation, of suggestions for improved treatment and management, of pathological inferences, of notes on medico-legal and legal questions and cases; but all such, and mostly valuable, hints and notices are isolated, without continuity or relation with the contents of subsequent reports, fired off like random shots of which the results and consequences need not be further inquired into. There is in the whole matter of these reports an immense and deplorable waste of medical talent, observation, and experience, and it would be a highly advantageous proceeding for the authorities of the "Association of Medical Officers of Asylums" to promote some scheme for more harmonious and useful reports, and in the meantime to attempt a sort of digest of those annually issued in the pages of the excellent journal now published under their auspices. As things now are, there is no system of reporting; the passing incidents of the year, trivial or otherwise as they may be, so far as the progress of psychological medicine is concerned, are strung together and commented on without arrangement and without reference to any course of inquiry. In the case of some asylum reports, the barrenness and lack of interest are remarkable; and even in the majority the earnest student of his profession will deplore the absence of such contributions to the progress of medical science as might rightly be expected from the ample fields of study and research which the asylums of this country offer.

The general physician who looks on, and is desirous of learning what he can of cerebral pathology, must on the whole be surprised at the meagreness of the contributions to that subject which have come from the medical officers of our lunatic asylums, notwithstanding their unrivalled opportunities for studying diseases of the cerebro-spinal system in all their varieties, inasmuch as insanity, in most instances, is associated with such lesions; and still more, if he looks to the progress realized in what falls especially within the scope of the special practitioner in lunacy, in what relates to the medical treatment and pathology of insanity, what material additions can he find to have been made by the whole body of medical superintendents in England? Esquirol's great work, 'Des Maladies Mentales,' written forty years ago, must still be esteemed the text-book for the student of insanity on every point except on the moral treatment of the insane, which, thanks to Dr. Conolly especially, has been developed in our own country from the germs supplied by the great Pinel and his follower Esquirol. Though epilepsy is so rife in our asylums, we know of no researches contributed by English asylum physicians to take their stand alongside the works of general physicians; and, although the prevalence in our asylums of the general paralysis of the insane is a circumstance much commented and deplored on in reports, we have to go to foreign physicians—to Calmeil, Falret, Salomon, and others—for our knowledge of its pathology. To notice one more field of research: how almost entirely uncultivated has been that of the relations of the brain to the mental powers, of the brain-mass to its several segments, and of the brain to its bony case? Yet, with the unparalleled opportunities which asylum physicians possess for such study, we are at a loss to point to any substantial contributions made by them regarding it, but have again to turn our eyes to the labours of Wagner, as contained in his 'Vorstudien,' and other foreign physicians.

Whence this barrenness? the reader will ask. For an answer we may refer him to the observations on a preceding page relative to the position and requirements of an English asylum superintendent. The English system, in short, is to place a medical man at the head of several hundred patients, to charge him with their medical supervision and general government, with the control and direction of a host of servants, with a mass of official book-keeping, registering and correspondence, with much of the domestic economy of the establishment, with the general oversight of the whole premises and estate, and with as much agricultural and horticultural overseership as can be got out of him, or as he is willing to do. The result is, the study and treatment of the patients as individuals is rendered impracticable;

pathological researches and contributions to the advance of medical science are only to be effected by a race against time; and in general a tendency exists to the gradual and progressive conversion of the superintendent into a steward and farmer, the medical character subsisting enough to mark the cross-breed between those useful callings and the doctor.

Our preceding remarks must be accepted generally, for there are happily some exceptions, among which we may cite the reports of the Somerset County Asylum, by Dr. R. Boyd. This excellent physician divides his reports into two parts, the first being devoted to general matters occurring in the management and condition of the asylum during the year, the second to medical notes and reflections. The report is not of that irregular, desultory character noticed in most such productions, but its writer has, year after year, followed up certain observations, especially with regard to epilepsy and general paralysis. For some years past, we find tables of the age of epileptics and of the frequency and duration of their fits, both by night and day, and therein possess a collection of facts available to the student of the phenomena of epilepsy, and actually turned to account in the recent work of Dr. Reynolds on that malady. So again, with regard to general paralysis, Dr. Boyd has perseveringly examined, not as usual, the brain only of patients dying from this disease, but also the spinal cord, and has arrived at the conviction that this terrible disease is connected more particularly with inflammatory lesion of the meninges of the cord. The notes of the post-mortem changes are carefully tabulated, and the useful plan of adding a summary is adopted.

We have singled out Dr. Boyd's reports, because they come nearest to what such documents should be. They present a systematic arrangement of their contents, and the medical reader can at once turn to the purely medical details without having to look over pages informing him of trivial incidents in the establishment, such as the fixing of a new boiler, the hiring of an additional laundrymaid, or the discharges of inefficient attendants. They moreover are valuable, as before said, by annually following up certain lines of research, and thereby serving as repertories of facts.

It may be objected that we mistake the object of asylum reports; that they are not addressed to the profession principally, but primarily and especially to the committees of visiting justices, who require to be informed by their superintendents of all those minor details in the everyday operations of the institutions, and have no concern with medical observation and inquiry. If these be their only object and purpose, it would save much cost to the county rate-payers, if such reports were never put in print, but were simply entered on the written records of the establishments. As it is, we apprehend many reports conceived in this spirit make a rapid transit from the printers' shop to the waste-paper basket. But if reports of superintendents are especially, or all but exclusively for the benefit of non-professional committee-men, why broach medical topics at all, and comment upon diseases, their causes and remedies—a course pursued in all the reports we have seen. If such matters are entered upon, they should be discussed for the information and benefit of medical men.

Such views, however, of the fitting character of asylum reports ought not to obtain among a class of enlightened medical men, like those found in the asylums of this country. It would be a crying evil were no fruit gathered from the wide fields of observation and research existing in our great institutions for the insane. We have felt compelled to reflect on the little they have hitherto yielded, but we still hope to witness a moderate harvest, even if asylum politics and organization continue on the same model as hitherto. And it occurs to us, that if Dr. Boyd's plan of separating the medical from the general report be adopted, and if this separation be carried so far as to print off the medical part as a distinct report, there would be some encouragement to asylum physicians to give their professional brethren an annual history of their observations and experience in a convenient form, and in one likely to be perused and preserved. Some of the Scottish asylum reports have had this purely medical character; we may instance those of Dr. W. A. F. Browne, one of the present Lunacy Commissioners for Scotland, issued annually during his residence as physician of the Crichton Asylum, near Dumfries; and those of Dr. Skae, of the Royal Edinburgh Asylum.

The objection to this separate issue on the score of expense cannot seriously be put forward, when we find no stint in the amount of printing appropriated by the committees and allotted by them in some asylums, such as those of Middlesex, where the cry of economy is always in vogue, to reporters of all sorts, matron, chaplain, architect, engineer, solicitors, &c. Besides, in some asylums the printing is done on the premises, and the cost therefore much reduced.

The reference to those other contents of asylum reports as published makes some notice of them necessary. The report of the medical man is not always the chief feature, at least as regards length, for there are others connected with asylums who like to appear in print. The committee has a distinct legal duty annually to make a return respecting the population, receipts, expenditure, and requirements of the institution to the county magistrates in General Quarter Session assembled; and as may be presumed, there is a vast difference in the manner in which this duty is performed by the several committees, as well as in the conception formed by them of their duties in the management of the asylums. The varieties of committee-men may be reduced to two categories—the one satisfied to conform with the legal course laid down for them, and to watch over the interests of the ratepayers, as exemplified in the reports of the Derby, Devon, Somerset, and most other asylums; the other aiming at being lords paramount in everything, medical and general, by whom all ameliorations and improvements, both of buildings and patients are, according to their own version, effected. With the first described, the medical officer is actually the superintendent; with the latter, he is an inferior officer to be directed by the committee. Did space permit, we might quote many illustrations of these two classes, and of the second particularly.

Besides the reports of the Committee of Visitors and of the superintendent, there are others introduced in most asylum reports, the most constant and general of which is that of the chaplain. The medical officers are superintendents in name, as required by Act of Parliament, but it is evident that the committee charge themselves with many of the functions of their office, and that they regard the medical men as incapable of, or not to be trusted with, the superintendence of the asylum, and therefore delegate other of their duties than they themselves assume to *employés* directly responsible to them. Thus, for example, the medical officer of the female division is found having a divided authority with the matron, and the latter, instead of constantly looking up to and informing the superintendent of all that falls under her observation in matters within her own province, according to the rule in every well-ordered asylum, is directly accountable to the committee, and therefore presents her annual report for the edification of the county magistrates in Quarter Session assembled, and of all other her Majesty's liege ratepayers concerned.

An elaborate series of statistical tables is comprised in all the asylum reports, but in some of more extensive range than in others; some of these tables set forth the numbers admitted, discharged, and dead during the preceding year, and those remaining of each sex; and the condition of those discharged, whether cured, uncured, or relieved, and show these movements for each month; others are tables of age on admission, and at death or discharge; of the social condition and religious persuasion and occupation of those admitted; of the duration of their disease and of the number of attacks they have had and the form of insanity assumed. A table of mortality during the year, exhibiting the cause of death, a table of recoveries indicating the form of insanity recovered from, and the duration of the attack, and a table of assigned causes of the malady are also found in most of them. Among the most valuable tabulated summaries are those of the post-mortem appearances found on dissection, given, we regret to find, in only some few asylum reports; among which we may mention the Somerset, Worcester, Norfolk, and, in a more condensed manner, the Oxford and Gloucester. A collection of such records of autopsies, carefully and minutely made for a series of years, ought to add much to our acquaintance with the morbid anatomy and pathology of mental disease.

The various medical statistical tables enumerated are most of them calculated to convey much information, and we would commend to those who prepare them the advantages that would accrue from making an annual summary of the results deducible from some at least of them, as well as from a more general summary made at intervals of five or ten years.

To some of the tables, indeed, as those of "causes assigned," little value attaches, by reason of the difficulty, and in a multitude of cases the practical impossibility, of discovering a satisfactory and adequate cause, either from the total absence of information from relatives well acquainted with the history of the patient, or from the jumble of incidents which the several relatives or friends have to recount, and the various prepossessions and prejudices which occupy their minds. Some approximation may be, indeed, made towards discovering in many instances, the operation of moral causes as distinguishable from physical causes. Again, the tables of the duration of the attack, of the number of attacks, and, in fact, of most of the particulars falling under the head of the history of the cases, must be received with much allowance and often misgiving. Dr. Sankey commented upon the difficulties experienced in obtaining reliable information on the subjects in his report for 1861. He writes (p. 35): "It is provided by the Act that certain particulars connected with the medical history of the patient should be given with the order for the admission. This information is to be obtained by the relieving officer, who seldom takes such a scientific interest in the matter as to weigh with any nicety the truth of the particulars reported to him, and the consequence is that often great inaccuracies exist in the statements thus made." These remarks he illustrates by examples falling under his own observation.

Dr. Hitchman, of the Derby Asylum, has remarks in his first and third reports to the same effect, and so impressed does he seem to be with the valuelessness of such tables, that he compiles none of the sort.

On looking through the statistical tables presented by the officers of the various asylums, the glaring defect becomes obvious that there is no uniformity among them as to the manner in which they are drawn up and the facts tabulated. Each superintendent appears to pursue his own course both in the tables he publishes and in the manner in which he compiles them. Hence a comparison of the statistics of the different asylums on any question which it is desired to elucidate becomes difficult or impossible. Dr. Lockhart Robertson, of the Sussex Asylum, has drawn up two elaborate tables, one showing the history of the discharges, the other presenting an obituary for the year; each detailing most of the facts in the history of the cases from the date of admission to that of the discharge or death, enabling the reader to gather at a glance the main incidents in the course of each single case which leaves the asylum. Similar tables for each asylum would constitute a very valuable collection of facts. We would commend this whole question of the matter and manner of asylum statistical tables to the consideration of the members of "the Association of Medical Officers of Asylums," with the hope that a uniform scheme may be decided upon.

Our account of the contents of Asylum Reports would not be complete without a notice that there are various other statistical tables, of greater or less interest, particularly to asylum superintendents, setting forth the dietary of the institutions, the amount, nature, and value of the work done by patients; the provision, clothing, and farm and garden accounts, together with the balance-sheet of the receipts and expenditure. The above reference to these tables is sufficient in this place.

Our readers will gather from the foregoing remarks that, though there is much useful information contained in the reports annually issued from the fifty or more county asylums of this country, it is little available and for the most part sacrificed in waste. Moreover, these reports, considered as the selected vehicles of the superintendents for recording their observations and opinions, cannot be considered satisfactory records of the medical experience and advancement in professional knowledge that should accumulate and flow from a number of such magnificent institutions for the sick as our English lunatic asylums.

Review XII.

Clinical Memoirs on Diseases of Women. By Alfred H. M'Clintock, M.D., late Master of the Dublin Lying-in Hospital, &c.—*Dublin*, 1863. pp. 935.

There is, perhaps, no obstetric hospital which has yielded more extensive and more valuable contributions to the clinical history of midwifery proper than the Dublin Lying-in Hospital. Dr. M'Clintock himself, who has passed eleven years of active and earnest work in that institution, has added largely to the rich stores of obstetric knowledge extracted from this inexhaustible mine. But the work now before us is, we believe, the first fruit of the working of a vein, scarcely less valuable than the pure obstetric one—namely, the department called gynæcological, of this hospital.

The book well deserves its title. It is eminently clinical. The skilled practitioner will recognise in its descriptions the faithful representations of diseases he has himself observed, interpreted by a mind singularly sagacious, calm, and free from prejudice. The student, on the other hand, on reading this book, may almost imagine that he is walking from bed to bed, observing typical cases under the immediate guidance of an accomplished clinical teacher.

The scope of the work does not embrace a complete systematic account of the diseases of women. It is, indeed, much more full and serviceable in this respect than some books professedly more comprehensive. But in strict fidelity to its title, the work is simply a series of clinical memoirs or monographs, in which particular subjects are illustrated by cases and commentary. Among the subjects so treated are: puerperal and non-puerperal pelvic cellulitis, procidentia and inversion of the uterus, fibrous tumours and polypi of the uterus, tumours of the vagina and vulva, pelvic hæmatocele, pudendal and uterine hæmatoceles, stone in the bladder, mammary inflammation and abscess, secondary hæmorrhage after parturition, the semeiological value of the pulse in childbed, dropsy of the ovum, and cystic disease of the ovum.

Upon all these subjects the author brings to bear a large amount of clinical illustration. Eminently qualified by the wealth of information at his command to teach from experience, he never bewilders his readers by the vain speculations which men whose knowledge is not equal to their ambition to shine as instructors are apt to indulge in.

Puerperal pelvic cellulitis Dr. M'Clintock says, though sometimes very exhausting to the patient's strength, very rarely ends fatally. Of 70 cases which came under his care, he only knows of 2 which so terminated. This is, no doubt, generally true, but we have known cases in which death was brought about after many months, and even years, of protracted suppuration. The author rightly draws attention to the insidious manner in which the pelvic inflammatory tumour steals its way; and he lays it down as a very important rule in all cases of convalescence from uterine or peritoneal inflammation, to examine the iliac regions day by day as regularly as to feel the pulse. The course and symptoms of the affection are very clearly traced. The author insists with much earnestness upon the paramount importance of rest in the treatment.

Dr. M'Clintock prefers to consider pelvic inflammation and abscess in the non-puerperal state in a distinct memoir. He says this inflammation may present itself under two forms—a primary or idiopathic form, and a secondary or symptomatic. The former is the more rare; it is more apt to have a distinct, if not an acute mode of invasion. In a large proportion of cases the inflammation is consecutive upon some uterine or ovarian disease, or abdominal inflammation. One case, he says, followed the use of an intra-uterine stem-pessary. We have also seen the same result from the same cause. The inflammation does not necessarily proceed to suppuration. An interesting case is related, in which pelvic abscess and death followed the ligature by Gooch's canula of a uterine polypus. The author observes, that whilst three cases of non-puerperal pelvic abscess were brought to a sudden termination by bursting of the sac into the peritoneal cavity, no such accident ever occurred in his experience of pelvic abscess succeeding to parturition. He differs from certain French authors, who are of opinion that the puerperal disease is more dangerous than the non-puerperal. Cases related in this memoir tend to establish the existence of a danger connected with non-puerperal pelvic inflammation, almost unknown to the allied disease of the puerperal state. This danger is the greatest in the secondary or symptomatic form of the complaint when it presents itself as a complication of organic disease of the uterus or ovaries, and this consists in the liability of the abscess to burst into the peritoneal cavity.

Much valuable practical information will be found in the memoir on procidentia uteri, and the use of pessaries. The drawings illustrating the various forms and conditions of this displacement are especially profuse and instructive. In these days, when pessaries are absolutely condemned by some, and still, perhaps, unduly extolled by others, the moderate and rational practice of the author must exercise an useful influence in bringing us back to a correct appreciation of the value of these instruments. Many of our readers, however, would not assent to the use of the globe-pessary, which appears to act on a vicious principle, destroying all prospect of recovering the tone and contractibility of the vagina, and occasionally leading to serious mischief. Dr. M'Clintock, indeed, refers to a case where the os uteri became incarcerated in the aperture of a globe-pessary; and this accident is especially likely to happen with the various forms of rings.

Some most interesting clinical histories illustrate the subject of inversion of the uterus. Dr. M'Clintock's experience seems to incline him to prefer amputation of the inverted uterus in obstinate chronic cases. This operation has been performed eight times in Dublin, in every case with perfect success.

A full chapter is devoted to fibrous tumours of the uterus. Dr. M'Clintock speaks well of the hæmostatic property of Indian hemp, and gives examples of the efficacy of free incisions of the os and cervix uteri in restraining the profuse bleedings that sometimes attend the presence of fibrous tumours in the uterus.

In the treatment of uterine polypus, the author confirms by his experience the practice now we believe generally adopted, of immediate removal by the écraseur. No other application of this instrument has given such striking and satisfactory results. The superiority of this operation over the method by strangulation and sloughing by Gooch's canula is completely established. Dr. M'Clintock thus sums up the objections to the system: 1. Death may occur before the ligature has cut through the neck of the polypus, even in cases where this event has not been unusually delayed, and where no part of the uterus has been imbedded in the ligature. 2. In some cases a fatal termination has been brought about by the supervention of peritoneal inflammation. 3. Uterine phlebitis, ending in death, has succeeded the application of the ligature. 4. In not a few instances the fatal issue has apparently been attributable to a kind of low toxæmic fever, or constitutional irritation. 5. Pelvic abscess, with its attendant dangers, is a result to be apprehended from the operation of ligaturing uterine polypi. 6. Phlegmasia dolens of one or both legs may follow. 7. It would seem possible for a patient to be carried off by tetanus.

We find a good example of what Rokitansky and Braun have described as fibrinous polypi. A young woman, twenty-four days after labour, expelled a very dense, firm coagulum, being a perfect cast of the cavity of the uterus, having at its superior angles projections corresponding to the orifices of the Fallopian tubes.

The memoir on tumours of the vagina is altogether unique as a monograph on this subject. The information we possess concerning it is scanty, and widely scattered. Until recently, indeed, the existence of vaginal fibrous tumours and polypi was hardly recognised. The author relates the histories of three cases of fibrous tumour springing from the vagina. He also gives two cases of cystic tumour of the vagina. We agree that many of these vaginal cystic tumours originate, as Huguier says, in obstructed mucous follicles. But it is important to distinguish the seat of the tumour. It is probable that the explanation of Huguier applies more strictly to the tumours of the vulva, and that the cysts sometimes, but very rarely, found at higher points of the vagina, originate in a different manner. A case of this latter kind is described by Dr. Barnes.* The cyst sprang from the anterior wall of the vagina near the cervix uteri. In another case, related by the same physician, the tumour appeared to be formed by a distension of the duct of Bartholini's gland. Both cases were successfully treated by puncture, excision of a portion of the cyst wall, and application of tincture of iodine to the remaining portion of the cyst cavity.

A separate chapter is devoted to the study of tumours of the vulva. Some excellent drawings, exhibiting forms of syphilitic hypertrophy of the nymphæ and adjoining structures are given. The important question of the distinction between growths of syphilitic origin and of cancerous nature is discussed with the care it deserves. Having observed that in several cases of undoubted syphilitic character the tumour was deeply fissured and tuberculated, whereas in cases where there was no ground for suspicion of syphilitic taint the surface was only rough and corrugated, Dr. M'Clintock puts the proposition suggestively, that we may in this difference possess a diagnostic mark. Since the syphilitic hypertrophied masses bear removal by the écraseur with the best results, the importance of being able to distinguish cases so curable from those of cancerous nature is obvious. We therefore invite the attention of gynækologists to this conjecture of Dr. M'Clintock's, with a view to the verification of its accuracy.

In discussing the treatment of pelvic hæmatocele, we are glad to find the author expressing himself as adverse to the practice of puncturing the hæmatic cyst.

A very practical chapter is devoted to pudendal and uterine hæmatoceles. Dr. M'Clintock differs from most writers, who have supposed that a varicose state of the vessels of the vagina or vulva is a necessary or very constant precursor of the pudendal form of this accident. He says that, out of thirty-eight cases, he found only two in which a varicose state of the veins was noted. On the other hand, we think it cannot be denied that the extreme hyperæmic condition of the vagina attending pregnancy is the essential predisposing cause. The accident is considered as it occurs: 1st, before the commencement of labour; 2nd, during the progress of labour; 3rd, upon the termination of labour. An interesting case is given in which a pudendal hæmatocele occurred spontaneously in the seventh month of pregnancy. The tumour burst, discharged a quantity of coagula, and disappeared. The labour did not take place till some weeks later.

A pudendal thrombus, the author says, may begin to form *during labour*. This fact, and the spontaneous occurrence of the accident during pregnancy before labour, are sufficient to show that direct violence from the pressure of the child's head is not necessary to produce extravasation of blood. In the largest proportion of cases, however, the accident is first discovered within a short time after the birth of the child. Dr. M'Clintock is not favourable to the practice of

* Medical Times and Gazette, Dec. 1862.

opening these tumours, at least during the acute or formative stage. He next describes "that very rare variety of thrombus, in which the lip or lower part of the cervix of the womb is the seat of extravasation." To such cases he restricts the term "uterine hæmatocele." The occurrence of this accident was noticed by Denman. We suspect that it is more frequent than is supposed. If it is not more often detected, it is because the cervix uteri, the seat of it, rarely comes into sight during or after labour; and two cases, in which he observed very extensive extravasation of blood in both lips, recovered without a bad symptom; so that, unless the hæmatocele had been actually *seen*, the existence of the accident might not have been suspected. Dr. M'Clintock, however, records two fatal cases. He says that the hæmorrhage which occasionally takes place in the second stage of labour may proceed from a ruptured thrombus of the uterine lip; and further, that the rupture of one of these uterine hæmatoceles may give rise to troublesome or dangerous post-partum hæmorrhage. He agrees with Dr. Montgomery in thinking that the post-partum hæmorrhage, with firm contraction of the uterus, described by Gooch as arising from undue excitement of the vascular system, is in reality to be accounted for by the rupture of a uterine thrombus. Dr. M'Clintock is fairly justified by the facts and observations he has brought forward in stating that the subject of uterine hæmatocele is one of extreme interest, even in a practical point of view, and that it has a close bearing upon three of the forms of hæmorrhage incident to childbirth—viz., hæmorrhage in the second stage of labour, hæmorrhage immediately succeeding parturition, and secondary hæmorrhage, occurring some hours or days after labour.

The memoir on mammary inflammation and abscess is one eminently deserving study. It will serve to correct many prevalent errors as to the causes and treatment of this affection. He in this says a very large proportion of his patients had some form of sore nipple previously to the occurrence of inflammation of the gland. He does not, however, regard abrasion or fissure of the nipple as the only cause of mastitis, but cites for the purpose of condemnation the still popular notion that retention of the milk, and the consequent distension of the breast, causes the inflammation in almost every instance. He expressly declares that he has rarely known inflammation and abscess to result after delivery from distension of the breast alone; neither does he recollect a single instance of mammary abscess in a woman who did not nurse at all, nor supervening after the death of a nursling, where no other exciting cause of inflammation was present. We may state for ourselves, that we have several times seen inflammation produced in women who did not suckle; but then there was the "other exciting cause," which Dr. M'Clintock refers to, supplied by assiduous sucking at the breast by the nurse, or by the use of the breast-pump. In fact, here, as in so many other cases, the great principle of "rest," so well illustrated by Mr. Hilton, is the one paramount condition of prevention as well as of cure. All treatment should be directed to this end. And we are glad to see that Dr. M'Clintock's practice is in strict harmony with this principle. He does not believe in the efficacy of belladonna in arresting or averting the secretion of milk; neither does Mr. Nunn, who has written an excellent paper on abscess of the breast. Our own present experience entirely accords with theirs. With regard to the time for opening mammary abscesses, the author advocates the rule of *late puncture,* but insists upon the judicious qualification that the puncture should be made early enough to save loss of skin by mortification. In the treatment of abscess of the breast there is hardly anything more valuable in our experience than the method of compression advocated by M. Trousseau; but after using it extensively under a variety of circumstances, in the manner we learned from the French physician, we have arrived at the same conclusion as Dr. M'Clintock—namely, that it must be limited to the period when the abscess has opened and the surrounding inflammation has entirely subsided.

Mr. Roberton of Manchester and the author were among the first to call special attention to the important subject of "secondary hæmorrhage after parturition," and both have illustrated it by apposite clinical relations and sound medical criticism. In this volume Dr. M'Clintock has entirely re-cast his former paper published in 1851, without, however, materially altering the views he then expressed, which were too well founded on observation to require modification:

"The causes which may directly or indirectly bring about secondary hæmorrhage are:

"1. Disturbances of the vascular system.
"2. Retraction of the uterus.
"3. Retention of a portion of the secundines, or of a coagulum.
"4. A polypus of the uterus.
"5. Inversion of the uterus.
"6. The bursting of a uterine hæmatocele.

"Besides these there are some other causes, which on very rare occasions have operated in the production of secondary hæmorrhage—viz., *a*, sloughing of the vagina; *β*, constipation; *γ*, ulceration of the os uteri; *δ*, growths from the interior of the uterus."

Dr. M'Clintock assigns, and we think rightly, a foremost place to disturbance of the general vascular system, but he has, if our observation does not much mislead us, underrated the influence of ulceration of the os uteri. A series of clinical histories illustrates the various causes and forms of secondary hæmorrhage.

The volume includes a Memoir on the Semeiological Value of the Pulse in Childbed. The author's original researches on this subject were first published in May, 1861. His views derived strong confirmation from Professor Levy, of Copenhagen, who in the following year published an excellent monograph on the subject. The clinical value of the indications furnished by the pulse is admirably illustrated. Acceleration of the pulse is shown to precede the development of many grave affections, and thus oftentimes to furnish the only premonition of coming danger. Dr. M'Clintock pointedly says, even where no puerperal disease can be discovered to account for the symptom, a guarded prognosis should still be given, and a cautious expectant line of treatment should be adopted.

The work concludes with two interesting Memoirs on Dropsy of the Ovum and Cystic Disease of the Ovum.

In taking leave for the present of our author — we hope not for long—we feel bound to express our deep obligation to him for the instruction derived from the perusal of his work. We can confidently recommend it to our readers as a mine of information at once full, sound, and of enduring value.

Review XIII.

1. *On the Chronometry of Life.* By J. Paget, F.R.S. ('Proceedings of Royal Institution,' April, 1859 : Croonian Lecture before the Royal Society, 1857.)

2. *On Liquid Diffusion applied to Analysis.* By T. Graham, F.R.S. ('Philosophical Transactions,' 1862, and 'Journal of the Chemical Society,' 1862.)

3. *The Correlation of Physical, Chemical, and Vital Force, and the Conservation of Force in Vital Phenomena.* By J. Le Conte, Professor of Geology and Chemistry in South Carolina College. ('American Journal of Science and Arts,' No 28. 1859.)

4. *Vier Reden über Leben und Kranksein.* Von Rudolf Virchow.—*Berlin,* 1862. pp. 136.

Four Lectures upon Life and Disease. By Rudolf Virchow.

5. *Göthe als Naturforscher und im besonderer Beziehung auf Schiller.* Von Rudolf Virchow.—*Berlin,* 1861. pp. 127.

Goethe as a Man of Science, with special reference to Schiller. By Rudolf Virchow.

It has been the custom of certain disciples of the positive philosophy to repudiate as extravagant the well-known opinion of Protagoras, that man was the measure of the universe. If the proposition be understood of man as he is known to himself by the revelations of self-consciousness, there is unquestionably great reason for its rejection; but if it be applied to man as an objective study, it is manifest that modern science is tending to prove it by no means so absurd as it has been sometimes deemed. Day by day, indeed, is it becoming more and more clear that, as Sir T. Browne has it, man "parallels nature in the cosmography of himself," that, in truth, "we are that bold and adventurous piece of nature which he that studies wisely learns in a compendium what others labour at in a divided piece and endless volume."* The "heaven-descended γνῶθε σεαυτον" acquires new value as a maxim inculcating on man the objective study of himself.

* Religio Medici.

The earliest cultivators of Grecian philosophy—Thales, Anaximenes, and Diogenes of Apollonia—did seek objectively for the ἀρχη or first principle of things common to man and the rest of nature. This primitive kind of induction was soon, however, abandoned for the easier and speedier deduction from the subjective facts of consciousness; so that, as the German philosopher is said to have done with the elephant, man constructed the laws of an external world out of the depths of his own consciousness. Because an individual was conscious of certain passions which influenced his conduct, he fancied that natural bodies were affected in their relations to one another by similar passions. Hence the phenomena of nature were explained by sympathies, antipathies, loves, discords; oil had an antipathy to water; nature abhorred a vacuum; Love was the creative force which produced development and harmony—Hate, the destructive force which produced disorder and discord. The method was only a part of that anthropomorphism by which the Dryad was placed in the tree, the Naiad in the fountain, and the gods of mankind were created by man.

The result of such a method was inevitable. When in a language there is but one word for two or three different meanings, as happens in all languages before the cultivation of science—when, for example, the loadstone is said to attract iron, the earth to attract heavy bodies, plants to attract moisture, and one mind to attract another, without further differentiation—there necessarily is an ambiguity about words, disputes thereupon arise, and the unavoidable issue is sophistry and sophists. That was a result at which the ingenious and active mind of Greece soon arrived. In scientific nomenclature it is constantly necessary to discard words which are in common use because of their vagueness and want of precision; for as it is with life objectively, and as it is with cognition or life subjectively, so must it be with the language in which the phenomena are expressed. A scientific nomenclature must rightly present a progress from the general to the special, must reflect in its increasing speciality the increased speciality of human adaptation to external nature. As might be expected, Plato and Aristotle both recognised the evil in Greece, and both tried to check it. The metaphysics, analytics, &c., of the latter have been described as a dictionary of general terms, "the process throughout being first to discover and establish definite meanings, and then to appropriate to each a several word."* But it is in vain to attempt to establish words except as living outgrowths of actual facts in nature. The method was a mistaken one; there was not an intending of the mind to the realities of external nature, and knowledge wanted those

* Coleridge's Literary Correspondence. It is for this attempt, praiseworthy surely as far as it went, that Bacon is unduly severe upon Aristotle in some parts. Thus: "And herein I cannot a little marvel at the philosopher Aristotle that did proceed in such a spirit of difference and contradiction towards all antiquity, undertaking not only to form new words of science at pleasure, but to confound and extinguish all ancient wisdom."—De Augmentis Scientiarum. And again: "Aristotle, as though he had been of the race of the Ottomans, thought he could not reign except the first thing he did he killed all his brethren."—De Augm. Scient.

"fruits and invented works," which Bacon pronounces to be, as it were, "sponsors and sureties for the truth of philosophy."

Much the same thing happened in the earlier part of the Middle Ages. The mysticism and sophistry which then prevailed, the endless and unprofitable, but learned and ingenious, disputes about words which had no definite meanings, might be said to represent the wasted efforts and unavailing strength of a blind giant. But as the infant, moved by an internal impulse, at first unconsciously strives for its mother's breast and draws its nourishment therefrom, gradually awakening thereby to a consciousness of the mother who supplies it, so the human mind unconsciously for a time gathered the material of its knowledge from nature, until it was gradually awakened to a full consciousness of the fruitful bosom which was supplying it. The alchemist, moved by his avarice and the instinct of a unity in nature, and the astrologer, moved by the feeling of a destiny governing human actions, both lighted on treasures which, though not then appreciated, were yet not lost; for of astrology came astronomy, and from alchemy, in the fulness of time, was born chemistry. In Roger Bacon, who successfully interrogated nature in the spirit of the inductive method, we see the human mind instinctively and, as it were, unconsciously striving after the true source of knowledge; while in the Chancellor Bacon, who established the principles and systematized the rules of the inductive philosophy, we see the human mind awakened to a clear consciousness of the necessity of doing with design and method that which in an imperfect manner it had for some time been blindly aiming at. But as it is with the infant, so it was with humanity; action preceded consciousness, and Bacon was the efflux of a spirit which prevailed, and not the creator of it.

The method of investigation has accordingly been completely reversed. Instead of beginning with himself and passing thence to external nature, man begins with nature and ends with himself; he is the complex to which his investigations gradually ascend through progressively increasing complications of the simple. Not only so, but the necessity of studying himself objectively is fully recognised; it is not the subjective feeling of heat or cold in a feverish patient, but the figure at which the thermometer stands, that is appealed to as the trustworthy index of the real temperature. The development of the senses, or, in other words, the increased specialty of human adaptation to external nature, has been, as the progress of science proves, the foundation of intellectual advance; the understanding has been developed through the senses, and has in turn constructed instruments for extending the action of the senses.* The telescope has merely been a means for enabling the eye to penetrate into distant

* A great desideratum is a history of such development of the senses: Wir besitzen gar treffliche Werke über die Geschichte von Schlachten und Staatsformen, genaue Tagebücher von Königen und fleissige Verzeichnisse von den Schöpfungen der Dichter. Aber den wichtigsten Beitrag zu einer Bildungsgeschichte des Menschen in der eingreifendsten Bedeutung des Wortes hat noch Niemand geliefert. Uns fehlt eine Entwicklungsgeschichte der Sinne."—Moleschott, Kreislauf des Lebens.

space, and to observe the motions of worlds which the unaided vision would have never known; by the microscope the minute structure of tissues and the history of the little world of the organic cell has been made known; the balance has shown the indestructibility of matter, and has supplied to science the exactness of the numerical method; and, in the electric stream, there has been found a means of investigating nerve action, like that which there is in polarized light for ascertaining the internal condition of crystallized bodies. Who would have ventured to predict some time since that it would ever be possible to measure the velocity with which an impulse of the will travels along the nerves?* And who will venture to say, some may ask, that it will not at a future time be possible to measure the velocity with which one idea calls up another in the brain? Biology must plainly of necessity be the last and most difficult study, for it supposes the other sciences as vital force supposes inferior forces; but it is the evident tendency of advancing knowledge to bring life more and more within the compass of human investigation. And if it is sometimes made a reproach to science, as it was by Comte, that it has not discovered the laws of life, it may well rest calm under the censure, pointing to the history of the earth to show that nature having done all else, required a long period before it accomplished the evolution of life.

In spite, then, of a desire on the part of some to separate biology from the other sciences, and, notwithstanding the alarm occasionally displayed with regard to the dignity of vitality, it is the certain tendency of advancing knowledge to bring a science of life into close and indissoluble relations with other sciences, and thus to establish in cognition, or to reflect in consciousness, the unity which exists in nature. When, in ancient times, life was assigned to the stars, the air, the water, a certain unity was recognised, but recognised only by explaining nature from a very imperfect knowledge of man; now the task is to explain man on the basis of an increasing knowledge of nature, and in that way to demonstrate the unity of the whole. What must be the result? Nothing less, indeed, than the reconciliation of the ideal and the real, the identification of subjective and objective.† As life is a condition in which an intimate correlation exists between the individual and nature, it is evident that whilst Plato dealt only with the *ideas* of the mind, his system must remain comparatively unprofitable; but it is evident also that since we have learnt to discover the *laws* or *ideas* in nature of which the *ideas* in the mind are correlates, it

* Such an eminent physiologist as Müller could venture to predict the impossibility thereof. In his Physiology he says: "Wir werden auch wohl nie die Mittel gewinnen die Geschwindigkeit der Nervenwirkung zu ermitteln da uns die Vergleichung ungeheurer Entfernung fehlt aus der die Schnelligkeit einer dem Nerven in dieser Hinsicht analogen Wirkung des Lichto berechnet werden kann." With which compare Helmholtz: "Ueber die Methoden kleinste Zeittheilchen zu messen," &c. 1850.

† Some, who talk much of the Baconian method, reject any talk about "unity" as nonsense. Bacon, however, after speaking of the works of nature as "holy in the connexion or concatenation of them," and "holy in the union of them in a perpetual and uniform law," adds: "And, therefore, the speculation was excellent in Parmenides and Plato, although but a speculation in them, that all things by scale did ascend to unity."—De Augmentis Scientiarum.

becomes possible to find in nature a true interpretation of Plato's ideas. Once for all, it may be taken for granted that the ideas of genius never can be meaningless; for its mental life is a reflection in consciousness of the unconscious life of nature. How excellently has this been exemplified in him who embodied in poetical form the scientific spirit of this age! It was the great characteristic of Goethe, as Lavater justly said of him, to give a poetical form to the real; he proved, in fact, that science, in place of rendering poetry impossible, opened a field for the highest poetry. His Romance of the Elective Affinities (*Wahlverswandschaften*) starts from the chemical affinities of elements, and applies such affinities to human beings, therein exactly reversing the old method which, starting from the phenomena of self-consciousness, applied the passions of the human mind to the phenomena of external nature. It may be justly said of Goethe, that the ideal and the real were happily blended in him; that he embodied the scientific spirit of the age, and yet was in advance of it; that he represents that which must be the course of development of the human mind, if it is destined to be developed.*

The foregoing general sketch of the course and tendency of knowledge is fully justified by the present aspect of science. When nature was first objectively examined, the difference in matter appeared manifold, and its modes of energy or activity, that is, its forces, appeared many also. On a more careful use of the senses, however—in fact, by the application of the delicate balance to the products of combustion—it became evident that one form of matter only disappeared to reappear in another form; that it never perished, but only changed. Elementary matter thus passes upwards into chemical and organic compounds, and then backwards from organic to chemical, and from chemical compounds to its elementary condition. Out of dust man is formed by an upward transformation of matter, and to dust he returns by a downward metamorphosis thereof. Corresponding with the changes in the form of matter are changes in its modes of energy or its forces; to different combinations and arrangements of molecules correspond different modes of dynamism. Force therefore is eternal, like matter, and passes through a corresponding cycle of transformations. The correlation and conservation of forces, which have always been more or less clearly recognised as necessities of human thought, are now received as scientific axioms, and are daily receiving experimental demonstration.†

* When analytical industry rails at idealism, it is unquestionably right as regards itself; but it should remember that it is analytical industry, and not synthetical genius; that it is not, therefore, exactly qualified to judge idealism. How tiresome it is to hear so much said of the Baconian method by people who have clearly only comprehended a part of Bacon! What was his 'Philosophia Prima'? "But it is manifest that Plato, in his opinion of ideas as one that had a wit of elevation situate as upon a cliff, did descry 'that forms were the true object of knowledge,' but lost the real fruit of his opinion by considering of forms as absolutely abstracted from matter, and not confined and determined by matter; and so turning his opinion on theology, wherewith all his natural philosophy is infected."—De Aug. Scient.

† Epicurus, Democritus, Aristotle, all upheld the eternity of matter; the quotations from Lucretius and Persius on that subject are well known, but the following

Though it may seem difficult to avoid the conclusion that there is fundamentally but one force which manifests itself under different modes, yet such a supposition at present transcends the domain of science. As a matter of fact we are compelled, in order to form a satisfactory conception of matter and its forces, to regard it under a twofold aspect. In all our conceptions we imply a sort of dualism of power in every body. The hinges of gravitation, for example, keep worlds in their orbits by opposing a centrifugal force which would otherwise drive them afloat into space. The smaller hinges of molecular cohesion retain the infinitely smaller bodies which we call molecules of matter, in opposition to a repulsive force, which, on the application of a little heat, may drive them off into space, and in volatile substances does so drive them off without heat. It is the same with liquids; their diffusion power is similar in character to the volatility of solids; while "colloïds" are volatile, "crystalloïds" are comparatively "fixed." There is a relation of molecules to one another which we are compelled to represent in conception as the result of a force of repulsion or tension. And as some sensible image is necessary to the clearness of a conception of the invisible, physics assumes between the ponderable molecules of a body certain ethereal particles which are in a state of stationary oscillation, the degree of temperature of the body being supposed to depend upon the intensity of the active force of these imponderable intermolecular particles. If the body be suddenly and greatly compressed, these motions are communicated to the imponderable ether outside the body, and tension force thus becomes free force in manifest radiation of heat. "What is heat in us," very justly said Locke, "is in the heated body nothing but motion." When heat is withdrawn from matter—that is, when the tension force becomes free, its molecules get nearer to one another, their cohesion is greater; thus vapours become liquids and liquids become solids.

It seems probable that the necessity of regarding matter under this twofold aspect of attraction and repulsion is owing to man's inability, as being himself a part of nature, to form a conception of nature as a whole. He must necessarily regard things in relation to himself; for as he exists only in relation to nature, and as every phase of consciousness is an expression of this relation, it is plain that one of the elements of the relation cannot free itself, and from an independent point of view watch unconcernedly things as they really are. Thus, though we speak of passivity and activity, they are really not different kinds of action, but different relations of the same kind of action.* Whatever be the cause, and however doubtful the philosophical validity of the

passage from the 'De Augmentis' is not so common: "All things change, but nothing is lost. This is an axiom in physics, and holds in natural theology; for as the sum of matter neither diminishes nor increases, so it is equally the work of Omnipotence to create or to annihilate." And the Brahminical doctrine is as follows: "The ignorant assert that the universe in the beginning did not exist in its author, and that it was created out of nothing. O ye, whose hearts are pure, how could something come out of nothing?"

* Locke, in his chapter on "Power," divides force into passive and active force, for

distinction, we are compelled to regard matter in a statical and a dynamical relation. One aspect of the relation we describe as passive, statical, cohesion, or, to use the generic term, attraction; the other is active, dynamical, tension, or, to use the generic term, repulsion. Attraction plus repulsion of molecules constitutes our conception of matter; and in observation of its modes of energy, attraction is recognised in gravitation, cohesion, magnetism, while repulsion is found in the centrifugal force, in heat, and in electricity.

In rising to the higher department of chemical compounds, attraction is found under a new and special phase as chemical affinity. But when the chemical union of two molecules into a single one takes place, a diminution of the tension-force surrounding each molecule must occur, and according to the law of the conservation of force, an equivalent of another force must be set free. This happens in the production of heat and electricity; for, as Faraday has shown, no chemical action can ever take place without the development of electricity. The amount of force liberated in a simple chemical combination will be the equivalent of the tension-force lost. When one atom of carbon combines with one atom of oxygen, a definite quantity of tension-force surrounding each molecule disappears, and a definite quantity of heat is accordingly produced. When two molecules separate in chemical decomposition, they necessarily make passive or latent so much active force; so much heat becomes so much tension-force. But furthermore, in a chemical decomposition we have the resolution of that very intense and special force, chemical affinity itself; so that the force set free will far exceed that which becomes latent as tension-force around the molecules. We know not why two molecules should chemically combine; we accept as a fundamental law of their nature this high, special, and powerful form of attraction; but we do know that when chemical decomposition takes place a little chemical force must be resolved into a large manifestation of inferior force. It is a fact authenticated by Faraday, that one drop of water contains, and may be made to evolve, as much electricity as under different modes of display would suffice to produce a lightning flash. The decomposition of matter is the resolution of force, and one equivalent of chemical force will correspond to several equivalents of inferior force. Thus chemical force, though correlated with the physical forces, may be said to be of a much higher order than they are.

In the still higher stage of vitality, chemical combination of a much more complex character than in inorganic matter is met with; attraction appears under its most special and complex form. Matter, which

which Reid rashly ridiculed him. Sir W. Hamilton has shown that the distinction is a very proper one; that it is not absurd to speak of passive force. So also Coleridge in his 'Literary Correspondence,' and Mr. J. S. Mill in his 'System of Logic,' vol. ii.

Dr. Boase, in his 'Philosophy of Nature,' maintains the real dualism of powers in all natural bodies. "Our case," he says, "is now before the scientific world; we repudiate positivism and the identity of forces, maintaining that all natural bodies are dualisms of power, and that each power is a reason-directed force." (p. 358.)

in its elementary condition might occupy some space, is so welded or combined as to occupy a minimum of space, and force which, under a lower mode, might suffice perhaps to illuminate the heavens, is here confined within the small compass of an organic cell. We have to do, however, with organic matter under two forms, as dead and as living matter, as displaying energy of its own, or as displaying no energy. Dead organic matter has ceased to act, and it is now acted upon; it is at the mercy of the forces which surround it, and immediately begin to effect its dissolution. Heat hastens such decomposition, because in the separation of its molecules into the ultimate products, carbonic acid, ammonia, and water, a certain amount of active force must become latent as the tension-force of these molecules. There is also the force of the chemical affinity of the oxyyen of the air for the oxidizable elements of the substance; and the combination is necessarily attended with the production of heat. The heating value of organic matter will accordingly increase with the quantity of oxidizable elements; but the matter is by no means so simple as it has sometimes been assumed to be. Suppose the atom of carbon with which an atom of oxygen combines was previously in combination with, for example, an atom of hydrogen; and the question is, whether the amount of heat produced will be the same as though the atom of carbon had been free? In reality it will not; it must be less, because in the separation of the carbon atom and the hydrogen atom so much active force must become tension-force—that is, so much heat must disappear or become latent; and that loss of heat will necessarily do away with a part of the heat produced, or the decrease of tension-force which occurs through the combination of the atom of carbon with the atom of oxygen. It is this fact which appears to invalidate the experiments of Dulong and Despretz on animal heat.

But there is another consideration. In this mere burning or decomposition of organic matter, or that which represents the passive, statical, or attractive phase of vitality, the active force which results is due partly to force from without, and not solely to the liberation of force latent in the matter. External forces have, as it were, been pulling it to pieces. What, then, on the principle of the conservation of force, becomes of that intense chemical force which is implied in the organic nature of the material, that power which holds it together as a specific material differing in properties from all kinds of inorganic matter? Though dead, the chemical composition of organic substance is the same as when alive; and its future destiny is entirely dependent on the circumstances in which it may be placed. In the air, it is true, it will undergo decomposition into inorganic products; but if it be surrounded with the conditions of vitality, if it be exposed to the influence of higher forces by being given as food to some animal, it does not go downward, but upward, and somehow takes on life again. It is plain what becomes of the statical force under the latter circumstances. But in the decomposition of organic matter in the air and the correlative resolution of force, it is not so evident what becomes of all the force which must be liberated. That it returns to general nature can admit of no doubt; but does it all appear as heat? A part of it must necessarily become latent as the tension-force of the molecules of the ultimate products of its decomposition, and the rest is liberated under some form or other, if not entirely as heat. There is some reason to believe, however, that dead organic substance does not always undergo the extreme retrograde metamorphosis of material and of force before being used up again in vital compounds, even by the vegetable kingdom. It has been shown that not only do pale plants, such as fungi, feed on organic matter, but that soluble humus is regularly taken up by the roots of almost all plants. Professor Le Conte has shown it to be probable that the decomposition of the organic matter supplies the force necessary for raising other matter from a lower to a higher stage. The force necessary for organization is thus furnished by the force which results from disorganization; death and destruction are the conditions of life and development.

When organic matter displays energy—that is, when it has life—its relations with its surroundings are different. As chemical affinity seems to hold the place of attraction in it, and to correspond to gravitation amongst celestial bodies, cohesive force amongst molecules, and magnetic force amongst polar molecules, so its dynamism or vital action seems to correspond to the force of repulsion, to the centrifugal force of heavenly bodies, the tension-force of molecules, and electrical repulsion. The display of energy coincides with a molecular change in the statical element. With the dynanism of a ganglionic nerve-cell, for example, a correlative molecular change, or "waste," as it is called, necessarily takes place. The substances which are met with in the so-called extractives of nerve-tissue afford abundant evidence of a material waste; for as products of the retrograde metamorphosis are found lactic acid in considerable quantities, kreatin, uric acid, probably also hypoxanthin, and representing the fatty acids, formic and acetic acid.* And what Du Bois Reymond proved to happen in muscle, Funke has observed to happen also with nerve; while the contents of nerve-tubes are neutral during rest in the living state, they become acid after death, and also after great activity during life. After excessive mental exercise, it is well known that phosphates appear in the urine in considerable quantities; and it is only by supposing an idea to be accompanied by a correlative change in the nerve-cells that we can explain the bodily exhaustion which is produced by mental labour, and the breaking down of the brain under prolonged intellectual efforts. There is even at times a sensation of something going on in the brain; and in insanity, such anomalous feelings are sometimes persistently complained of.† But the change

* It is interesting to remark how the products of chemical transformation of nerve agree with the products of muscular decomposition, and how the results coincide with what, *à priori*, might have been expected from the great vital activity of nerve-structure.

† Such cases are related by Guislain ('Leçons orales,' ii. p. 178), and by Trélat ('Annal. Med. Psychol.,' 1856, viii.) Griesinger is rather inclined to refer such feelings to the membranes of the brain, or to the distribution of the cerebro-spinal fluid.—Pathol. und Therapie der Psychisch. Krankh., p. 26.

or waste which accompanies energy is restored by nutrition during rest; and the conditions of future energy are thus established, nutritive attraction steadily repairing the waste of dynamical repulsion. The cell thus, for a time at least, preserves its individuality; and definiteness of energy, with the maintenance of individuality, are what is connoted by vitality.

Is the energy displayed by living matter something quite special? In attempting to answer that question two considerations should be kept in view. In the first place, an effect need not at all resemble in properties its cause; the qualities of a chemical compound are quite different from those of its constituents. Such a complex compound as organic matter really is may be expected, therefore, to exhibit peculiar properties in no way resembling those of its constituent elements or those of simple compounds. In the second place, the arrangement or grouping of the molecules in a substance, independently of its chemical constitution, may greatly alter its properties. In that condition of bodies which is described as Isomerism there are atoms alike in number, nature, and relative proportion, so grouped as somehow to produce compounds having very different chemical properties. Again, it has been found that the same matter may exist under two very different conditions, and with very different properties, as colloïdal and as crystalloïdal, in a gelatinous or in a crystalline state. And what is the chief difference? It is that the colloïdal is a dynamical state of matter, the crystalloïdal a statical state. The colloïd exhibits energy, its existence is a continued metastasis; and it may be looked upon, says Graham, "as the probable primary source of the force appearing in the phenomena of vitality." The distinction between the two kinds of matter is, in fact, "that subsisting between the material of a mineral and the material of an organized mass." And yet minerals may exist in the colloïdal state; the hydrated peroxides of the aluminous class, for example, are colloïds. Furthermore, the mineral forms of silicic acid deposited from water, such as flint, are found to have passed during the geological ages from the colloïdal into the crystalline condition; and, on the other hand, in the so-called blood crystals of Funke, a soft and gelatinous albuminoïd is seen to assume a crystalline contour. "Can any facts," asks Graham, "more strikingly illustrate the maxim, that in nature there are no abrupt transitions, and that distinctions of class are never absolute?" *

The foregoing considerations render it evident that the manifestation of organic energy by matter is not a contrast to the kind of energy which is displayed by inorganic matter, and so far justify the supposition that it may be a question of chemical composition and intimate molecular constitution. Vitality would not then be a special

* A further characteristic of colloïds is their singular inertness in all ordinary chemical relations, though they have a compensating activity of their own in their penetrability; they are permeable when in mass, as water is, by the more highly diffusive class of substances, but they cut off entirely other colloïdal substances that may be in solution. It is evident that our conception of solid matter must soon undergo considerable modification.

principle, but a result, and would be explained from the operation of the so-called molecular forces. Coleridge's assertion, that the division of substances into living and dead, though *psychologically* necessary, was of doubtful philosophical validity, would receive a support which its author could scarce have expected for it.

Before granting any conclusion, it is desirable to examine into that which is generally deemed to constitute the specialty of life. Now it is certain, when we consider the vast range of vitality from the simple life of a molecule or cell to the complex life of man, that valid objections may be made to any definition of life. The problem is to investigate the conditions of the manifestation of life. A great fault in many attempted definitions has been the description of life as a resistance or complete contrast to the rest of nature, which was supposed to be continually striving to destroy it. But the elements of organic matter are not different from those of inorganic, whence they are derived, and to which they return; and the chemical and mechanical forces of these elements cannot be suspended or removed within the organism. What is special is the manner of composition of the elements, the concurrence of manifold substances, and the complex way in which they are combined or grouped together. Such union or grouping is, however, only a further advance upon, and by no means a contrast to, the kind of combination which is met with in inorganic bodies. Life is not a contrast to non-living nature, but a further development of it. The more knowledge advances, the more plainly is it shown that there are physical and chemical processes upon which life depends. Heat is produced by combustion in the organism as it is in the fire; starch is converted into sugar as it is in the chemical laboratory; and the process of excitation in a nerve, on the closure of a constant stream, appears to be analogous to the process of electrolysis in which hydrogen is given off at the negative pole.* The peculiarity of life is the complexity of combination in so small a space, the intimate operation of many simultaneously acting forces in the microcosm of the organic cell. Knowledge cannot pass the cell-boundary, because there are not at present any means of following the intimate changes which take place within it; there is a world here into which the senses of man cannot yet enter. And as each great advance of science has followed some invention by which the operation of the senses has been extended, there can be little doubt that the important step towards a true science of life will only be made with the discovery of a means of tracing the delicate processes of cell activity. It is a microscopic chemistry that is needed; and the duty of investigation lies with the chemist, for whom biology is now waiting.

Before dealing with that which is considered to mark a second and great peculiarity of life, it will be well to illustrate the foregoing remarks from the phenomena of conscious vitality. It is, in truth, with the lowest form of vitality as it is with the lowest form of con-

* A. von Bezold: Untersuchungen über die Electrische Erregung der Nerven und Muskeln. Leipsig, 1861.

scious vitality—with the human mind in the earliest stages of its evolution. A self-conservative impulse moves the most barbarous people to regard the operation of the external forces of nature, and to adopt rude means to preserve life and to obtain comfort; the savage avoids the current which would drive his frail canoe on the hungry breakers, and shelters his hut from the overwhelming fury of the storm; he may be said to war with nature for the maintenance of individual power, as the vital force of a cell may be said to war with the nature that immediately surrounds it. But it is obvious that man only struggles successfully with the physical forces by recognising the laws of their action, and by accommodating his individual forces to the physical laws; it is victory by obedience. By conscious obedience to the physical law, man appropriates, as it were, the force thereof in the increase of his own power; the idea is developed in his mind as the correlative of the law or idea in nature; in his mental progress nature is undergoing development. By keeping in mind this analogy of the mental force the difficulty will be obviated, which there might seem to be in conceiving the organic cell as a result of physical and chemical forces, and yet as resisting the action of these forces. Every act of so-called resistance on the part of the cell to the natural forces is really a phenomenon indicating the development of them; its life is not a contrast to non-living nature, but a further complication of it. The fundamental law of life is the same for its conscious and unconscious manifestations; it is individuation by appropriation. And however necessary it may seem to the individual, as a part of a whole looking at the rest, to represent the vital as in constant antagonism to the physical, such a conception does not faithfully express the condition of the whole regarded as a whole. A just conception of nature as one harmonious whole is plainly not antagonistic to the spirit of any investigations which may seem to tend towards proving the dependence of life on physical and chemical processes.

That which is commonly said to constitute the specialty of life is the maintenance of a certain definite plan; and accordingly Coleridge, following Schelling, defined life as "the principle of individuation." Given the different kinds of force and of matter, and how, it is asked, is the pattern determined and brought out? As every individual is in life weaving out some pattern "on the roaring loom of time," though "what he weaves no weaver knows," so the lowest form of vitality manifests a definite energy, and is said to accomplish a definite plan. A crystal would go on increasing if suitable materials and the conditions of its growth were present, "but it has been provided that trees do not grow up into heaven." Life works according to an aim, said Aristotle. Admitting, then, all this, it does not exhibit a special contrast to the rest of nature. Liebig compares the living body to a building which is constructed after a definite, pre-ordained plan; but it is obvious that exactly in the same sense might the positive biologist say of the chemical atom, that it is constructed and displays energy according to a pre-ordained plan; or even of the crystal, that it works out a certain pattern, seeing that it cannot overstep the laws of its form. The plan is the law of omnipotence; and the law is not something outside the matter, but it is implanted in the matter. Organic matter, like the chemical element, has an activity given to itself which it must display; the law of causality is true of it as of inorganic matter, and the organic effect, the so-called accomplishment of the plan, is the necessary result of a certain molecular constitution and certain intimate combinations which exist in the organic molecule or cell.

The direct denial of a special vital force has been the natural reaction against that dogmatism which assumed a vital principle that was self-generating, did anything it liked, and was not amenable to investigation. That any force should be self-generating in inexhaustible quantity is really an inconceivable supposition. If the axiom that force, like matter, is not capable of annihilation, be accepted, and we find, as we do, that organic bodies incorporate, or somehow make to disappear inorganic matter and force, and thereby themselves increase, it is an unavoidable conclusion that the organic matter and force must represent the converted inorganic matter and force. To suppose that the vital force was self-produced would be to suppose a disturbance of the equilibrium of nature, and it might not then be unreasonable to fear lest the earth, by the increase of its repulsion force, should break through the hinges of gravitation and float off into space, or burst into fragments, as a planet between Mars and Jupiter is supposed at one time to have done.*

When, however, it is said that a minute portion of living matter converts inorganic matter, and thus develops new organic matter which has the power of doing likewise, it is evident that a great and peculiar potentiality is assumed in the living molecule. What power is it which transforms the matter and force? Some who have advocated the correlation of the vital force with the physical force seem not to have duly regarded this question: they have laid such great stress on the external force as to have fallen into an error almost as great as, though the opposite of, that of the advocates of a self-generating vital force. External circumstances are the necessary conditions of inward activity, but the inward fact is the important condition—it is the determining condition, and, as far as we know, it can only be derived from a like living mother structure. Nevertheless, even in that inherited potentiality there is not a contrast to that which happens in the rest of nature. When heat is converted into electricity, or any

* Science, in its view of life, seems to be following the course of development in Humboldt's mind. In his earlier writings he defined vital force as the unknown cause which prevents the elements from following their original attractive forces. (Aphorism. ex doct. Phys. Chem. Plant.) "Reflection and prolonged study," he says, in his 'Aspects of Nature,' "in the departments of physiology and chemistry have deeply shaken my earlier belief in peculiar so-called vital force." And again: "The difficulty of satisfactorily referring the vital phenomena of organism to physical and chemical laws depends chiefly (and almost in the same manner as the prediction of meteorological processes in the atmosphere) on the complication of the phenomena, and on the great number of the simultaneously-acting forces, as well as the conditions of their activity."

force into another, the change is not self-determined; the determining force is placed in the molecules of the matter, in the so-called statical force, that which Aristotle in his division of causes names the material cause. And if it be objected that a little vitality is able to do so much, the answer is that a like thing happens in fermentation. When a certain organic substance makes the inorganic matter in contact with it become organic, it may be that it does so by a kind of infection or fermentation by which the molecular relations of its smallest particles are transferred to the particles of the inorganic, just as in the inorganic world forces pass from matter to matter.

But there are further considerations. Admitting that the vital transforming matter is at first derived from vital structure, it is evident that the external force and matter transformed does in turn become transforming force—that is, vital. And if that takes place after the vital process *has commenced*, is it, it may be asked, extravagant to suppose that a similar transformation might at some period have *commenced* the process? The fact that in growth and development life is continually increasing from a transformation of physical and chemical forces, is after all in favour of the presumption that it may at first have so originated. And the advocate of that view turns upon his opponent, and demands of him how he, with a due regard to the axiom that force is not self-generating, and to the fact that living matter does increase from the size of a little cell to the magnitude of a human body, accounts for the continual production of transforming force? A definite quantity only could have been derived from the mother structure, and that must have been exhausted at an early period of growth. The obvious refuge is to the facts that it is impossible now to evolve vitality artificially out of any combination of physical and chemical forces, and that such a transformation is never observed save under the conditions of vitality.

Thus the argument stands. Meanwhile, those who do believe in the origination of vital force from other forces hope to succeed in artificially producing the upward transformation, and say reasonably enough that it is not to be expected that such transformation should now take place as a regular process in nature, except under conditions of vitality. Such a supposition is as unnecessary as it would be to suppose that the savage must continue to rub together his sticks, after he has obtained the spark, in order to make the fire burn. What only is necessary is that the spark of fire, or the spark of life, once evolved, should be placed under suitable conditions, and it will then go on increasing. The minutest portion of living matter really now contains implicitly, as it were in a microcosm, the complexity of chemical and physical combinations and the conditions which were necessary for the first production of life in the microcosm, and it supplies these as the conditions of further vital transformations. In fact, nature (as we say) having accomplished a result, does not need on each future occasion to go through the preliminary steps by which the result was first arrived at. And it is very interesting to observe how much use is made of the force supplied by the destruction of certain organic matter in raising other matter to a higher stage. It is now supposed, for example, that urea is partly produced by the oxidation of an excess of so-called albuminous matters in the blood, without these having entered into the formation of tissue; and the force thus supplied in the retrograde metamorphosis must exalt other elements.

It needs but little consideration to see that the living cell cannot supply all the force which is used in increasing and advancing life, in the multiplication and transformation of cells; heat and other external conditions are necessary, as being, so to speak, material for transformation. It is a mistake, however, to say, as some have said, that heat and external conditions determine the rate of growth. The rate of germination, for example, certainly varies according to external conditions, but the limits of variation are fixed by the inherent properties of the structure. The seeds of a begonia taken from the same pod will germinate, some in a day, some at the end of a year, and some at various intermediate times, even when they are all placed under the same conditions. And Mr. Paget has pointed out other indications of self-dependent time-rates in the lower organisms. There are, in fact, internal as well as external conditions of growth, and the former are the more important, for they are really the determining conditions. It is with the organic cell and its conditions as it is with the individual and his circumstances; the latter may greatly modify character, and are necessary for development, but the essential fact, which determines the limit of the modifying power of circumstances, is the nature implanted in the individual.

It is evidently impossible, in the present state of science, to come to any positive conclusion with regard to the vital force. All that can be said is, that advancing knowledge more and more clearly proves the dependence of vitality on physical and chemical processes, and tends to show that vital action does not contrast with the kind of action exhibited by inorganic nature. When vital force undergoes resolution into inferior force, simultaneously with the decomposition of substance, it is into heat, chemical force, and electricity that we find it, as it were, unfolded; it is a natural conjecture, therefore, that the conditions of the artificial production of vitality must be the highest and most complex chemistry to represent the statical correlative, and some mode of repulsion force, as heat or electricity, or both, to represent the dynamical correlative. It is certainly extremely unphilosophical in the present condition of knowledge to refuse to accept vitality as a special mode of manifestation of force; the special character of its phenomena demand that, whatever its real nature may be, vital force should be received as a distinct force on the same terms as chemical force or electrical force. The facts of observation, as well as *à priori* considerations, unquestionably demand also that it should be regarded as subject to the laws of the correlation and conservation of force.

As then vital force is plainly by far the highest force in dignity, a small quantity of it will correspond in value to a much greater quantity of an inferior force; one equivalent of vital force, in fact, will correspond to many equivalents of the lower forces. An immense amount of force is required to raise matter from its elementary state to that condition in which it is described as organic; and the upward transformation evidently only takes place through the intermediate action of chemical force. But vital force surpasses chemical force apparently in as great degree as chemical force surpasses physical force. How great then must be its mechanical equivalent! And what wonder that it was the last and highest development of nature, and that it was produced only after the inferior forces had been long in existence! What ground, furthermore, it might be asked, have we for supposing that it is destined to be the last development of force? Is it not possible that a still higher manifestation of force than that which we call vital, may ultimately result from the complexity of forces and conditions which are now present on earth? The hypothesis of La Place is, that in primæval times a large quantity of nebulous matter was spread through space. This nebulous matter was through gravitation aggregated into solid masses. Immense heat must have been produced, and this heat might produce light, and develope electricity as it does now when acting on the thermo-electric plates. Electricity might appear again as heat or as light, or as chemical force, as it does in the decomposing cell of a voltaic battery. The correlation of these forces we are able to trace now, and it is not difficult to conceive how they mutually excited and affected one another in the primæval times when the earth was, as we are told, without form and void. But there was a time when no life existed on the earth. So that as we can now obtain one force from another up to the point where life begins, when we are at fault, just as in nature considerable time elapsed before vital force followed on the physical and chemical forces. Science may, then, claim that in its difficulty it only reflects a corresponding difficulty in nature.

But there are other important considerations with regard to vitality. It does not follow because we recognise a special vital manifestation that there is but one kind thereof; it is in reality necessary to admit different degrees, if not different kinds, of vitality. As with organic matter so with organic force, we trace an advance from the most simple and general to the most complex and special. The tissue of the simple protozoon is uniform and exhibits no trace of structure; its dynamical relations are equally simple. In the ascending scale of life continuous differentiation of tissue corresponds with increasing speciality and complexity of relation with the external, until in man there is a unity of organism proceeding from manifold varieties of elements, and a unity of action from the co-ordination of many forces. And as it is with the animal kingdom so it is with the elementary structures which form it; there is a scale of dignity, a hierarchy of tissues; the lowest appear first, and are necessary steps for the evolution of the highest. All the force of nature could not develope a nerve-cell directly out of inorganic matter; and the cell of the Protococcus nivalis, or the molecules of the Amœba could not, under any possible circumstances, energize as nerve force. Between the vitality of thought and the vitality of the fungus there is scarcely a comparison possible;

the former is dependent upon the widest and most complex, and at the same time the most intense and special relations with external nature, while the latter exhibits only a few general and comparatively simple relations therewith. Between the relations of a nerve-cell and an epidermic-cell with their surroundings, there is as much difference as there is between the relations of a Rhizopod and those of a Cephalopod with external nature. And the relations of a nerve-cell with its surroundings, are, it must be remembered, dependent on the maintenance of the relations of all the inferior elements of the body which intervene in the descending scale between it and the inorganic.

Whatever, then, may be the fact in animal development, it is certain that transformation of species takes place in the structural elements. When a tissue takes material from the blood, it does not merely aggregate, but it assimilates it—that is, it makes it of the same *kind* with itself. In development, a higher tissue constantly proceeds from a lower one, and demands the lower one as a necessary antecedent to its production; it has thus, as external conditions, not only those which are general, but the intimate and special influences of the tissue which is before it in the order of existence. In the latter are supplied the special and essential conditions for the exaltation or transpeciation of force and material. But all exaltation of force is, as it were, a concentration of it; one equivalent of it corresponds to many equivalents of the inferior force which has been transformed. Hence it is that the power of reproducing tissues or parts in animals is diminished much more by development than by growth; and the law which describes the reparative power in each species of animal as being in an inverse ratio to its position in the scale of life, though not strictly proved, is yet true as a general proposition.

If, now, the degree of dignity of an element represents a correlative degree of vitality, it is obviously right to speak of the life of the blood without any design of placing its life on the same level with that of nerve. In the decomposition of material and the correlative resolution of force which take place when the blood-cell returns to the inorganic state, there will be much less force liberated than when a nerve-cell undergoes the retrograde metamorphosis. As a great expenditure of force is needed to raise matter from the inorganic to the organic state, so a further greater expenditure is required to raise matter from a low organic to its highest organic condition. The nerve-cell is, so to say, the highest parasite which sucks up the life of the blood; and if the process of its decomposition were accurately observed it would be found that all the force which had been consumed by it in its upward transformation was given back to nature in its downward metamorphosis.

The retrograde metamorphosis of organic elements is constantly taking place as a part of the history of life. In the function of nerve-cell, a nerve-force is liberated which excites muscular force, and is ultimately given back to external nature as motion; the correlative "waste" of substance is received into the blood and ultimately also passes back to nature. It is probable, however, that this "waste" does not pass directly out of the body, but that it is first used as the nutriment of some lower element. Thus, as there seemed reason to believe that in the economy of nature, animal matter did not undergo the extreme retrograde metamorphosis before being used as food by vegetables, so in the animal body the higher elements do not appear at once to undergo the extreme retrograde metamorphosis, but are first used as the nutriment of lower organic element. How admirably does nature thus economize in the body! Just as on a larger scale, the carbonic acid exhaled by animals is taken up by vegetables, and a poison thus removed from the atmosphere in which the animal lives, so by one organic element of the body, the blood is purified from the waste matter of a higher element which would be poisonous to it.

The parts impaired by activity, as all parts must be, are repaired during rest in a condition of health. And it is very interesting to observe, as Mr. Paget has pointed out, that the organic processes of repair in each tissue are adjusted to a certain time-rate, which is variable according to, but is not determined by, external conditions. The time-rate is determined by the implanted properties, and "for each unit of nutrition might be reckoned a unit of time." The periodicities of organic life appear to be prominent instances of the law, and the rhythmic motions of the heart, or the motions of cilia are, Mr. Paget supposes, due "to a method of nutrition in which the acting parts are, at certain periods, raised with time-regulated progress to a state of instability of composition from which they then decline, and in their decline may change their shape and move with a definite velocity, or (as nervous centres) may discharge nerve-force." In this recognition of the chronometry of organic processes, there is unquestionably great promise for the future; for it is plain that the observance of time in the motions of organic molecules is as certain and universal, if not as exact, as that in the motions of heavenly bodies. Each organic process has its definite time-rate; and each cell has its appointed period of life different for different kinds of cells. The exercise of its energy is the accomplishment of the life-task of the gland-cell of the stomach, and its existence ends therewith; but it is not known how soon the blood-cell and other cells die. The blood-cell may be ephemeral, and after the manufacture of its material straightway perish, supplying in the products of its decomposition material for the colouring matters of the bile; or it may accomplish its function more than once, and live therefore for some time. Certain facts do, indeed, point to a short duration, as, for example, the destruction of the nucleus in the blood-cell, the analogy of the cells of the stomach and milk glands, the sebaceous and spermatic cells, and the great production of blood-cells; but nothing positive is known, and the subject is one which awaits, and is at present receiving, careful attention.*

Such, then, is the general process of life physiologically regarded.

* The experiments of Helmholtz on the time-rate of conduction in nerves are well known, but the following words of his well illustrate the tendency of scientific investigation: "As long as physiologists considered it necessary to refer the operations of the nerves to the extension of an imponderable or psychical principle, it might well

But there is nothing special in disease. Although the destructive cancerous mass seems at first sight to admit of no sort of comparison with the beneficial formation of a developing organ, yet the production is governed by laws of organic growth and activity. No new forces nor new laws appear in the organism under the circumstances which are described as disease. "'Tis as natural to die as to be born," says Sir T Browne; and if we choose to accept the doctrine of final cause, we must acknowledge that the disease which leads to death is as natural, as much in the purpose of omnipotence, as the physiological processes which constitute health. An individual exists in certain relations with the external, and the harmony which results from the maintenance of these relations is health, while a disturbance of them, whether from a cause in the organism or in the external circumstances, or partly in one and partly in the other, is discord or disease. The phenomena of morbid action may therefore, when properly regarded, be serviceable as experiments illustrating the character and relations of vital action.

As each cell has its appointed period of life, and each species of cell its natural degree of life, and as there are many cells and many kinds of cells in the human body, it is evident that disease will be more easily initiated in it than in an organism with less differentiation of tissue, and less complexity of structure. For the life of the organism is the sum of the life of its individual parts, and superiority of vitality signifies more numerous, special, and complex relations with the external. In the lowest organisms, where there is a similarity of structure, one part is independent of another, and dependent only in the maintenance of certain general and simple relations with the external; there is, therefore, comparatively little liability to disturbance.* When the parts are, however, unlike, and there is a definite subordination of them, so that the well-being of the highest structure is dependent on the well-being of all the structures which intervene in the descending scale between it and inorganic nature, there is plainly abundant room for disturbance. As in the state, so in the organism, the vitality of the government flows from, and rests upon, the well-being of individuals.

When, from some of the many disturbing causes which initiate

appear incredible that the rapidity of the stream should be measurable within the limits of the animal body. At present we know, from the investigations of Du Bois-Reymond on the electro-motor properties of nerves, that the activity by which the propagation of a stimulus is accomplished is closely connected with an altered arrangement of their material molecules—perhaps even essentially determined by them. Accordingly, the process of conduction in nerves may belong to the series of continuous molecular operations of ponderable bodies, in which, for example, the conduction of sound in the air, or the combustion in a tube filled with an explosive mixture, is to be reckoned. It is not surprising, therefore," he adds, "that the speed of conduction should be very moderate."—Ueber die Methoden, &c.

* Goethe, after saying that everything living is a collection of living self-dependent beings, adds, "Je unvollkommener das Geschöpf ist, desto mehr sind diese Theile einander gleich oder ähnlich, und desto mehr gleichen sie dem Ganzen. Je volkommner das Geschöpf wird, desto unähnlicher werden die Theile einander. Je ähnlicher die Theile einander sind, desto weniger sind sie einander subordinirt. Die Subordination der Theile deutet auf ein volkommneres Geschöpf."

disease, a particular elementary constituent of the body is prevented from rising to the dignity of its specific constitution and energy, there will, if the disturbing cause has not been so serious as to destroy the life of the part, be a production of an element of a lower kind with a lower energy ; and that is a diseased product. It is as if the tissue of a polype were produced amongst the higher histological elements of the human body. There may be a production of foreign substance in larger quantity than that which should rightly be formed of the natural tissue and a greater display of force, but both structure and energy are of a lower order. What is gained in quantity is lost in quality, and the vitality is intrinsically less.

Inflammation in a part is really the result of a degeneration of its vitality. When a wound heals by the "first intention," there is direct adhesion of its surfaces, and no inflammation, for the natural vitality of the part is maintained, and effects the repair. When slight inflammation occurs, the vitality of the part has undergone a certain degeneration, and material of an inferior order to the proper element of the part is produced; this substance binds the surfaces together, and it may in process of time, on the complete subsidence of inflammation, and under the favourable conditions of surrounding natural vitality, even rise to the condition of the proper structure. But the lymph does not appear to be thrown out with any special beneficial design ; it is the simple result of a deterioration of vitality, is only a less degree of a positive evil. When greater inflammation takes place, or when the natural vitality of the part is feeble, there is a greater degeneration, and material of a still lower kind, which is not even organizable under any circumstances, is produced. Pus is poured out, and ceases to appear with the restoration of the proper vitality of the tissue. If the inflammation is still greater, the degeneration passes into actual destruction of life, and mortification ensues. When Hunter, therefore, speaks as he does of nature calling up the vital powers to produce suppuration, his words convey a false notion of what really happens. The injury has so damaged the parts that the vital force cannot rise to its specific elevation; an inferior kind of vital action is alone possible, which is really disease, and only so far beneficial as it proves that the vitality of the part has not been killed outright. As might be expected, therefore, it is in exhausting diseases that inflammation most commonly and easily occurs. How incorrect, then, is it to speak of inflammation as if it were a process specially provided for restoring the natural vitality of parts! When adhesive inflammation is said to limit the suppuration of an abscess, its occurrence is a result of diminishing mischief, and testifies to a less serious degeneration of vital force. When adhesive inflammation fixes a piece of strangulated gut to the side of the belly, so as happily to prevent the passage of fæcal matter into the peritoneal cavity, it is sometimes said to be a wise and kindly provision of nature. What then shall be said of inflammation when it glues the gut to a hernial cavity, or manufactures a fibrous band which strangles the gut?

That which is true of the material products of inflammation is necessarily true of its force; the heat, and pain, and rigors, the forces as well as the material, testify to a degeneration of vital force. The sort of stormy rage and demonstrative activity which characterize inflammation, though unquestionably an exhibition of force, are not really an increased display of the proper vital force. The latter has undergone a transformation from the quiet self-contained activity of development into the unrestrained dissipation of a lower activity; and, as regards the latter, it might be said that several monads of its matter, or volumes of its force, are equivalent only to one monad of matter, or one volume of force of the former. Rigors, as the involuntary action of voluntary muscle, are a degradation of action witnessing to a molecular deterioration of vital conditions. Heat is a physical force which must have resulted from the retrograde metamorphosis of vital force. The existence of pain, where rightly there should be no sensation, testifies to a molecular deterioration of statical element, and a correlative exhibition of force. The increased action of inflammation in a part is, therefore, diminished vital action. Perhaps it might once for all be stated, as a law of vital action, that the dignity of the force is in an inverse ratio to its volumetrical display. It is, indeed, with organic action, as it is with mental action. The emotional man displays considerable force, and often produces great effects in the way of destruction, but his force is vastly inferior to that of the man who has developed emotional force into the higher form of will-force, who has co-ordinated the passions into the calm, self-contained activity of definite productive aim. Surely creation always testifies to a much higher force than destruction.

The foregoing considerations unavoidably flow from a conception of vitality as correlate with other natural forces, and as subject to the law of the conservation of force. They obtain their greatest value, however, from being in accordance with the important generalizations which one of the most philosophical physiologists of the present time has made with regard to morbid products. Virchow has, as is well known, referred all morbid structures to physiological types, and maintains that there is no new structure produced in the organism by disease. The cancer-cell, the pus-cell, and all other disease-produced cells, have their patterns in the cells of healthy structure. The cells of tubercle correspond with the corpuscles of the lymphatic glands; pus and colourless blood-corpuscles cannot be distinguished except by looking at the place whence they come; the cells of cancer in bone "are the immediate descendants of the cells in bone;" and certain colloïd tumours have the structure of the umbilical cord. "Where a new formation takes place, certain histological elements of the body must generally also cease to exist;" and every kind of new formation is really, therefore, destructive, and destroys something of what previously existed. The connective-tissue, with its equivalents, he describes as the common stock of germs of the body; from them, morbid structures proceed by continuous development. "Heterologous tissues have physiological types; and there is no other kind of heterology in morbid structures than the abnormal manner in which they arise as to place (heterotopia), time (heterochronia), and quantity (heterometria)."*

The conclusions with regard to vital force, which a consistent conception of it as a natural force seems to necessitate, will find extensive application in the various phenomena of disease. We have seen that if the resolution of the vitality of a single nerve-cell into a vitality of a lower kind be supposed, into that, for example, of polype substance, it would necessarily suffice for the production of a whole polype, or perhaps of more polypes than one. In other words, one nervous unit, monad, or molecule, is the vital equivalent of many units, monads, or molecules of polype substance. How idle it is, then, to dispute, as some do, as to whether epilepsy is increased vital action or diminished vital action, when there exists no clear conception of what is meant by the words! No one can deny that there is great display of force in the convulsions of epilepsy, but is it increased vital force? Is a man in convulsions a strong man? for that is the real question. Does convulsion in a paralyzed limb indicate increased vital action of it? When tetanus of a muscle is produced, as Weber showed it might be, by putting a loop of thread round its nerve and slowly and gradually tightening it, does the violent action of the muscle testify to increased vitality? If it really does, then the mechanical tetanomotor of Heidenhain might, properly used, suffice for the cure of every paralysis, and effect a complete renewal of life.

In speaking of vital action, we may either consider the whole organism as individual, or we may consider the cell or organic monad as the individual. If we regard the organism as individual, then when general convulsions take place in it—that is, violent and aimless movements completely withdrawn from the control of the will, which should rightly co-ordinate them into definite action—it is simply to use words without meaning to say that the vital action of the individual is increased. There is not, then, individual action; and the definition of vitality is not applicable to the organism as a whole. The highest manifestation of individuality is in the consciousness of man, the so-called unity of the ego; but when the co-ordination of forces for a definite end is replaced by the convulsions of epilepsy, there is neither subjective nor objective unity of action. Instead of that quiet will-force which expresses conscious unity, or that unconscious unity of organic action which is manifest in sleep, there is the violent and incoherent exhibition of inferior force. Increased action is the result of a degeneration of the proper vital action. "A man in convulsions is not strong, though six men cannot hold him."

Like considerations apply when the single cell is regarded as individual. In virtue of a certain chemical constitution and a certain definite arrangement of molecules, a cell exhibits energy as nerve-force. That special mode of energy is the definite result of a certain co-ordination of chemical combinations and molecular relations; and these are connoted in the individuality of the cell. When, however,

* Cellular Pathology.

in place of the definite process of statical attraction (nutrition) and dynamical repulsion (energy), there takes place a large demonstrative display of force, as general epileptic convulsions, being the sum of the action of the individual cells, prove there must, it is impossible to pronounce such force as of the same rank or kind as the proper energy of the cell. It is an inferior kind of power, and the certain indication of a degeneration of the statical correlative. It is the duty of a cell, so to speak, as of an individual, to live in certain relations with its surroundings—it is, indeed, its essence as an *individual* cell of specific character, and when it is not so living, it is really degenerating, losing its nature or kind, passing more or less quickly towards death. Its action is certainly not increased functional action. In truth, it would be as just to call the extravagant action of madness in an individual occupying a certain position in a system of Government increased functional action, and to say that the Government was stronger for his degenerate action. A State, again, would not be powerful, would not even exist, if each individual did as his passions prompted, altogether regardless of his relations to others; and it would certainly be a strange use of language to say then that the functional action of that individual was increased.

The phenomena of conscious vitality might be used to illustrate the same principles. A passionate man is not strong-minded, nor do the ravings of insanity reveal mental vigour. A completely-fashioned will is the true evidence of a strong mind. "A character," said Novalis, "is a completely-fashioned will." As in the order of natural development, there has been an ascent from the physical and chemical forces to the aim-working vital force, and thence from the lowest vitality to the highest manifestation thereof, so in the course of mental development there is a progress through sensation, passion, emotion, reason, to the highest phase of mental force, a well-fashioned will. The rightly-developed mind, like the healthy cell, recognises its relations to others; self-feeling gives place to or expands into moral feeling, and in the will all the phases of consciousness are co-ordinated into calm, just, definite action. Noise and fury surely indicate weakness; they are the manifestation of inferior force—the tale of an idiot signifying nothing. The strongest force is quiet force, and the ravings of insanity might not unjustly be compared to the convulsions of epilepsy.

May we not, then, already perceive, what advancing knowledge must ever render more clear, how the conscious mind of man blends in unity of development with the unconscious life of nature? As the revelation of nature proceeds in the progress of science, the idealism of Plato and the realism of Bacon will be found to harmonize as expressions of the same truths; the generalizations of Humboldt and the poetical intuitions of Goethe may be looked upon as but different descriptions of the same facts. Idealism and realism blend and are extinguished in the intimate harmony between the individual and nature. How great, then, the ignorance which fancies that poetry demands a rude age for its successful development! How little, again, the insight which would make of science an ugly anatomy only! After analysis comes synthesis, and beyond the practical realization of science in works which add to human comfort, there remains the æsthetical embodiment of science. Art has now opening before it a field so wide that imagination cannot dare to limit it, for science must plainly attain to its highest development in the work of the future poet, who shall give to its reality a beautiful form. Goethe indicated the path, but he who shall accomplish it will be a greater than Goethe.*

REVIEW XIV.

Physiological Researches. By JOHN DAVY, M.D., F.R.S.—*London*, 1862. pp. 448.

THE title of the above work will be sufficient to lead to its recognition by many readers as the legitimate successor of the well-known 'Physiological and Anatomical Researches' given to the world by Dr. Davy, now nearly a quarter of a century ago. Like them, it consists partly of reprints of papers published in the Transactions of the Royal and other Societies, partly of contributions to physiological science not previously made public. We hope to show, in the course of this article, that in interest and importance the present volume falls in no way short of its predecessors. The earliest paper in the former volumes appeared first in the 'Philosophical Transactions for 1814,' and consists of experiments on animal heat; and much of the new matter in the present volume relates to the same subject—a subject on which Dr. Davy has remained throughout the long period between these dates the chief original authority. It is seldom indeed that it falls to the lot of a philosopher to contribute actively to the advancement of science during half a century, and still more rarely does it happen, as is the case with Dr. Davy, that his latest—we hope not his last—contributions exhibit the same accuracy and careful research, as well as the same vigour and elasticity of mind which distinguished his earliest efforts.

The number and variety of the subjects upon which Dr. Davy treats in the volume before us, preclude the possibility of noticing all within the limits of any ordinary article; we shall therefore content ourselves with some remarks upon a few of them, which either from their intrinsic importance, or from their touching upon some of the controversies of the day, occupy at the present time the most prominent position.

Besides and beyond the value of the results at which Dr. Davy has arrived in his investigations on the subject of temperature, another

* Perhaps the truest estimate of science, and the most remarkable prophecy with regard to it, is to be found in that wonderful tale by Goethe, "Das Mährchen," a tale which has been described by one who has done most towards making Goethe known and understood in England "as the deepest poem of its sort in existence —as the only true prophecy emitted for who knows how many centuries." With increase of understanding, too, the second part of Faust may probably be found not to be the failure which it is sometimes said to be.

excellence which his papers possess in an eminent degree is that of suggestiveness. Interspersed among the records of his own experiments, sometimes depending upon their results, sometimes suggested incidentally by the processes employed, we find constant indication of the direction in which further investigations should be pursued—indications which it were well if some other physiologist as untiring as Dr. Davy would follow out to their results. The conclusions which Dr. Davy has drawn on the subject of the temperature of man, are so well known, that we do not think it necessary to repeat them here, but shall merely refer to certain of them which affect the results arrived at by other writers, or seem to touch upon other questions of interest to our readers.

In the comparison drawn (p. 45) between the results arrived at in temperate and tropical regions, as to the temperature of the body at different periods of the day, we find it stated that the minimum temperature in the latter occurs just at the same time as the maximum temperature in England—viz., on first rising in the morning, and conversely, at the last examination before going to rest in England, the body was almost uniformly found at its minimum temperature; in Barbadoes, at its maximum. The following is the author's explanation of these curious and somewhat puzzling phenomena:

"Probably the low morning temperature of the body within the tropics, as shown above, may be owing principally to three circumstances—to the depressing or lowering power of sleep, to the light bed-covering used there, and the free circulation of the air in the rooms. A sheet and a thin coverlet were the only protection from the air, and the windows mostly being open, the outer air had free access, so that towards morning the temperature of the air of the room did not differ from that of the external air commonly more than two or three degrees. Moreover, the higher temperature observed at night, before going to rest, may have conduced to the lower morning temperature noticed on rising; as the lower temperature at night in England, with the opposite circumstances as to bed-clothing and the air of the sleeping-room, may have conduced to the higher morning temperature recorded there—fluctuations these in accordance with well-known physiological laws."

Of the great efficacy of the second of these assigned causes, no one can doubt when he has once admitted the fact, amply proved in these papers, that the temperature even of warm-blooded animals is considerably affected by that of the medium by which they are surrounded. Thus we often find persons in England of sluggish circulation and inactive habits who positively never feel warm in winter, except when they have been for some hours in bed. On the other hand, one of our many unwholesome national habits is that of sleeping in hot weather in closed-up rooms and under hot and heavy bed-clothes. This is especially the case with the poorer classes. It is scarcely possible to enter the wards of a hospital in the summer months without being struck with astonishment at the determined way in which many of the patients insist on covering themselves with a load of bed-clothes, under which they unceasingly perspire. It is therefore not difficult to understand why the heat of the body should in our climate and with our habits reach its maximum towards the conclusion of a long night's rest, nor why the contrary should be the case in the tropics, where precisely opposite habits obtain. It is by no means equally easy to understand why the temperature, which falls in England in the evening, should continue to rise in Barbadoes, but we cannot divest ourselves of the notion that it may appear to do so to a greater extent than is really the case, from the fact that Dr. Davy made his last observation in England at midnight, and in Barbadoes about ten P.M. But whatever be the cause of the discrepancy, its existence alone must serve, at any rate, to limit the application of such results as those recently published by Dr. Smith in his work on 'Cyclical Changes in Health and Disease.'* Dr. Smith there argues from the facts of the lowness of the pulse and other such symptoms in the evening and night, and the contrary condition in the morning, to various conclusions in regard to the times of the day most suitable for labour, food, recreation, rest, &c. Now, it is clear that since a totally different state of things exists in this very particular in the tropics, either his conclusions cannot be applied to the inhabitants of tropical countries, or, if so, they must rest upon entirely different premises.

It is much to be desired that Dr. Davy had been able to make some experiments, similar to those instituted upon himself, on negroes or other natives of tropical regions. If this were done, we should learn whether the higher temperature which obtains in Europeans in hot climates is a constant and natural phenomenon or a quasi-pathological effect of imperfect acclimatisation.

Before leaving the subject of temperature, we must notice two more very interesting papers, in one of which the author shows (p. 92) the extremely slight variations of temperature which produce in insects the vast difference between activity, torpor, and even death. Thus, butterflies (*Vanessa urticæ* and *io*) were found to be torpid at temperatures varying between 56° and 62°, and to become active when the temperature was raised, though only 4° or 5°. The other contains a number of observations, now published for the first time, upon the temperature of various animals, fishes, reptiles, and birds. These are remarkable, as showing in every case, with one exception, the temperature of fishes to be considerably above that of the water in which they are taken, and their internal organs very much warmer than those near the surface of their bodies. It is worthy of notice, however, in regard to these observations upon fishes, that they were made in most cases immediately after the fish was taken either by a harpoon or a hook, and frequently by introducing the thermometer bulb through a wound in the dorsal muscles. Now Dr. Davy has elsewhere shown us (p. 81), that mental labour and other excitement of the nervous system raises the temperature of the human subject. Something similar is doubtless the case with the fish, and we can imagine few cases of more violent excitement than would be produced by being dragged up by a hook after a violent struggle, into a medium in which it is almost impossible to breathe, and after suffering a variety of contumelious treatment, having a gash cut through one's integument, and a thermometer

* P. 86 et seq.

suddenly thrust into the wound. The one shark (p. 81) whose temperature at the end of all this was found still to be low, must either have exhausted his whole powers before he could be hauled on board, or must have been a very stoic among fishes.

In the same paper are contained some experiments on reptiles and hybernating mammals, which show how the former may fall to the low temperature of the surrounding air even at 48°, retaining life, as it were, reduced to its lowest term. In this condition all the vital processes seem to go on at the slowest conceivable rate; animal life, properly so called, is suspended, and only the vegetative functions continue in just a sufficient degree of action to prevent decomposition. The blood is dark and loaded with carbonic acid, the heart contracts slowly and feebly, and the whole process of nutrition and tissue-change is reduced to its minimum. These animals, then, may be said to be more cold-blooded than fishes; for while the latter, as we have just shown, seem generally to preserve a temperature several degrees higher than that of the medium in which they live, many of the reptilia, as, for example, the frog, toad, and tortoise, fall to the level, and even below the level, of the atmosphere around them. In the toad, too, this habitual low temperature is interesting in relation to the remarkably small amount of oxygen which the animal consumes in respiration. Thus, Dr. Davy found that a toad confined in a limited quantity of atmospheric air during twenty-two hours consumed an amount of oxygen not equal to double its own volume.

One further fact in relation to the temperature of man, seems to demand some notice on account of its relation to some of the great social problems of the day. It is the effect of alcoholic drinks. These Dr. Davy has found, at least during residence in the tropics, when taken in any considerable quantity, to have a decidedly lowering effect upon the temperature, as judged of by a thermometer placed under the tongue. This lowering of the temperature is followed after an interval of some hours by a somewhat contrary effect, the heat of the body being found on the following morning somewhat greater than usual.

This is a result which at first takes us rather by surprise, but we find that the experiments of Dr. E. Smith* lead to a conclusion not altogether inconsistent with it. He speaks of alcohol as diminishing the effect of cold rather by checking the action of the skin, and thus preventing the dispersion of heat, than by in any way adding to its production within the body. We are, nevertheless, inclined to look for some further explanation of this phenomenon, inasmuch as there is no doubt that the ingestion of alcoholic drink excites and stimulates the circulation, and we know that in almost every case where the circulation is stimulated the heat of the body is increased. It seems probable, since Dr. Davy has shown in the case of fishes that very great differences in temperature may exist simultaneously in different and distant organs, that the apparent diminution of temperature which follows the imbibition of wine in considerable quantity, may in reality be due to a local congestion produced thereby in the mucous membrane of the stomach and

* Op. cit., p. 89.

other central organs of the body ; just in the same way as we know that in some forms of nervous excitement arising from study or exciting conversation, the face and head become hot, and the cheeks flushed and burning, while the extremities are chilly and cold to the touch. If this be true, the subsequent increase of heat in the same organs in which it was at first deficient, will follow almost as a matter of course upon the subsidence of the original local congestion, or as a kind of reaction against it. This, at least, is a point upon which further investigation and experiment is eminently desirable.

Passing by some other papers, not for lack of interest in them, but by reason of the limits of our own space, we must in the next place call attention to several successive essays, Nos. 22 to 28, upon the Salmonidæ, chiefly in relation to the development of their ova and the conditions required for transporting them to new localities, without endangering the vitality or checking the development of the embryo fish. These, relating as they do to a subject much discussed in the present day, will be read by many with great interest and advantage. Dr. Davy's experiments leave no room for doubt that the ova may, with common and reasonable precautions, be conveyed in safety to considerable distances, and the fish hatched with success. Upon the further question of the practicability of naturalizing or acclimatising them when so transferred to new waters, he does not venture an opinion. This question, besides its bearing upon commercial considerations as to the advantages to be derived from attempts to increase artificially the supply of salmon and other fish as articles of food, has a further and more immediately scientific interest from its relations to some of Mr. Darwin's recent speculations. Indeed it was at the request of this gentleman that some of these experiments were undertaken by Dr. Davy, and his account of them takes the form of two letters to Mr. Darwin. The conclusions arrived at are, as far as they go, entirely favourable to the latter author's views, inasmuch as Dr. Davy shows that there is every reason to believe, from the considerable range of circumstances under which the ova of salmon retain their vitality, that they are quite capable of being carried, attached to the bills, feet, or feathers of aquatic birds, to very considerable distances ; and then, on being deposited in suitable localities, developing without let or hindrance. This is an attractive, though somewhat obscure subject. We cannot, however, discuss it here in the manner in which it deserves to be discussed, but must hasten on to consider somewhat more in detail another vexed question to which Dr. Davy devotes two papers in the volume before us (34 and 35)—viz., the Coagulation of the Blood.

This subject, like that of temperature, is, as many of our readers will remember, no new one to Dr. Davy. A large portion of the second volume of his 'Researches' is occupied with investigations into it. The immediate occasion of the present paper was afforded by the publication of Dr. Richardson's well-known Astley Cooper prize essay upon 'The Coagulation of the Blood,' in the year 1856. The main position maintained in that work may be shortly stated as follows :

1. That the fibrin of the blood is maintained in its fluid state by the action of ammonia.

2. That coagulation results from the escape of that substance, and may be prevented by any means which retain it.

3. That the cause of coagulation, whether in the vessels or out of them, is the same.

From these main propositions there follow as corollaries several others almost equally important, for not only does the acceptance of them lead to the acknowledging of the coagulation of the blood as a phenomenon brought about by purely chemical agency, but it also necessarily brings with it a train of pathological views which must lead, and indeed have in some cases already led, to important modifications in therapeutics. Dr. Richardson has supported his views not only with much ingenious reasoning, but also by a large number of experiments, making altogether a formidable array of evidence. To some few of the experiments we shall presently have occasion to refer more particularly. Dr. Richardson having settled to his own satisfaction, by some previous investigations, that coagulation is a chemical, and not a vital or merely physical phenomenon, arrives at the choice of ammonia as *the* chemical agent by which it is brought about by the following steps.* He finds that if the vapour of blood be passed through hydrochloric acid, and the latter then mixed with chloride of platinum, crystals of ammonio-chloride of platinum are formed. This is the first step, as demonstrating the long disputed fact of the presence of ammonia in freshly-drawn blood; and Dr. Richardson holds that it is not invalidated by the non-appearance of crystals of chloride of ammonium upon a rod, moistened with hydrochloric acid, and exposed to the vapour of such blood, inasmuch as the same negative result is produced even when ammonia in a very small proportion—1 to 450—has been previously mixed with the blood experimented on; when, therefore, the experimenter already knows that ammonia is present. That is to say, Dr. Richardson defends himself against the negative results of this ordinary test for the presence of ammonia by denying the value of the test itself. He finds further that fibrin dissolves, though slowly, in ammonia, and that blood received into a vessel containing ammonia is either delayed in its coagulation, or fails to coagulate at all, so long as the vessel is carefully closed, according to the quantity of the alkali in proportion to the mass of the blood introduced, but coagulates as usual when the ammonia has been allowed to evaporate. The following are two of the experiments by which this point is established, in Dr. Richardson's words:

"One fluid ounce of blood from a dog was received into a bottle containing one grain of ammonia in ten minims of water. No coagulation took place for several hours in a stoppered bottle; but a little of the blood, on being exposed in a wineglass, coagulated in forty-five minutes. It evolved no such amount of ammonia as could be detected by a glass rod dipped in hydrochloric acid. The blood-corpuscles were unaltered. The natural period of coagulation of the blood used in the experiment was one minute and thirty seconds." (p. 290.)

* See chapter vi. of his Essay.

Again :

"Five hundred grains of blood, freshly drawn [from an ox], were received into a bottle containing one-fourth of a grain of ammonia with twenty minims of water. The blood remained fluid for twenty-three minutes as it stood exposed to the air, the temperature of which was 58° Fahr. It evolved ammonia freely for several minutes. After the twenty-three minutes coagulation commenced, and at the half hour the whole blood was in a firm, but rather dark clot. The blood of the animal used coagulated naturally in two minutes on exposure to the air." (pp. 291-2.)

These supply the next step in the argument by showing that the same conditions which favour the escape of ammonia favour coagulation, and that where ammonia is known to be present, and is prevented from escaping, then coagulation does not occur. Such are some of the experiments and results of Dr. Richardson, well known, doubtless, to our readers, but nevertheless necessary to be quoted before we could enter upon the consideration of researches which tend especially to impugn them.

Dr. Davy, however, in the paper now before us, and first published in the 'Transactions of the Royal Society of Edinburgh' for 1859, was not the first to impugn Dr. Richardson's conclusions. Mr. Lister had previously published an elaborate and very interesting paper in the 'Edinburgh Medical Journal' for April, 1858, in which, while he admitted Dr. Richardson's experiments as conclusive as to the cause of the coagulation of the blood when drawn from the body, he showed reason for believing, both from pathology and from the results of some very ingenious experiments on animals, that the escape of ammonia could not be the cause of that phenomenon when it occurs, as it so often does, within the vessels of the body during life. Referring to coagulation as it occurs in phlebitis, Mr. Lister very pertinently asks, "How can the fact that the wall of the vessel is inflamed determine, on simply chemical principles, the evolution of ammonia from the blood within it?" and upon investigating the matter, he found that in the limbs of several animals (sheep and cats) which he examined, the blood which had remained in the small vessels continued fluid for as long as six days after the limb had been severed from the body, and still retained its power of coagulation when shed out of them.

"Again," says Mr. Lister, a little further on in the same paper, "if a vein be cut across with fine sharp scissors, without disturbing its connexions, or inflicting much injury on its coats, the blood will be found, after about six hours, perfectly fluid in the vein up to within perhaps one-twentieth of an inch of the wound, where a small clot is perhaps seen, utterly insufficient to obstruct the progress of ammoniacal vapour. Hence it appears to me to follow, as a matter of demonstration, that, if free ammonia really exists in the blood within the vessels, the circumstance of its being in those vessels deprives it entirely of its volatility; and that, whether the ammonia be free in the blood or not, its chemical tendencies, such as it exhibits outside the body, are in some manner entirely modified by the vicinity of the vascular tissue. With regard to the nature of the modifying influence, no other explanation appeared to offer itself than that it depended upon residual vitality in the tissues." (p. 900.)

Here, then, are some facts which appear to us to be quite irreconcileable with Dr. Richardson's theory; nor are these by any means all which Mr. Lister has collected. Thus, he finds that in an ordinary post-mortem examination, some six-and-thirty hours after death, you may find the blood fluid, and still capable of coagulation, in the smaller veins, while in the heart and large vessels it has long ago formed a firm coagulum, thus showing in his opinion that the tendency to post-mortem coagulation is increased with the mass of the blood contained within a vessel in proportion to that portion of it which is in immediate contact with the vessel's walls; moreover, that when the vitality of the walls of a vessel are destroyed for a certain distance—say an inch—by the application of strong ammonia to an isolated vein during an animal's life, small coagula are found in less than two hours adhering firmly to the interior of the wall in the damaged portion. All this tends to support the view that it is by what we must term the special vital endowments of the vascular walls that the fluid condition of the blood within the living body is maintained, rather than by any merely chemical agency. One further observation of Mr. Lister we add bearing upon the question. The author is speaking of observations upon sheeps' feet already separated from the body, after having been rendered somewhat turgid with blood by having been tightly bound round before amputation; he says:

"If the skin be reflected from over a subcutaneous vein full of blood, and lightly replaced, so as to protect the subjacent parts from evaporation, without excluding the air, the vessel will be found in two or three hours changed from a dark venous colour to a scarlet arterial tint; yet no coagulation will occur in the blood, although the oxygen of the atmosphere has evidently penetrated through the coats of the vessel, showing that abundant opportunity has existed for the evolution of ammonia, provided that any tendency to such an occurrence existed." (p. 899.)

Here, then, we have in Mr. Lister's paper a kind of revolt against the ammonia theory, though the view which he holds does not seem a very philosophical one; for while he maintains thus strongly the position that the maintenance of the natural fluidity of the blood during life is due to the vitality of the vascular walls, he is ready to admit that Dr. Richardson has proved its coagulation when withdrawn from the vessels to be brought about by the escape of ammonia. Now it is a very old and a very sound principle which forbids us to assume two different causes for the same phenomenon; and if, as in the case before us, a phenomenon occurs in certain cases in which it is clear that what is in other cases supposed to be its cause cannot be in action, it should always warn us of the necessity which exists for re-examining the other cases, and reconsidering our theory altogether.

It is to the stage of the question which we have now reached that Dr. Davy's paper on this subject chronologically belongs, part of it having been laid before the Royal Society of Edinburgh in 1859, and the rest formed part of a paper read at the meeting of the British Association in 1862. The ammonia theory may be said to have been at this time the theory in possession, and Mr. Lister's view, as we have said, the first symptom of revolt against it. If it be said that Dr. Davy's labours in this matter are purely destructive, we must remember the words of an earlier English physiologist, justly famed for his investigations into this very subject—Thackrah: "The erroneous notions and unfounded theories which have been vainly adduced to remove the veil of nature, have greatly obstructed the path of inquiry, and added darkness to obscurity;" and reflect, that in the settlement of an uncleared country the pioneer must ever precede the architect.

Of the steps which are enumerated above, by which Dr. Richardson arrives at his conclusions, there is one only—viz., that which shows the presence of ammonia in the blood by the formation of ammonio-chloride of platinum, which he omits to notice—which Dr. Davy does not call in question, and justify himself in so doing by the record of experiments yielding results contradictory of those obtained from very similar experiments by Dr. Richardson. Thus, after having shown, what Dr. Richardson also admits, that the ordinary tests fail to show the presence of ammonia in fresh-drawn blood, even when it has not been exposed to the air, he proceeds as follows:

"Two ounces of blood were received in a glass vessel into which two drops of aqua ammoniæ had just before been poured; as soon as caught, it was gently moved by inclining the vessel backwards and forwards, to favour the action of the volatile alkali. *In less than two minutes coagulation had taken place.* A distinct odour of the volatile alkali was emitted, and when a plate of glass, moistened with dilute muriatic acid, was for a short time kept over the vessel, crystals of muriate of ammonia formed on it—that is, after evaporation, and as viewed under the microscope. The clot was of about equal consistence throughout, and tolerably firm; cut into pieces, *each piece, tested by the approach of a rod dipped in muriatic acid, showed by the fumes produced the presence of ammonia.* The serum, which in a few hours had separated from the crassamentum, had an ammoniacal odour." (pp. 384–5.)

It is remarkable how this one simple experiment appears to contravene the results of Dr. Richardson, as far as it goes, in almost every single point. Thus, here was ammonia mixed with blood in a proportion a little less than that in which Dr. Richardson had found that it could not be detected by the test of fumes formed with hydrochloric acid, yet it was so detected in this instance with the utmost ease; and on the other hand, notwithstanding the obvious presence of ammonia in easily appreciable quantity, the blood with which it was mixed coagulated firmly and rapidly. Several other experiments follow, which demonstrate that very much larger quantities of ammonia may be added to blood and retained in close vessels without preventing coagulation. On one occasion, some blood falling on a flagstone of 40° temperature remained fluid for fully ten minutes after some of the same blood drawn at the same time and mixed with aqua ammoniæ in a stoppered bottle had become coagulated. From these experiments Dr. Davy draws the following conclusions:

"1st. That there are no indications afforded of the escape of volatile ammonia during the coagulation of the blood of the fowl, or of its presence in the blood. 2ndly. That the addition of ammonia in a notable quantity does not prevent coagulation. 3rdly. That a sudden reduction of the temperature of the blood, even when fully exposed to the air, has a greater influence in retarding the coagulation than the assigned cause of its fluidity, the volatile alkali, when added."

This is followed by one or two more experiments calculated to show that the salts of ammonia, like those of other alkalies, have the effect of retarding, and in large quantities of preventing, for an indefinite time, the coagulation of the blood, which will, however, afterwards take place if the mixture be freely diluted with water. These points had long ago been investigated and freely illustrated by the author in one of his former works.

Again, some further experiments made for the purpose of testing the loss of ammonia by evaporation, lead to the conclusion that were it present in the blood, no appreciable quantity could escape in the time usually found to elapse before coagulation takes place. Thus Dr. Davy finds that at ordinary temperatures the loss in ten minutes by this process is hardly appreciable, or amounts to ·01; and even when raised to a temperature of 95° to ·04. On another important part of the subject, too—namely, the solvent power of ammonia upon fibrin—Dr. Davy's results by no means agree with those arrived at by Dr. Richardson. Indeed, in a series of experiments upon this point, he never succeeded in getting the whole of the fibrin used dissolved in the ammonia solution, although the proportion of fibrin to the ammonia in his experiments is less than that used by Dr. Richardson. Upon this point Dr. Davy remarks, that—

"Even Dr. Richardson's own results, considering the proportion used of fibrin and ammonia, and the length of time required for the solution, seem to me in no wise to bear out the idea that the volatile alkali can be the solvent of the fibrin of the blood in its healthy state."

The theory of Brücke, that fibrin, as such, does not exist in the living fluid blood, and that as it occurs in coagulation it is a solid form of albumen arising from the composition and subsequent decomposition of albuminates, complicated and therefore unattractive as it is, is apparently more consistent with well-observed facts than the ammonia hypothesis. It is singular that the view of Brücke, to which we refer, should have been embodied in an elaborate essay composed for the very prize which Dr. Richardson's work obtained, and we believe first published in the pages of this Review.*

There is another objection of a theoretical character brought by Dr. Davy against ammonia as the solvent of blood-fibrin which is worthy of notice, although in so complicated a subject we cannot but look somewhat diffidently upon theoretical views. It is this. Saturating the blood with carbonic acid is found "rather to retard than accelerate its coagulation. Now, were its liquidity owing to the presence of a minute quantity of the volatile alkali, surely the acid gas, by combining with and neutralizing the ammonia, ought to have directly the opposite effect." This objection appears to us one which a chemical theory of coagulation is bound to explain.

* Medico-Chirurgical Review, vol. xix. p. 183.

Since the above remarks were written, Mr. Lister's Croonian Lecture on the Coagulation of the Blood has been published.* His further labours upon this subject have served to confirm him in his rejection of the ammonia theory, only they have made that rejection complete by compelling him to give up the opinion which he was formerly disposed to hold, that the escape of ammonia might be the cause of coagulation when it occurs out of the vessels of the body. Our space will not permit us to give an account of the steps by which Mr. Lister is led to this conclusion, and as the lecture is ere this doubtless in the hands of most of our readers, it is not needful for us to do so. We must nevertheless call attention to the constructive result of his inquiries, with which he has supplemented his rejection of the ammonia theory. The views, then, which Mr. Lister now holds, so far as we comprehend them, may be shortly stated in the following propositions:

1. That the cause of coagulation, as it occurs in the blood taken as a whole—i. e., liquor sanguinis and corpuscles together—is its contact with a foreign solid, including in this term the walls of the vessels or other tissues of the body when deprived in any way (as by inflammation) of their vitality.

2. That the walls of the vessels in the natural state have no active power to prevent coagulation, but are simply inert, and, as it were, indifferent to the blood.

3. That in order to coagulation, it is necessary that blood-corpuscles should be present. In respect of this last proposition, he refers to some experiments by Dr. Buchanan, of Glasgow, Dr. Schmidt, of Dorpat, and himself, which go to show that the fluid of a hydrocele or other dropsical effusion, ordinarily uncoagulable without the application of heat, may be made to coagulate by the introduction of some blood-corpuscles.

4. That the process of coagulation is brought about by a chemical influence or action of the corpuscles upon the liquor sanguinis, and that the presence of a foreign solid body sets up or favours this action much in the same way as the presence of a piece of spongy platinum favours the combination of oxygen with hydrogen. Whether this, the positive portion of Mr. Lister's view, is destined to maintain its ground, is a matter upon which we shall venture no predictions. It is due to Dr. Richardson to admit, that he has supported his own view with great industry and acuteness, and that it has, as Mr. Lister remarks, enormously increased the labours of subsequent investigation by its ingenious and attractive character.

Two other papers, somewhat widely separated in the volume though dealing with the same subject, must be noticed before we conclude our review of the very interesting work before us, these are those upon the eggs and growth of birds—viz., No. 42 and a part of No. 15; the latter of these (pp. 157–61) contains some very original and important observations upon the relative rapidity of growth, and the comparative

* Lancet, Aug. 8th and Aug. 15th, 1863.

size of the egg in those kinds of birds in which, as for example in the swallow tribe, the young are entirely sustained at first by the parents, and those like the turkey and common fowl, whose young begin from the very first to provide for their own sustenance. Thus it would appear that the young of the common house-martin, which is fed by its parents until able to take wing, reaches its maximum weight almost as soon as it can leave the nest. On the other hand, the young turkey, rapid as is its growth, has not attained more than about half its full weight in four months from the time at which it leaves the egg. Again, in comparing the size of the egg, Dr. Davy comes to the conclusion that there is a constant relation between it and the habits of life of the young bird. Thus, the eggs of those birds whose young have to provide for their own sustenance, and whose growth is comparatively slow, are always found to be larger than those of the carnivorous or insectivorous kinds; and this probably because the young of the former classes of birds are more dependent for nourishment on the residual included yolk than are those of the latter. Though, no doubt, the above is the general law, there is also room, in Dr. Davy's opinion, for the effect of certain modifying circumstances; as, for instance, the time of year at which the bird is to be hatched, or the climate which it is destined to inhabit. Thus, he asks—

> "May not the eggs of the missel-thrush be so large, having to be hatched early in the spring? May not those of the turkey be smaller than those of the goose, their time of hatching being considerably later? May not those of the Cochin-China variety of the common fowl be smaller than those of any of our northern varieties—the former being the breed of a warm climate, where food, it may be inferred, is always abundant?"

In birds, moreover, as the above examples show, there does not exist the same relation, which appears to be the rule with mammals, between the length of life of the species and the time which elapses before the young attain maturity. The necessities of their existence do not permit this, and accordingly, as we have seen, the rapidity of their development is arranged in relation to totally different requirements.

The observations contained in the other paper which we have mentioned are of so miscellaneous a character that it is not easy to convey any clear notion of them in an article like the present. We have been struck, however, with the following curious confirmation of Lehmann's conclusion, that casein enters largely into the composition of the yolk of the egg. It appears that Dr. Davy having laid by and gradually dried the yolk of an egg with its membrane entire, found, after three months, that both in consistence and smell, and also in its behaviour with reagents, and its microscopical character, it bore a striking resemblance to cheese (pp. 432, 433.) The section which describes the air-vesicle and its functions, which Dr. Davy considers to constitute a kind of incipient respiration, will also be found well worthy of attention, as also that entitled "On the relations of the Ingredients of the Egg to the Bony Skeleton of the Chick." In this last Dr. Davy has given a solution of a point in physiology which the late Dr. Prout looked upon as an unsolved problem. The latter physiologist, as is well known, found less lime in the whole contents of the egg before development commenced, than exists in the bones of the chick at the end of that process. The only source remaining from whence this could be supplied was the shell, and this he was unable to look upon as really affording it, inasmuch as the membrane which lines it is non-vascular, and resembles epidermis. Dr. Davy objects not to Dr. Prout's reasoning, but to his facts, having found that on examining a chick in the act of hatching, and immediately after, there was a vascular membrane in immediate and close connexion with the shell. It appeared, moreover, that although the proper membrane of the shell was interposed, yet it seemed to have lost part of its substance, and to be thin and worn. Hence he believes that the shell *is* the source whence the lime found in the bones of the young animal is derived, and that the large supply of phosphoric acid, or phosphorus convertible into phosphoric acid, which is contained in the yolk, is required to combine with the lime so obtained, and thus aids in their formation. A full account of the examination made by Dr. Davy of the chick at the period of hatching is given in a previous paper in this volume, page 342.

We have endeavoured thus to give some examples of the various subjects concerning which the volume before us is full of curious and important information, and to enter more fully upon one which is pre-eminently a "subject of the day," and which, moreover, is one of the most important questions at present *sub judice*, in the whole range of physiology. Our readers will see that, from the wide range of subjects with which the work deals, and the fragmentary character of many parts of it, it is one to which it is not easy to do justice in a review. But it is not the less worthy of the attentive study of physiologists, and more especially of young physiologists, for not only does it abound, as we have elsewhere observed, with suggestions and hints which will serve as admirable guides for future investigations into the various questions with which it deals, but it affords that which should be most encouraging and most inspiriting to others, the reflex of a mind which, during a long life spent in professional labours, has been always ready to assimilate and to follow out a new idea; always fertile in methods of investigation, and never wearied in the application of them; which is as ready to apply strict tests and judicious criticisms to a theory propounded at the present day as to one put forward half a century ago. It is impossible to read this latest of Dr. Davy's contributions to physiological science without being struck with the genuine philosophical acumen with which he has seized upon the really important points in the most familiar and apparently trite phenomena, and by really careful and thorough observation of them has turned them to account as illustrating some of the secrets of nature.

Review XV.

Recherches Expérimentales sur l'Action Physiologique de l'Ipécacuanha. Par G. Pécholier, Professeur-agrégé à la Faculté de Médecine de Montpellier, ex-Professeur de Clinique Interne à l'École Préparatoire de Médecine d'Alger, &c.—*Paris*, 1862. pp. 53.

Experimental Researches on the Physiological Action of Ipecacuanha. By G. Pécholier, Professeur-agrégé to the Faculty of Medicine at Montpellier, Clinical Professor at the Preparatory School of Medicine at Algiers, Titular Member of the Academy of Sciences and Belles-lettres at Montpellier, &c.

In the employment of remedies intended to modify those simpler and more intelligible phenomena of disease which, in contradistinction to complex morbid conditions, may be characterized as morbid processes or acts, the therapeutic action of these remedial substances will not, it is true, be found exactly to correspond to what is known of their physiological action (as discovered by observation and experiment on man and animals); still, in the greater number of instances, at least, there is a strong analogy to be observed, the vital facts being of the same kind in man as in brutes; and even as regards disease, we may say that the therapeutic agents operate in the same vital sense as in health, the "vital ideas," to use an expression quoted by our author, being the same in both. Experiment may thus enable us to arrive at some conclusions, if we confine ourselves to abnormal acts of a simpler character, such as acceleration or retardation in the rhythmical movements of the heart, stimulating or calming the nervous system, exciting and diminishing the muscular contractility, promoting and checking the secretion of the glands, &c. On the other hand, as regards those more complex morbid conditions or states which constitute and typify disease, the action of medicines employed to combat them (including specifics and all of that class), receives far less elucidation from physiological experiment, as, indeed, their mode of action lies, for the most part, beyond the reach of physiological argument.

We have endeavoured, in these few words, to make apparent the intention evidenced in the present pamphlet, as to the spirit in which the author's experiments have been undertaken.

The very exclusive and faithful attention which has been given in this country to clinical observations leaves us more open to suggestion derived from the field of inquiry we have just indicated. The merit and value of such researches runs little risk of being over-estimated in the very practical age and country in which our lot is cast.

The properties of ipecacuanha as a vomit and expectorant have as yet, with regard to it, absorbed the greatest share of attention: its anti-dysenteric, anti-febrile, and other valuable qualities have also been sufficiently considered; it has been reserved for our author, however, to investigate its effects, not on this or that feature of disease, but on the entire living organism, and regarding emetine (first discovered by Pelletier and Magendie in 1817), as representing the single active principle of the drug, he has employed it for experiment in its soluble form of chloride, making use of a concentrated infusion, and of etherial, watery, and alcoholic extracts for correction and corroboration of results. Treated with ether, ipecacuanha yields an extract which has all the peculiar offensive properties of the drug upon the organs of smell, respiration, &c. These affect different people very unequally. M. Pécholier relates that ipecacuanha has sometimes with children the effect of throwing them, when administered, into strong convulsions of anger; and he seriously proposes that all ipecacuanha used in pharmacy should be subject to the abstraction of this volatile extraneous substance by means of ether, before offering it for sale. This etherial extract has none of the properties of emetine. The alcoholic extract and watery infusion have those qualities, and are identical with it in their action.

Opinions have been hitherto much divided as to the tonic and depressing powers of ipecacuanha. It seems to have been decided by our author that it is unquestionably of a lowering, enfeebling, and contra-stimulating nature. This is especially the case when the doses are large. Any reaction on the rabbits subjected to experiment never lasted more than two or three minutes, and seemed plainly due to fruitless efforts at vomiting. More important still are the results arrived at with regard to the pulmonic organs, over which the operation of the drug produces the effect of largely diminishing their circulation, blanching them visibly when under experiment. The previous researches of Pelletier and Magendie with emetine had shown an inflammatory condition of the lungs, with cerebral dulness (*assoupissement*), and visceral congestion, as the principal subjects of remark. It is noticeable, however, that the experiments of these early investigators were performed upon dogs, which are subject to the action of violent vomiting; whereas M. Pécholier has chosen rabbits as the subject of experiment, in which animals the act of vomiting is not realized, although the abdominal muscles and diaphragm often energetically contract. The experiments of Magendie and Pelletier, moreover, were performed with a view of merely ascertaining the toxic properties of emetine. Magendie is also generally accused of referring too much in his investigations to the effects of inflammation.

The results which M. Pécholier has obtained from his experiments with ipecacuanha on rabbits and frogs may be stated as follows:

I. A considerable diminution in the number and energy of the strokes of the heart and pulse.

II. A diminution as considerable in the number of respirations, and in the quantity of blood sent to the lungs.

III. A lowering of the temperature in mouth, axilla, and ear, with an increased, or at least an undiminished, temperature in the rectum.

IV. Constant efforts at vomiting; hyperæmia of the stomach and superior half of the intestine, and disappearance of glucose from the liver.

V. A diminution of the activity of the nervous system; paralysis of the sensitive nerves, while the function of the motor nerves and muscular contractility, without being quite abolished, are diminished.

The contra-stimulating power intimated is noted as being far less in degree than that which follows bleeding, or the administration of tartar emetic, and the rabbits recovered with great rapidity. This result naturally directs us to the therapeutic employment of ipecacuanha in hyperæmia of the pulmonic organs. Not a few physicians, chiefly of the school of Montpellier, have used it in this sense very extensively; among them we shall select M. Ressiguier, of Carcassonne:

> "Ipecacuanha," he says, "has been employed for a length of time in the hospital of Montpellier, where it is sometimes advantageously substituted for large doses of emetic tartar. We owe to Professor Broussounet this application of it as a contra-stimulant. Administered in the form of infusion, of the strength of 1·50 to 3 grammes of the drug, in a vehicle of 120 to 180 grammes, it possesses properties which may be compared with those of antimony administered in large doses. Like it, ipecacuanha diminishes the intensity of the pneumonia, either by its depressing action, or by occasioning violent diaphoresis, which brings about a crisis in the malady."*

These views of M. Ressiguier are partaken by M. Delioux, who has published an interesting work on the subject.

In that particular cycle, whether of professional bias or of constitutional change, in which we at present stand, these ideas, we are inclined to think, will not fall unprofitably upon the minds of our readers. If inflammation of the lungs can be controlled at a smaller expense to the system than by bleeding and antimony, a large amount of wavering opinion might, we believe, be conciliated by the general use of ipecacuanha, experimentally, in this direction.

An English physician, Higginbottom, in 1814, signalized the excellence of ipecacuanha in favouring the absorption of that sanies which hepatizes the lung; but he considered it (erroneously, according to our author) as a tonic.†

Other physicians who have advocated its liberal employment (and notably M. Cade, as médecin-en-chef of the Hôtel Dieu, at Avignon), have taken the same view of its tonic mode of action. In his opposition to this notion our author does not consider its action as a contra-stimulant identical with that of tartar emetic. Used in minute doses, M. Pécholier seems to doubt whether its operation extends to the more important processes of the economy such as were made evident in his experiments.

When larger doses, amounting to one gramme of ipecacuanha, or thereabouts, were repeatedly given, "occasionally," he says, "no symptoms were manifested;" if vomiting ensues it is followed by a temporary excitement. "There is generally," he says, "vomiting," which finally ceases, and the remedy "is excellently borne." At other

* Compte Rendu des Maladies Aigues et Chroniques qui ont été observés dans l'Hôpital Général de Montpellier en 1849. Montpellier, 1850, p. 66.

† De l'Ipecacuanha dans la fluxion de poitrine muqueuse. (Bulletin Général de Thérapeutique, tome l. p. 251.)

times this tolerance was not realized, each teaspoonful brought on nausea and incessant vomiting, with weakness and prostration; and these symptoms were so disagreeable as to cause suspension of the remedy.

M. Pécholier seems inclined to attribute the exsanguineous and pale state of the pulmonary tissue in the animals operated on by him to the diminution of the number of respirations under the influence of emetine, and also, in part, to the considerable flow of blood towards the gastro-intestinal tubes.

This, as regards the stomach, was always near the larger curvature, and extended to the pylorus and upper end of the jejunum. But whatever the cause, he holds that, for the time, a true "bleeding" of the lung takes place, and that the drug, by a similar licence of speech, might be called a lung-specific.

How far in the human subject this benefit to the lung would be modified by the act of repeated vomiting, is a question we think by no means resolved by the experiments of M. Pécholier upon rabbits, considering that these creatures escape the complication. His experiments only prove that, introduced into the circulation, ipecacuanha has a certain physiological effect upon the lungs; and the value of this information must be subjected to the test of clinical experience.

We refer to the second experiment to show that the efforts at vomiting which occurred in the rabbit under experiment, were much less after the second dose of emetine than after the first, and less strong after the third than after the second; so that, with failing power, a toleration may tend to be effected.

The question will readily suggest itself, whether ipecacuanha might not be administered by the rectum or by the skin. Both of these methods have been tried in the experiments before us. In the ninth experiment an infusion used as enema was speedily returned, and only slight constitutional results were obtained. They were greater when emetine was applied in solution to the freshly-shaven skin of the rabbit; although in this animal the skin absorbs but badly. The sixteenth experiment shows violent inflammation, extending to the brain, from insufflation of powdered ipecacuanha to the eye, with rapid destruction of the visual organ.

When ipecacuanha or emetine do not bring about a rapid dissolution their effects upon the circulation are very fugacious. Thus, animals in a perfect state of collapse, having but half the number of normal heart-pulsations, recovered that number in half an hour, three quarters of an hour, or at least in an hour. When the rabbits died, it was with a rapidity varying from eight to twenty-five minutes. The dogs experimented on by Pelletier and Magendie died much later, after a period of fifteen hours.

From tables which our author has constructed it would appear that the temperature of the mouth, axilla, and ear, is always lowered under the action of ipecacuanha and emetine. The most considerable diminution noted is 2·7 Centigrade for 5 centigrammes of emetine. More frequently a diminution of about one degree occurred.

The diminution of animal heat was the most persistent feature of constitutional derangement, even after an hour it had frequently not recovered its normal standard.

On the other hand the temperature of the rectum never declined; it either remained stationary or advanced. The greatest increase was 0·7 Centigrade. The hyperæmia of the lower intestine remarked in the autopsies, sufficiently accounts for this change of temperature.

From doses of 5 milligrammes to 5 centigrammes of emetine, and 20 to 80 centigrammes of ipecacuanha, there was a deviation in the number of respirations ranging from 140 maximum to 90 minimum. When death did not supervene, the normal standard was regained within an hour.

It remains to state that the character of the urine did not appear to vary under the administration of ipecacuanha or emetine.

Not the least interesting parts of our author's researches are those which he has undertaken upon frogs, with a view to ascertain the action of ipecacuanha and emetine upon the nervous system. Is that action primitive or secondary, or in what direction does it operate? His experiments serve to show that the effects of emetine on the nerves somewhat resemble those from strychnine, and may be contrasted with those from woorara, which kills the motor nerves, and to those from sulpho-cyanuret of potassium, which destroys muscular contractility.

He concludes that ipecacuanha reduces in a rapid manner the energetic action of the nervous system, and determines considerable collapse. The paralysis produced by it falls chiefly upon the sensitive nerves, while the motor power and the muscular contractility are only weakened, but are not abolished.

Much interest attaches to the fourteenth and fifteenth experiments, in the first of which a solution of chloride of emetine was applied to the surface of the heart in a frog. The cardiac pulsations fell from 120 to 90 beats in ten minutes' time, and in twenty minutes to 62 beats. After thirty-three minutes the pulsation ceased entirely; whereas the exposed heart of another frog, prepared for counter-experiment, continued to act for one hour. In the fifteenth experiment, the relative periods were twenty-eight minutes against one hour and a quarter. In further experiments, in which the hearts were detached from the bodies, those which were subject to the action of chloride of emetine constantly ceased to beat in half the time occupied by the duration of pulsation in those reserved for counterproof. We note the above experiments as highly interesting, considering the variety of results which ensue from the application of different substances to the heart when thus experimented on; and they seemingly confirm the conclusions of our author, that the action of ipecacuanha diminishes considerably the number and energy of the heart-pulsations.

Review XVI.

Transactions of the Obstetrical Society of London for the Year 1862. *With a List of Officers, Fellows, &c.* Vol. IV.

The Obstetrical Society, which has been established only within a very recent period, is now in a flourishing condition, for in the Annual Report in 1862, it appears that it then comprised 482 members, of whom 82 joined it during the previous year; and after all the preliminary expenses were paid, a balance of nearly 500*l.* remained ready for investment in the hands of the treasurer. During the four years of the existence of the Society, three volumes of 'Transactions' had already been published, and the present is the fourth of the series. The Society includes among its fellows practitioners in the East and West Indies, Australia, New Zealand, Constantinople, and many other places in different parts of the world; and an obstetrical library is in course of formation.

The 'Transactions for 1862' comprise a list of the officers and fellows of the Society, a selection of the papers read at the ordinary meetings, and in connexion with these an abstract of the subsequent discussions; and there are also an index of the contents of the volume, and a catalogue of the books already contained in the library of the Society. The papers are rather numerous, but, in general, short, and the reports of the discussions are still shorter, only the chief points of the speeches being recorded. The subjects are almost all of a practical character, consisting in many instances of the records of cases, and in very few are speculative topics introduced or theoretical views insisted upon. The nature of the contents, however, will be best understood by the following abstract of the forty-two cases of which the volume mainly consists:

I. *Case of Spina Bifida.* By Graily Hewitt, M.D. Lond., M.R.C.P.—This is a short case, the subject being a female child, presenting a depression arising from a defective condition of the bony arch of the sacrum, and a smooth, soft, and compressible tumour rising from the cavity. The general condition of the infant seemed to be good, and means were taken to protect the tumour from external pressure by pads applied on each side; but without any obvious reason, the child became weak after six days from birth, convulsions set in, and death ensued on the twelfth day. An engraving is given, showing the relations, proportionate size, and outline of the tumour.

II. *Protracted Retention of a Blighted Ovum.* By R. Uvedale West, M.D.—This is also a very short communication on the case of a woman who expelled a blighted ovum on the 3rd of February, 1862, having become pregnant (as she believed) in the early part of February, 161. At the time when she ought to have quickened, she began to have frequent hæmorrhages, until the full period in November, when the hæmorrhage became constant. Dr. West thought that the embryo probably perished at the period of quickening, and that at the same time the ovum ceased to grow, although retaining until the full period of nine months an amount of vitality enabling the membranes to blend themselves together into the fleshy, thickened mass, which was at last expelled, and which Dr. West exhibited to the Society.

III. *Discoloration of the Skin of the Forearms and Hands during Pregnancy.* By J. G. Swayne, M.D.—The subject of the case was a blonde, who in her fourth pregnancy exhibited a very general discoloration of the skin of the forearms and hands, much more marked on the dorsal than on the palmar aspect. It was found, on inquiry, that the skin had been similarly affected in each preceding pregnancy, and that the dark colour first appeared about the end of the third month, and increased *pari passu* with the development of the areola, until it attained its height at the time of labour, after which it began to diminish in intensity, and in about three months it had entirely disappeared. The same was the case in the present instance. A plate is given, showing the dark patches on the arms and hands, the palmar surface of the latter, however, being not at all affected.

IV. *Difficult Labour from Locked Heads.* By Henry Raynes, Esq. —In this case, which occurred twenty-four years ago, the labour was impeded by the circumstance that the heads of the twin children were locked together. Mr. Raynes, finding that the body of one child (a footling case) was born, and that its entire expulsion was prevented by the cause just mentioned, divided the body from the head by excision, after which the second child was born alive by the unaided powers of nature. The head of the first child then descended, and was removed by a little manipulation. The second child expired soon after birth.

V. *Case of Retained Menses of Two Years' Duration, caused by Atresia Vaginæ; Puncture of Uterus by Rectum; Recovery.* By I. B. Brown, Esq., F.R.C.S.—The patient was a girl of fifteen years of age, who had enjoyed good health until about two years ago, when she began to suffer much pain at every monthly period, unaccompanied by any appearance of the menses. When examined on admission, the uterus was felt externally as large as the gravid organ at four months, and on introducing the finger into the rectum, it was felt bulging and prominent; there were no signs of a vagina, but all the other external parts were perfect. Mr. Brown tapped the uterus through the rectum with a curved trocar and canula, and about an ounce of treacle-like fluid came away, the canula being left in the wound. About six weeks after the operation, a thick muco-gelatinous discharge, coloured with blood, was evacuated from the rectum, and was no doubt the menstrual fluid. Since this occurrence the patient improved in all respects, but it was stated that the future progress of the case would be carefully watched.

VI. *On the Uterine Douche as a Therapeutic Agent, with Exhibition of a New Instrument.* By GRAILY HEWITT, M.D., M.R.C.P.—The apparatus was constructed under Dr. Hewitt's directions by Mr. T. H. Savory, and consists of an india-rubber vessel folding up like a "Gibus" opera-hat, from the bottom of which a long flexible tube conveys the fluid. When opened out, the reservoir holds nearly a gallon of water, the whole of which can be used at one operation. The water flows out merely by gravitation, the apparatus being placed a foot or two higher than the seat or couch on which the patient is reclining, and the rapidity of the stream is regulated by a stop-cock. Irrigation of the os and cervix uteri, by the aid of this instrument, is of very great service in all cases in which there is an undue fulness of the uterine vessels or defective tonicity of the muscular fibres generally, as profuse menstruation connected with a lax condition of the vessels of the uterus, chronic leucorrhœa, &c. In the discussion which followed the reading of this paper, Mr. Baker Brown observed that the principle of Dr. Hewitt's apparatus had long been known, and that he had used a similar instrument himself for several years, but Dr. Hewitt replied that although the principle of the instruments was the same, yet in the adaptation of the principle, his own instrument was far superior; and Dr. Tyler Smith expressed a similar opinion.

VII. *On the Thrombosis and Embolia of Lying-in Women.* By ROBERT BARNES, M.D. Lond., F.R.C.P.—Dr. Barnes remarks that the recent researches made upon the subject of embolia and thrombosis will probably throw much light on the pathology of some hitherto ill-understood diseases incident to childbirth. He relates a fatal case, occurring in his own practice in the year 1854, in which, although no post-mortem examination was permitted, there was very little doubt that the death was caused by the formation of a coagulum in the right external iliac or the femoral artery, causing mortification of the corresponding limb as effectually and rapidly as if the artery had been tied. The lady who was the subject of the case had an easy labour, but about a week after delivery she experienced a chill, and suffered a series of anomalous symptoms not referrible to inflammation or fever, but terminating in mortification of one leg, general prostration, and death. Dr. Barnes, after relating this case, arranges in a tabular form a series of cases which he has collected together from several sources, of thrombosis and embolia in lying-in women, with the post-mortem examinations, where they could be obtained. In the first table he gives the particulars of fifteen cases of systemic arterial thrombosis or embolia; and in the second, fourteen cases of pulmonary thrombosis or embolia, all in lying-in women. In commenting on the first series of cases, Dr. Barnes remarks that, when the circumstances are fully related, there appears to be generally dyspnœa, syncope, and irregular action of the heart, and pain of the most excruciating character is a diagnostic sign of the greatest importance. In all the cases, one or more of the limbs was affected, and in two cases amputation was resorted to, in one instance with success. As to the second series of cases, or those in which there was pulmonary thrombosis or embolia, it was remarked that clots generally existed in the peripheral veins, as the crural, iliac, hypogastric, or uterine, and it would appear that these veins were affected in the first instance, and that the lungs and their vessels were affected subsequently. It is therefore reasonable to conclude that in these cases there is in the first instance a dyscrasia of the blood immediately proceeding from the puerperal condition, which is favourable to the production of clots in the uterine veins and the veins of the lower extremities. These clots are carried by embolism to the right side of the heart, causing either rapid coagulation in the right cavities, followed by a similar process in the larger pulmonary arteries, or minute emboli are carried into the finer divisions of the pulmonary artery, causing lobular pneumonia, and ending in death, or perhaps in recovery. With respect to the treatment of such cases, not much is as yet known, but the prophylactic measures to be adopted are such as are calculated to support the general health of the patient, care being taken to avoid all sources of mental or bodily disturbance, to encourage lactation, which maintains the healthy contraction of the uterus, and to give adequate nourishment. The treatment of the disease itself consists in the administration of bark, stimulants, and the mineral acids, especially the nitro-hydrochloric, on the efficacy of which latter Dr. Barnes particularly insists.

VIII. *Note on the Broncho-Pneumonia of Lying-in Women.* By Robert Barnes, M.D.—Lying-in women are liable to a peculiar form of broncho-pneumonia, which may be regarded as a symptom, or a part, of puerperal fever. In these cases there is a considerable degree of prostration, there is expectoration amounting to bronchorrhœa, depletion is not borne, and the treatment consists in the administration of cinchona, senega, or serpentaria, in decoction or infusion, with ammonia, and, at a later stage, with nitro-hydrochloric acid, blisters to the chest, good nourishment, and stimulants. The form of broncho-pneumonia here alluded to is not to be confounded with that which is the result of capillary embolia of the pulmonary arteries, although they are so far alike that they are both set up by offending matter brought to the lung-tissues by the circulating blood.

IX. *On Ovariotomy, the Mode of its Performance, and the Results obtained at the London Surgical Home.* By I. Baker Brown, Esq., F.R.C.S.—Mr. Brown commences his paper by observing that the operation of ovariotomy is not only perfectly justifiable in properly selected cases, but that its mortality is very much less than that which occurs in many other capital operations of surgery. The mortality has very much diminished of late years, owing to the greater care taken in the performance of the operation. Thus, by the statistics collected by Dr. Clay, of Birmingham, it appears that of the cases of complete ovariotomy 212 recovered and 183 died; but, speaking generally, Mr. Brown considers that at the present period the recoveries are two to one, or the deaths are about 33 per cent. The greater success of the operation in the present day is due, not so much to the method of performing the operation itself, as to the precautions taken before the operation, and the careful guidance of the after-treatment. In the first place, the surgeon must be satisfied that the tumour is ovarian, and that it is not cancerous; the latter, however, being a very difficult, and sometimes an impracticable point of diagnosis. The existence of adhesions, although formerly considered as a reason for abandoning the operation, is not an insurmountable obstacle to its performance, as they may be either broken through with the écraseur or the hand, or they may be divided by a knife or scissors after they have been tied. If any of these adhesions are to be tied, Mr. Brown suggests that silver wire should be used instead of thread or twine, and the ligatures allowed to remain within the abdominal cavity, simply cutting them off short and close. The details of the operation have been previously described in Mr. Brown's book on the 'Surgical Diseases of Women.' In securing the pedicle of the tumour, Mr. Brown now always uses the clamp in preference to the ligature, taking care to remove it in a few hours if the pedicle is very short and the drawing gives pain to the patient. Mr. Brown objects to the plan lately introduced of placing the patient in an arm-chair during the operation, and he deprecates the use of a sponge within the cavity of the abdomen, thinking it better to leave what cannot be removed with the hand or a piece of flannel. The patient should be placed in a warm bath several times before the operation, and tonics should be administered, and on the morning of the operation the bowels should be opened. The temperature of the patient's body should also be carefully regulated. After the operation, opium may be administered, but only when great pain is present; and if there is any tendency to sickness, it should be given by the rectum. If pain should continue, even after taking the opium, and the patient be troubled with flatus, fomentations should be applied over the abdomen by means of flannels steeped in hot water and freely sprinkled with turpentine. For the first twenty-four or forty-eight hours nothing should be given in the way of food, except barley-water, iced drinks, &c.; but on the third day a mutton-chop and beer may be allowed in many cases. Mr. Brown then makes an analysis of the cases on which he has operated, and among other conclusions he finds that a better chance of success is held out to the female who undergoes the operation before she is thirty. In analyzing the causes of death in the unsuccessful cases, which were six in number, he considers that in four the fatal result could not fairly be attributed to the operation itself. In one of these four the patient's general health was very much impaired, but she was very urgent to have the operation performed. In another case, the patient was a hard drinker, and had disease of the liver. In the third, there was an abscess in the pelvis and scirrhous ulceration of the duodenum; and in the fourth there was disease of the heart, and a violent attack of diarrhœa carried off the patient eight days after the operation.

Mr. Brown appends a tabular history of the 19 cases alluded to in his paper, giving in separate columns the date of the operation, the name and age of the patient, the duration and progress of the disease and the condition of the patient before the operation, the length of the incision, the presence or absence of adhesions, the nature of the tumour, the events of the operation itself, the position of the pedicle, and the result of each case.

In the discussion which ensued after the reading of the paper, Mr. Spencer Wells objected to the use of silver wire for tying the adhesions, and to the practice of leaving a portion of the wire in the abdomen; he also preferred a ligature of wire rope to the caliper clamp, and he entirely disagreed with Mr. Brown in employing flannel instead of sponge for wiping out the peritoneal cavity. Dr. Tyler Smith advocated the semi-recumbent posture, in preference to the horizontal, for the patient during the operation, inasmuch as it facilitated the removal of fluid from the abdominal cavity, was convenient to the operator, and certainly did not produce syncope. He also preferred sponges to flannel in removing fluid and coagula. Mr. Brown, in reply to the foregoing remarks, still adhered to the views and the particular practice which he had advocated in his paper; and, in answer to a question from Mr. Spencer Wells, he stated that the whole number of cases on which he had operated were thirty-eight, with nineteen recoveries, and the operations were more successful at the London Surgical Home than in private practice.

X. *Description of a Specimen of an Ovum in Ovo.* By Robert Barnes, M.D.—In this case there were two hen's eggs one within the other; the inner one was of the size of an ordinary pullet's egg, surrounded by the albumen of the larger egg, the yolk of which lay somewhat compressed in the big end of the shell. The physiological formation of this double egg is not easy to be explained, as the shell of an egg is added near the cloaca.

XI. *A Case of Ovariotomy, and a Reply to a Statement respecting it made by Mr. Baker Brown at the last Meeting of the Society.* By T. Spencer Wells, Esq., F.R.C.S.—This case was introduced only to controvert a statement made by Mr. B. Brown, and was otherwise of no particular interest.

XII. *Complete Occlusion of the Os Uteri, with Retention of the Menses after Difficult Labour.* By John Hall Davis, M.D.—The patient was a lady, who, after a difficult labour with her first child, suffered from a slight rent in the perinæum, which was united by a plastic operation. Fifteen months after delivery she was placed under Dr. Davis's care, having never menstruated since that event, and having suffered much pain and inconvenience. The os uteri appeared to be completely obliterated, and it was afterwards ascertained that the uterus was much enlarged, the enlargement resembling pregnancy at the fourth month, and there was a soft and rather elastic prominence of the uterus at the top of the vagina, with distinct fluctuation. Dr. Davis therefore punctured the part which was most prominent, and a portion of the retained catamenial fluid came away; but on enlarging the aperture, the collected menses readily escaped, and continued to flow for the next two days. The same operation was repeated on subsequent occasions, but owing to want of care on the part of the patient, she suffered from pyæmia about six weeks after the first operation, but at last recovered, and the menses have since returned and continued in a regular manner. Dr. Davis thinks that the occlusion was the result of inflammatory action, caused by the long-continued pressure of the head of the child in her difficult labour.

XIII. *On the Nature, Treatment, &c., of Puerperal Peritonitis.* By Joseph Thomas Mitchell, F.R.C.S.E.—Mr. Mitchell's experience extends over a period of forty years; his practice comprising 4349 recorded cases of labour, among which he has been called upon to treat twenty-seven cases of well-marked puerperal peritonitis, and of this number only four terminated fatally. It is remarked, however, that none of these cases were epidemic, nor traceable to contagion, and they were all attended at the patient's own residence. Mr. Mitchell's mode of practice in this disease was derived from a lecture delivered on puerperal fever by the celebrated Dr. Armstrong, who, as is well known, advocated bleeding in fevers. After attending the lecture alluded to, Mr. Mitchell was called to attend a woman labouring under puerperal peritonitis, and he adopted Dr. Armstrong's plan with perfect success. Since that period he has always adopted and advocated the same treatment. He bleeds to a considerable extent—generally only once, but sometimes twice, or even thrice—and follows up the bleeding by a pill, and almost poisonous doses of opium, and the application of fomentations of hot water and turpentine, or large sinapisms over the abdomen. He disapproves the administration of purgatives by the mouth, and he relieves the bowels only after four or five days by an injection of gruel and turpentine. In the four fatal cases which occurred among the twenty-seven, Mr. Mitchell explains the result by the occurrence of accidental complications or errors in treatment. Light nutritious diet is recommended, but no wine or other stimulant, during the acute stage of the disease.

In the discussion which followed the reading of this paper, it was admitted that the treatment recommended by Mr. Mitchell might be serviceable in sporadic cases, such as he had described, but was totally inapplicable in the epidemic form of the disease; and Dr. Braxton Hicks thought it very difficult in some cases to determine whether peritonitis really existed, although the symptoms very much resembled it. Such cases were sometimes instances only of cellulitis, not involving the peritonæum, but probably produced by inflammation of the lymphatics which pass freely within the folds of the broad ligament.

XIV. *Note on the State of the Internal Surface of the Uterus after Delivery.* By J. Matthews Duncan, M.D.—Dr. Duncan confirms the opinion offered by William Hunter that the decidua is an efflorescence of the internal coat of the uterus, that one stratum of it is always left on the uterus after delivery, and that at no time during pregnancy or after it is the decidua thrown off in mass or the muscular tissue denuded. Even at the site of the placenta, when it might be supposed that, after delivery, the surface of the uterus would be completely denuded, Dr. Duncan has convinced himself that only a small superficial portion presents a caducous mucous membrane, and that on the rest of that surface the thick mucous membrane could be easily and plainly seen. Dr. Duncan also asserts, in opposition to some other physiologists, that the decidua is not thrown off during pregnancy and replaced by a new mucous lamina, but that the original mucous lining produces new layers of epithelium, which appear like a new membrane.

XV. *Case of Unsuspected Pregnancy and Labour.* By THOMAS HAWKES TANNER, M.D.—This was the case of a lady, forty-two years of age, who, although married, had had no children, and the cessation of whose menses was attributed by herself to the change of life. She was, however, seized in April, 1862, with violent pain in the stomach, and Dr. Tanner, on examination, discovered that the os uteri was dilated, and the head of the child was presenting. He announced the state of the case, but his statement was received with incredulity. In a few hours after the pains began, Dr. Tanner completed the delivery by the short forceps. The medico-legal bearings of this case are evidently important, and as the parties had no intention to deceive, the fact seems to be established that a woman may conceive, may go to the full term of gestation, and may be in labour for several hours, without having any suspicion of her real state.

XVI. *A Practical Inquiry into the Properties of Nitrate of Silver, with an Account of a New Instrument for its Use in Uterine Disease.* By ROBERT ELLIS.—Mr. Ellis considers that nitrate of silver is a true escharotic, of a low power of penetration, and he has proved this view experimentally by observing the behaviour of the web of a frog's foot under the microscope, when treated with minute portions of this salt. A distinct eschar was produced, and a condition of inflammation and congestion was set up around the dead portion. The use of nitrate of silver in obstetric practice is founded upon its astringent and stimulant qualities, and upon the healthy and kindly-healing surface which results from its application. But in its ordinary form this caustic is almost unfit for full and free application to the neck of the uterus, owing to its great brittleness; and Mr. Ellis has endeavoured to remedy this quality by various expedients, as by mixing up with the fused material a small quantity of long asbestos fibres, on the same principle as hair is mixed with common mortar. But he has lately proposed a plan which, in his opinion, removes all the difficulty, and it consists in giving a solid and unyielding support to the cylinder of brittle caustic by passing a metallic pin through its centre. To effect this object, the caustic must be cast hollow, and Mr. Ellis has devised a simple mould by which this end may be accomplished. The caustic holder, although simple in its construction, differs in many respects from the instruments at present in use, but its peculiarities, which exhibit great ingenuity, can hardly be explained without the aid of the diagrams with which Mr. Ellis's paper is illustrated.

XVII. *Case of a large Fibrous Tumour impeding Delivery.* By HENRY M. MADGE, M.D.—The patient was a primipara, aged twenty-seven, who without having suffered any previous illness, and having gone her full time, was found to have her pelvis occupied by a large round tumour, which completely impeded delivery. A trocar was introduced, and a small portion of fluid drawn off, and subsequently the tumour was pushed upwards, and the child was brought down by the buttock, with the aid of a blunt hook. It showed some faint signs of life, but never breathed. The patient went on well for a short time, but died of peritonitis on the third day after delivery. On a post-mortem examination, the tumour was found attached by a large pedicle to the posterior aspect of the fundus uteri, and the external surface of the uterus was studded with small fibrous tumours, about the size of walnuts. In some remarks upon this case, Dr. Tyler Smith observed, that when fibrous tumours of the uterus existed below the pelvic brim, the best plan of treatment was to induce premature labour. He had once been called to such a case, when the child was extracted with great difficulty, but the patient ultimately did well, and the tumour disappeared.

XVIII. *The Polyptrite; a New Instrument for Crushing the Necks of Uterine Polypi.* By J. H. AVELING, M.D.—Dr. Aveling described this instrument, and gave a drawing, showing its construction, and stated that it possessed this advantage over the polyptome—that it made a crushed wound, which was less likely to bleed than an incised one.

XIX. *On a New Description of Nipple Shield and the Treatment of Sore Nipples.* By H. COOPER ROSE, M.D.—The shield is made of glass with a cone-shaped appendix, or mouth-piece, over which is tied a calf's teat, a piece of wash leather, or an india-rubber nipple. Together with the use of the shield, Dr. Rose recommends the application of a saturated alcoholic solution of gum benzoin and glycerine, in equal proportions, to the mother's nipples, after every time that the child is nursed.

XX. *Case of Double Uterus, with Simultaneous Gestation.* By HENRY GRACE, M.R.C.S.—The patient was in her fourth pregnancy, and after she had been in labour fifteen hours under the care of a midwife, it was found that there were two ora uteri, both dilated, a child's hand presenting at one os, and a child's head at the other. There was a septum between the first and second os uteri, about half an inch in thickness. One os uteri, from which the hand presented, was then dilated, and the child was delivered by turning; and then the second os was dilated, and the second child was also delivered by turning, but, though born alive, it did not live many hours. Both children were females, of the same size, and apparently of a like age. The woman recovered without any difficulty, and having been examined since her convalescence, the neck of each uterus was found to be much in the same position as at the time of her confinement, one being anterior, and being much more readily reached than the posterior os.

XXI. *On a Case of Presentation of the Right Arm and Shoulder; Delivery by the Natural Powers, or Spontaneous Evolution.* By RICHARD HODGES, M.D., F.R.C.S.—The patient, who was in labour with her fourth child, and in whom the os uteri was fully dilated, experienced but few pains for three days, when a child's hand was felt high up and presenting. Dr. Hodges attempted to bring down the feet, but he was unable to do so from the firm manner in which the uterus embraced the fœtus. By the continuance of the pains, however, the presentation was forced lower and lower, and, by spontaneous evolution, the breech at last presented, and the child was expelled as in an ordinary breech presentation. The infant was above the average size, and appeared to have been dead several days.

XXII. *Four Additional Cases of Ovariotomy.* By W. TYLER SMITH, M.D., F.R.C.P.—These were all polycystic cases, both ovaries being diseased in two, and only the left ovary in the two others. Both of the two former were followed by a fatal result after the operation: the two latter were operated upon and recovered. Up to the date of the paper, Dr. Tyler Smith had performed ovariotomy in twelve cases, of which three have died, and nine have perfectly recovered; one of the latter has since become pregnant. It was remarked as an important fact that, in one of Dr. Tyler Smith's successful cases, the pedicle was tied with a silk ligature, the pedicle and the ligature were cut off as short as possible, and both then dropped into the abdomen. The wound healed by the first intention.

XXIII. *The Vessels concerned in the Production of Phlegmasia Dolens.* By WM. TILBURY FOX, M.D. Lond.—The author of this paper, after reviewing and criticizing the various theories invented to explain the pathology of phlegmasia dolens, arrives at the conclusion that it consists essentially of an obstruction of the main lymphatic vessels in the vicinity of the swelling. He considers that it is a local, not a constitutional disease, that blood-poisoning is not essential to its existence, and that phlebitis, however produced, cannot give rise to it, but only to œdema. The obstruction of the lymphatic vessels may be the result of extrinsic pressure, as, for instance, of tumour, or of thrombosis, or of inflammatory changes in the vessels themselves. Phlegmasia dolens may occur in blood-poisoning, but forms no necessary part of it, and the effect of venous obstruction may intensify the general swelling in phlegmasia dolens, though the disease does not depend upon this obstruction. The experiments of Dr. Mackenzie on this subject are considered inconclusive by Dr. Fox, and inconsistent with clinical facts. In the few recorded cases of post-mortem examination in phlegmasia dolens, no trace of venous disease has ever been found, and if it be really a blood-disease, it is difficult to explain its low mortality. Dr. Tyler Smith expressed his opinion that the symptoms of phlegmasia dolens might be produced by obstruction either of the veins, arteries, or lymphatics, but that the most common cause was lesion of the veins. From this opinion, however, Dr. Fox dissented, and still adhered to his own view, that the lymphatics were the chief seat of disease.

XXIV. *A Report on a Twin* (?) *Abortion exhibited to the Society by Dr. Langmore.* By GEORGE HARLEY, M.D., and T. H. TANNER, M.D.—In this curious case, the patient was the mother of nine children; and after having been pregnant for about four months, she was seized with labour pains, and a fœtus, which had apparently been dead for some time, was expelled. As serious hæmorrhage commenced, Dr. Priestley, who was called in consultation, removed the placenta, which corresponded in size with the fœtus expelled; but as this proceeding did not check the flooding, Dr. Priestley again introduced his finger into the uterus, and succeeded in extracting a second bag of membranes, containing an embryo of about five or six weeks. This embryo appeared fresh and perfect, and not at all atrophied. Dr. Harley and Dr. Tanner, after examining both the preparations, arrived at the conclusion that in all probability this was a case of superfœtation, an occurrence which does sometimes happen. If this were not the case it is difficult to explain how the four months' fœtus had been dead some time before it was expelled, while the six weeks' fœtus was fresh and healthy. If the conception of both had been simultaneous, then it must be believed that the younger fœtus had died at six weeks, and yet was preserved fresh till the fourth month, while the older fœtus was atrophied and decomposed, although it had been dead only a few days.

XXV. *Enormous Development of Hydatids in Omentum, &c., simulating an Ovarian Tumour.* By WILLIAM NEWMAN, M.D.—The patient was a married woman, aged forty-three, who had had three children; and in the year 1857 she began to perceive some swelling in the abdomen, which slowly but steadily increased in size until the year 1862, when she died from an attack of acute peritonitis. On a post-mortem examination, it was discovered that the abdomen was very large, and the great omentum was seen stretched over a mass of glistening cysts on both sides; the diaphragm was pushed up by two masses of hydatids lying upon the liver, and a large mass of hydatid cysts embraced the uterus, left ovary, and bladder. The case, during life, was considered to be one of ovarian disease; and the post-mortem examination showed that an absolutely correct diagnosis during life would have been almost impossible. The size and character of the swelling were exactly those which might be presented by a multilocular ovarian tumour.

XXVI. *A Case in which Air was expelled from the Vagina.* By GEORGE HARLEY, M.D.—In this case the expulsion of air was accompanied by a loud noise, and, although odourless, was attended with great personal discomfort. It began at a catamenial period, lasted for about eighteen months, and recurred with increasing severity at each period. The patient was married, and the mother of three children, but her health was bad, the uterus was retroverted, and the posterior lip of the os was ulcerated. The vagina was carefully examined in order to discover if any communication existed between it and the rectum, but none was found. Dr. Harley, by means of a male catheter, ingeniously fitted for the purpose, ascertained that the air did not escape from the uterus, but from the vagina; and he also found that the vagina sucked in and expelled the air by spasmodic action. The result of the case was, that the retroverted uterus was restored to its normal position, and under a course of strong nervine tonics, together with astringent vaginal injections, the spasmodic affection was completely arrested in two or three weeks. In the discussion which ensued it was stated that the absorption and expulsion of air by the vagina was not a very uncommon occurrence.

XXVII. *Clinical Memoir on Turning in Cases of Disproportion.* By Alfred H. M'Clintock, M.D., F.R.C.S.I.—In this paper Dr. M'Clintock discusses the very important question of turning the child in cases of pelvic deformity, and he considers that it can only be determined by clinical observation. He therefore communicates his own experience of the practice in question. He has performed version in seventeen cases in the Dublin Lying-in Hospital, on account of disproportion between the child and the pelvis. The actual number of patients operated upon was only twelve, in consequence of three having been more than once under treatment; and of all the operations one only was followed by the death of the mother, and even this death was not fairly to be attributed to the operation, for she died of puerperal fever, which happened to be prevalent at the time. Of the seventeen children nine survived birth—that is to say, they lived and breathed. As to the effects of the operation on the child, Dr. M'Clintock thinks that the value of this measure has been exaggerated by its advocates, and that it is only where the degree of pelvic contraction—or, more properly, of disproportion—is very slight, that it can be resorted to with any certainty of saving the fœtus. Hence he would not deliberately advise a patient who had a decided contraction of the pelvis to trust to turning at the termination of parturition in preference to the induction of premature labour, for the saving of herself and her child. On the whole, he thinks that the cases in which version is applicable are but few in number, and that it cannot supersede the artificial induction of labour.

XXVIII. *Notes of a Case of Spina Bifida, followed by Hydrocephalus.* By D. Richards, M.R.C.S.E.—The child was a female, and was born on the 4th of April, 1860. There was spina bifida in the lower part of the dorsal region, angular curvature of the spine, cleft palate, and talipes varus. In about a month from birth the cavity in the spine became closed up with granulations, but the head was observed to enlarge till October, 1860, when a diminution was perceptible. The child died on the 11th of October from exhaustion, but no post-mortem examination was allowed.

XXIX. *Imperforate Rectum; attempt at relief by Operation; Death.* By W. Tilbury Fox, M.D. Lond.—The case was that of a child four days old, who had passed nothing per anum since birth. The belly was greatly distended, tympanitic, and somewhat tender; there was a small anus, but it merely opened into a cul de sac, and there was no fulness to be seen or felt, nor could the distended rectum be anywhere traced. Dr. Fox passed a canula into the anus in hope of reaching the rectum, but no good resulted, and very little bleeding occurred. He then proposed to open the colon, but some objection was raised, and the infant died. On a post-mortem examination, it was found that the rectum terminated in a cul de sac about an inch and a half from the anus, with which it was connected by cellular tissue. The commencement of the rectum was loaded with fæculent matter, but the termination was almost entirely empty; and hence the absence of impulse during life, and the failure of the operation in liberating the fæces.

XXX. *Ruptured Vagina during Labour; Child in Abdomen three and a half hours; Pelvic Cellulitis; Recovery.* By John Henry Bell, M.D.—The patient had had three children, and her fourth labour seemed at first to be going on pretty well, when the pains entirely ceased; and after waiting for several hours the medical gentleman in attendance gave her two doses of ergot. After this the head was found to have receded considerably, and the hand of the operator was carried forward over the side of the head into the abdominal cavity. Dr. Bell, finding that there was abundance of room, and that the external parts were much relaxed, attempted to deliver the child by the forceps, but failed in doing so, and he therefore determined to turn. The hand was introduced high up into the abdominal cavity, and a foot was grasped, and the child was removed. The placenta readily followed. It was now found that the rupture was in the vagina. She went on pretty well until about the twentieth day, when a large abscess began to form in the left groin, arising, as it appeared, from pelvic cellulitis. She would not allow any examination to be made; but after a time the abscess opened spontaneously, and discharged a large quantity of very offensive matter. Since that time she has generally enjoyed good health; and on being examined lately, it appeared probable that she was again pregnant. The laceration of the vagina had healed, and had left a prominent ridge about an inch and a quarter in length.

XXXI. *A Singular Case of Unsuspected Pregnancy and Awkward Delivery.* By John Shortt, M.D.—The patient was a married lady, aged forty, living in India, and was the mother of two children, and feeling herself ill, she applied to a medical gentleman, who treated her for inflammation and displacement of the womb. The treatment was of a very active description, consisting of drastic purgative pills, com posed of aloes, scammony, gamboge, &c., to relieve costiveness; and caustic, iodine, &c., were applied to the mouth of the womb. While this treatment was going on the abdomen gradually and visibly began to enlarge, and the medical attendant pronounced the case to be ovarian dropsy. Some months afterwards she found that she was passing small quantities of fluid by the vagina, and supposed it to be caused by a rupture of the ovarian cyst. After two days from this occurrence she was seized with violent pains, and the nearest surgeon being called in, she was directed to be placed in a warm bath, when a living child dropped into the bath, and was very nearly drowned. For nearly two-thirds of the period of gestation the speculum, caustic, &c., were applied to the mouth of the womb, and the most drastic purgatives administered without any injurious effects.

XXXII. *On the Use of Medicated Pessaries in the Treatment of Uterine Disease.* By T. H. TANNER, M.D.—The objection hitherto entertained to the use of medicated pessaries is derived from their being composed of drugs mixed with lard and wax, so that they are either too hard or too soft. A substance, however, has lately been introduced into ophthalmic therapeutics by Mr. White Cooper, which appears exceedingly well suited for the basis of these medicated pessaries. This substance is the butter obtained from the *Theobroma Cacao* nut, from which chocolate is made. It has an agreeable smell, does not soil the fingers, does not become rancid, and, though very firm, it becomes fluid at a low temperature. Pessaries made with cacao-butter, though they have the consistence of wax when cold, are dissolved in the course of a few minutes when introduced into the vagina. The medicated pessaries enumerated by Dr. Tanner are made with this substance, mixed with iodide of lead and belladonna, with mercury, with lead and opium, with zinc and belladonna, with iodide of potassium and conium, and with tannin and catechu. There are few uterine diseases in which the use of medicated pessaries may not advantageously form a part of the treatment.

XXXIII. *A Case of Sudden and Unconscious Delivery.* By JOHN SHORTT, M.D.—Dr. Shortt, when in India, once observed three Hindoo women walking before him, when suddenly one of the women, as she was walking along, stood still a second and stooped, when to his surprise he observed a fully-developed male child drop on the ground. One of the woman's companions went and brought a knife with which she cut the umbilical cord. As she was a woman of caste, Dr. Shortt was not permitted to approach her.

XXXIV.—*A Case of Obstructed Labour from the Presence of the Hymen.* By SILAS PALMER, M.D.—In this curious case, Dr. Palmer, on being called to attend the patient in labour, found a band apparently hanging from the right labium, but believing it to be the result of some laceration, he did not attach any importance to its presence. Some hours afterwards, however, the labour had advanced, and the head of the child was prevented from passing by a strong band placed across the vagina, and it was now proved that the hymen was the obstructing body. As there appeared to be no chance of its yielding to the natural efforts, Dr. Palmer divided the band with a pair of scissors, and almost immediately afterwards the child's head passed into the world. The scalp was tumefied, and there was a deep sulcus where the band had pressed.

XXXV. *A Case of Acephalo-Cyclopean Monstrosity.* By ROBERT HARDEY.—The fœtus exceeded the usual size of a child born at the full period, and was born dead. The peculiarities, which were of a very extraordinary kind, are figured in three woodcuts which accompany the paper. There was a face and cheeks, one eye, consisting of a large globe with two pupils, no head, and no nose. The internal organization was in correspondence with the external appearances; there was an entire absence of the upper and lateral cranial bones, the cerebral coverings were found inside, divided by the tentorium into two equal parts, but there was an entire absence of cerebral substance. The peculiarities of the skeleton of this monstrosity consisted chiefly in the absence of the nose, the defective hard palate, the single orbit, and the acephalous formation, and the spinal column was found cleft from the chest downwards. The mother had previously borne several children, all healthy and well-formed, and nothing had happened to her during gestation, which popular prejudice might allege as the cause of the malformation of the infant.

XXXVI. *Case of Puerperal Convulsions complicated with Mania; Apparent Recovery; Sudden Decease.* By ARCHIBALD HALL, M.D. Edin.—This patient was a young woman of twenty-one years of age, of a nervous and irritable disposition, very imaginative, but of a strictly religious turn of mind. The pains of labour were accompanied with violent convulsions and maniacal delusions, chiefly of a religious character. When the uterus was first examined, its orifice was very slightly dilated. Blood was freely drawn from the arm, a blister applied to the nape of the neck, the hair was removed, ice applied to the scalp, calomel was given periodically in small doses, and chloroform was administered. After some time the os uteri became sufficiently dilated to admit of the introduction of the forceps, and a dead child was extracted. After the delivery the convulsions abated, although the maniacal delusions continued for some time, but they also ceased, and she appeared convalescent, when suddenly the insane delusions returned, with symptoms of collapse, and she died. No post-mortem examination was permitted.

XXXVII. *Five Cases of Vaginal Closure.* By J. BRAXTON HICKS, M.D., F.R.S.—In the first of these cases, which was that of a girl aged eighteen, there was complete absence of the vagina, but there was great suffering at the menstrual periods, and the uterus, with the retained menses, could be felt by the rectum. At the time when the menses should have appeared, Dr. Hicks punctured the uterus through the rectum, and three or four ounces of thick, dark-coloured fluid escaped, with immediate relief to the patient. This operation was performed subsequently for the second time, and afterwards the menses were discharged by the rectum at the usual periods. In the second case, there was also entire atresia of the vagina, with retention of the menses. The patient was a married woman, and the urethra was very large, but there were no nymphæ, nor hymen, nor any channel. She had intense pain every month in the lower part of the abdomen, and a hard, elongated, irregularly-shaped tumour was situated above the pubes. In this instance Dr. Hicks resolved to attempt the formation of an artificial vagina in three operations, in order to obviate constitutional irritation as much as possible. Two of these operations he performed, and with success; but the patient refused to submit to the third operation, and the cure was therefore incomplete. In the other three cases, there was severe constriction of the vagina, the result of lingering labours. In one of them the labour had lasted ninety-six hours, and was followed by severe illness resulting in inflammation of the sexual organs and almost complete occlusion of the vagina. A series of operations were performed under chloroform, with the object of dividing the cicatrices and enlarging the outlet, and these proceedings were so successful that she subsequently became pregnant, and eventually the parts were restored to a natural condition. In another case, there was almost complete obliteration of the middle third of the vagina, and the upper third was converted into a large sac, distended with pus, the results of a former protracted labour, but the woman had become again pregnant, and Dr. Hicks saw her only when she was in actual labour. The cicatrices were divided under the use of chloroform, and the labour was completed by the natural efforts. In the last case the middle third of the vagina was contracted to the size of a quill, and the patient also became pregnant under this condition. The head of the fœtus was impacted at the brim of the pelvis. The cicatrices of the vagina were divided and the passages dilated, but as the child was evidently dead, from the putrid discharges which escaped and the absence of the fœtal sounds, the head was perforated and the labour thus brought to a conclusion. Dr. Hicks heard that this woman was delivered of a dead child two years afterwards without trouble.

XXXVIII. *A Case of Multiple Medullary Cancer, complicated with Pregnancy.* By Thomas Hawkes Tanner, M.D.—The patient was five months advanced in pregnancy, and on examination per vaginam, a very firm substance was found projecting into the canal, and seated in the recto-vaginal septum. The uterine souffle and the pulsations of the fœtal heart could be plainly distinguished, but as it was obvious that a live infant could never be born through the natural passages, it was determined to induce premature labour in order to increase the comfort of the woman even if her life were not prolonged. The membranes were accordingly punctured and labour pains set in, but owing to the obstruction offered by the tumour, or to the general cachexia, the case made very little progress, and it was necessary to dilate the os uteri, and to break up and remove the fœtus piecemeal with the forceps. The patient was somewhat relieved by these proceedings, and went on pretty well for a few days, but a series of unfavourable symptoms supervened, and she gradually sank and died. The post-mortem examination revealed the existence of medullary cancer in the abdominal parietes, in the liver, the great omentum, the spleen, and the colon; and in the pelvic cavity there was a separate deposit, extending chiefly down the recto-vaginal septum, and completely blocking up the vagina.

XXXIX. *Case of Retention of the Catamenia for more than two years, in a Married Woman.* By Walter Chapman, F.R.C.S. Eng.—In this case there was a large tumour in the abdomen, resembling the pregnant uterus at about the sixth month; but on making a vaginal examination, the cervix uteri appeared lost in a general enlargement of the uterus, the mouth of which could not be discovered. Dr. Tyler Smith being called in consultation, made a very careful examination, and confirmed the opinion of Mr. Chapman that the case was one of retained menses, an opinion which was strengthened by the circumstance that the patient always suffered great pain and spasm at each monthly period. Mr. Chapman therefore introduced his finger, and scratched through the site of the os uteri with his nail—a proceeding which was followed by a copious flow of treacly-looking fluid, and immediate relief of the symptoms. Some days afterwards Mr. Chapman passed a full-sized elastic male catheter into the uterus, and syringed it out with tepid water, and this operation he repeated daily for a week. But eleven days after the first opening of the uterus she rapidly sank, and died without any obvious cause. No post-mortem examination was permitted.

In the discussion which ensued after the reading of this paper, Mr. Owen and Dr. Graily Hewitt attributed the death of the patient to the admission of air into the uterus in its relaxed and enlarged condition, and they considered that such a step as the injection of water into that organ was inexpedient.

XL. *Case of Monstrosity.* By Alfred Meadows, M.D., M.R.C.P.—The mother of this monstrosity had had twelve children, all of whom were born at the seventh month, but only three were living, and none of them had been the subject of any malformation. In the present case, the labour presented no remarkable features, and the infant was born alive and lived about three-quarters of an hour. The chief peculiarities of this monstrosity (the structure of which is illustrated by three engravings) were, that its body, instead of possessing the usual lower organs of locomotion, terminated in a pointed caudal appendage, measuring about five and a half inches from the extremity of the spine. Near this point there was a kind of ball-and-socket joint, which allowed the tail to move backward and forward, and slightly from side to side. During life this caudal appendage was several times wagged backward and forward, but no other movement was effected by it. On internal examination, it was found that there was no trace of either vagina or uterus, or any external generative organs, nor any anal orifice, and there was a complete absence of all urinary apparatus, both kidneys and bladder. The abdominal aorta bifurcated at its usual situation, and each common iliac again divided into two branches, one of which went through a small aperture in the pelvis to the muscles at the back of the caudal appendage, and the other went to supply those on the front. The cavity of the pelvis was only a shallow depression, and except the alæ ilii was entirely cartilaginous. When the mother was questioned as to the cause of this strange deformity, she expressed no great surprise at the result, and stated that during the whole period of gestation she had been very much attracted by the "talking fish" which at the time was exhibited in London, and she had often walked to look at the picture of this animal. It is remarked, as an interesting physiological fact in this monstrosity, that as there was no urinary apparatus, either the non-elimination of urine is unimportant in intra-uterine life, or the intestine performs the functions of the kidney.

XLI. *The Influence of the Mother's Health in the production of Rickets.* By W. Tilbury Fox, M.D. Lond.—The symptoms and signs of rickets are both local and general: the general being a want of development in the whole system, and the local consisting in an abnormal infiltration of the organs with albuminous material and a non-completion of the process of ossification. Dr. Fox takes as his starting-point the proposition that the immediate pathology of rickets is a deficient supply of earthy material in the early nutrition of the child. The causes usually alleged to account for the occurrence of this disease are such as act directly upon the child, such as bad ventilation, want of cleanliness, insufficiency of food, light, and air, &c.; but Dr. Fox considers that the chief reason for the complaint is to be found in the health of the mother, and that wherever the rachitic child is entirely dependent upon the mother's milk, the mother will be found to have menstruated during lactation regularly for several months, and that, moreover, the degree of rachitis will be in direct ratio to the frequency, duration, and amount of the menstrual flow. It is essential to observe that if the child is partly fed upon other food than the nurse's milk, the disease is lessened in degree or altogether prevented. Dr. Fox gives a tabular statement of 39 cases, in which the connexion between rickets and menstruation during pregnancy is exhibited. On the subject of artificial food, Dr. Fox remarks, that where the substitution is desirable, care should be taken to give something that affords not only starch, but gluten or flesh-forming substance, and inorganic matter or salts. The "foods" usually recommended for children are for the most part valueless as nutritive agents, because they merely contain starch, and no gluten or salts; hence their use will favour the tendency to rickets, and it is probably owing to the milk that is used in their cooking that ill results are usually prevented. Dr. Fox thinks that medical men have it in their power to prevent the occurrence of rickets in the majority of cases, and that in every instance of women menstruating regularly during lactation, nursing should be relinquished, or the child fed by artificial means.

PART SECOND.

Bibliographical Record.

Art. I.—*On the Study of Character, including an Estimate of Phrenology.* By Alexander Bain, A.M., Professor of Logic in the University of Aberdeen.—*London,* 1861. pp. 344.

When we compare the study of mind with the study of matter, psychology with any of the exact sciences, such as astronomy or chemistry, or even physiology, we cannot but be impressed by the remarkable difference which they exhibit in their respective progress and advancement. The latter sciences have attained their present excellence within a comparatively short period; chemistry and physiology almost in the memory of the existing generation, for what was chemistry before the time of Black and Lavoisier; and, irrespective of the great discovery of Harvey of the circulation of the blood, physiology may be said to owe its stable foundations as a science to the labours of almost our contemporaries. Regarding psychology, on the contrary, its condition now is in no very remarkable manner dissimilar from what it was centuries ago; hardly more definite and precise than as we find it in the writings of Aristotle, or more subtle and refined than in those of the schoolmen. Why is this? Is it not owing to the different methods by which they have been severally investigated, and this connected—not a less important circumstance—with the peculiar and dissimilar nature of their subjects? Without the telescope penetrating into space, we can hardly imagine the existence of astronomy as a science; the observations would have been wanting for its foundation. Without the balance and other instrumental aid, chemistry could never have been more than an art. Without the microscope and the aid of chemistry, physiology must have remained in its feeble infancy. Such aids as those referred to, or the like of them, are of no avail in relation to the study of mind. It is hardly a subject for experiment; and from its infinite variations in individuals, a most difficult one for observation. In no way is language so severely tested, and so little accordant; the very same names of mental qualities, as used by different authors, have diversity of meanings. In no inquiry are there so many unsolved problems. When we reflect on the many speculations which have followed each other in psychology, from the earliest to the latest times, we are not surprised that some persons should be inclined to view the study of mind after the manner of our great poet, as expressed in his well-known lines descriptive of the topics of the contemplations of his fallen angels:

> "Others, apart, sat on a hill retir'd
> In thoughts more elevate, and reason'd high
> Of Providence, foreknowledge, will and fate;
> Fix'd fate, free will, foreknowledge absolute;
> And found no end, in wandering mazes lost.
> Of good and evil much they argued, then
> Of happiness and final misery,
> Passion and apathy, glory and shame;
> Vain wisdom all, and false philosophy."

Nor are we surprised that an unfavourable feeling should by many be entertained towards mental research, regarding it practically, and asking the question—What are the benefits which it has hitherto conferred on society, seeing that even now delusions are popular akin to witchcraft, such as table-turning, spirit-rapping, clairvoyance, and have believers amongst the so-called educated? Whether it ever will attain a rank approaching the more exact sciences is open to question. That it will be cultivated, and have engrossing attention paid it, at least from a few, is most certain, owing to the interest in it, and its presumed, and indeed, real importance, in a practical sense when justly applied. Of this we have sufficient proof in the work, the title of which is prefixed, the author of which is already so well known for metaphysical acumen and learning.

The work in question, as its title-page indicates, consists of two parts: one an estimate of phrenology, the other the study of character. The first, that with which it begins, is spread over one hundred and ninety pages, or more than one half the volume. The examination which the author institutes is of a critical kind, not so much regarding the truth of the system as a whole, as the correctness of its details, he seemingly admitting the principle of Gall and Spurzheim, that every faculty or function of the mind has its special activity in special parts of the brain—a very questionable postulate—and that the surface of the brain in consequence admits of division and representation, after the manner of the earth's surface on a map. Mr. Bain gives credit to this system, not only for its insisting on the connexion between the brain and the mind; but also commends it for the more complete discrimination, as he thinks, and classification of the mental qualities which it admits of. These, as many as thirty-five, he criticises *seriatim*. He begins with "amativeness," ending with "causality." In justice to him, we should remark that his approval of the doctrine is of a very qualified kind. He views the system, not as pretended by its strict followers, as a science of mind, but at best merely as *a science of character*, its evidence resting on observation, that of specific manifestations of qualities or powers in connexion with particular protuberant parts of the head, or cranial development.

It is not our intention at present to follow our author in his minute and searching examination of phrenology, according to his estimation of it in relation to diagnosis of character. Believing, as we do, that the whole system is an unsound one, founded on partial observation, like that of physiognomy, and no more trustworthy, we cannot but think that Mr. Bain has engaged in a work of supererogation in commenting on it so largely. We shall confine the few remarks we propose to make to the first of the list of faculties or organs to which they are presumed to belong, that already mentioned of "amativeness." Its seat is placed in the cerebellum, the organ, according to the phrenologists, of sexual love, and of the truth of which they have the greatest confidence. Yet, what is the result of physiological inquiry? Is it not to throw more than doubt that this portion of the encephalon is anywise connected with the generative organs and, *à fortiori*, with amatory propensities.*

Mr. Bain's criticism on this asserted function of the cerebellum ending in an expression of dissatisfaction, especially at the low standard of proof required by the phrenologists, is a very favourable specimen of his method, of his style and acumen. After pointing out the loose manner in which the phrenologists reason and draw their conclusions, compared with the precision and severity of logic required in chemistry and physiology as at present pursued, he says: "There is much that is notable in the coincidences between the shape of head and mental peculiarity; and of the entire number of such included in the phrenological systems, it is possible that some may stand, and others turn out a mistake. As yet there is no certainty either way." We quote this passage to show that our remark of his undertaking being supererogatory was not uncalled for.

This first part of Mr. Bain's book is preparatory to the second part, which he thus introduces: "Having criticized at considerable length the only scheme of human character that has hitherto been elaborated in a manner proportioned to the subject, I mean now to present another scheme, which appears to me more in accordance with the present state of our knowledge of the human constitution. The basis of what I propose is the threefold division of mind into emotion, volition, and intellect."

This division is the same as that which Mr. Bain adopted in his treatise on 'The Emotion and Will,' published in 1859, and which was commented on in the number of this Review for January, 1860. Serious objections were there made to it, which are equally applicable to its reproduction. We refer those interested in the subject to that article; it would be a waste of time and space to repeat them.

Whatever fault may be found in our author's speculative views relative to the qualities of mind, compensation, we think, will be found in the ability displayed in his analysis of character, and in his remarks on its elements. Our limits forbid our giving any quotations in proof; indeed, it would not be easy to select a passage which would be a fair example without trespassing on these limits, for Mr. Bain is not a

* Dr. Brown-Séquard, in his remarks on the conclusions of Rudolph Wagner in his 'Researches on the Physiology of the Brain,' one of which is that the cerebellum exercises an influence on the generative organs, ends with the statement, "que l'observation clinique et les vivisections s'accordent à montrer que le cervelet ne possède d'aucune des functions qu'on lui a attribuées."—Journal de la Physiologie, No. xv., July, 1861.

concise writer; he rather luxuriates in diffuseness, running into extreme refinement; yet he is always scholarly, and often eloquent, and we are sure that this his last work will be read with pleasure by those who have leisure and inclination for intellectual research.

ART. II.—*On the Occurrence of Malignant Pustule in England.* By WILLIAM BUDD, M.D., Physician to the Bristol Infirmary. Pamphlet. pp. 67. (Reprinted from the 'British Medical Journal,' 1863.)

IN a preceding number we brought under the notice of our readers a full and elaborate essay by M. Bourgeois, 'On the Malignant Pustule, as observed in France.' We have now to direct their attention to Dr. Budd's account of the same malady as met with (fortunately rarely) in England. At the same time, Dr. Budd has not overlooked the large amount of matter, relative to the history of malignant pustule, to be found in Continental medical literature, more especially in the treatise by M. Bourgeois, to which we have already referred.

ART. III.—*Sur les Déformations Pratiques du Crane.* Par M. le Dr. JOSEPH BARNARD DAVIS.

On the Plastic Deformities of the Cranium. (Reprinted from the 'Transactions of the Société d'Anthropologie.')—*Paris*, 1862.

THIS paper, originally written in English, was read before the Anthropological Society of Paris, and under their directions translated into the French language for publication in their 'Transactions.' It possesses much interest, pathologically, archæologically, and anthropologically, inasmuch as it describes the characters of certain skulls taken out of ancient sepulchres, and which have been found to have undergone changes of shape, traced by the author to morbid conditions of the osseous structure. These deformities he carefully distinguishes from varieties of form observed in some ancient skulls, considered to present examples of the types of the earliest members of the human race.

ART. IV.—1. *Lectures on the Diseases of the Kidney, generally known as "Bright's Disease," and Dropsy.* By S. J. GOODFELLOW, M.D. Lond.—*London*, 1861. pp. 306.

2. *On Dropsy connected with Disease of the Kidneys (Morbus Brightii), and on some other Diseases of those Organs associated with Albuminous and Purulent Urine.* By W. R. BASHAM, M.D. Second Edition.—*London*, 1862. pp. 347.

3. *The Urine in Health and Disease; and on the Pathology and Treatment of Urinary and Renal Disorders.* By ARTHUR HILL HASSALL, M.D. Second Edition.—*London*, 1863. pp. 416.

THESE three works, although from their title-pages they may seem to treat of like matters, are in truth so different as to their intrinsic characters that the investigator of renal or urinary complaints could hardly spare either. Dr. Basham presents his readers with a large collection of facts on the pathology, more particularly the cellular pathology, of *Morbus Brightii.* The work consists of a series of clinical records of cases of this disease, followed up by careful microscopic study of the urinary deposits observed in the course of each case. The author's object is—

"To ascertain if a more extended signification than has hitherto been accorded might not be given to the tube-casts and cells, and other deposits found in the urine, in renal dropsy; whether, in fact, in the specific character of these cell-deposits might not be found a more certain guide in prognosis than can be furnished by any other property of the urine, or by any other symptom exhibited by the patient. . . . . These deposits undergo marked alteration and change as the renal disease advances, and it has appeared [to the author] an inquiry of deep practical importance, whether a careful record of these modifications of the urinary deposits might not reveal the different stages of these diseases, and become a safe guide in prognosis as well as add another element of certainty to the detection of special forms of renal disorder."

Dr. Basham does not place reliance upon the indications of the progress of renal disease furnished by the amount of albumen or of solid constituents of the urine. An attentive perusal of Dr. Basham's work, together with close examination of its numerous and well-executed drawings of the microscopic characters of the urinary deposits in renal disease, cannot fail to afford a clearer insight into the real nature of this generally fatal disease.

In obedience to the wish expressed by friends in whose opinion he places great confidence, Dr. Goodfellow has republished, in the volume before us, his lectures, which have previously appeared in the 'Medical Times and Gazette.' Dr. Goodfellow informs his readers that he may at some future time publish a more complete work on diseases of the kidney, but whether or not he does so " depends on the reception accorded the present publication." If by this sentence the author intends to convey a confession of a consciousness of the incompleteness of the work now published, we think no persuasion of friends, and no pressure of other professional engagements, should have prevailed with the author to have appeared in print before he had completed his task. We do not mean to imply that Dr. Goodfellow has not justified his confidence in the judgment of his friends, for the Lectures contain a very full, and, indeed, elaborate history of Bright's disease, and one that, clinically, will prove of use; but we think that much that is speculative and theoretical in the chemical and physiological explanations it contains would, in a "more complete" treatise, have necessarily been omitted. Omission or condensation of the views of many writers under these heads, and the substitution of the author's own observation and experience as to the microscopic appearances of the urine and its chemical conditions, concurrently with the progress of the disease, would give the promised " complete work" a more practical character, and consequently a higher value, being the production of an accomplished physician in the enjoyment of a large field for observation. We should, however, do the author great injustice if we did not repeat our opinion that these lectures contain a very full and elaborate history of renal dropsy. It would form a very instructive task for the student to extract and note down Dr. Goodfellow's own facts and inferences, apart from, but side by side with, those of the various writers from whom he quotes. The result would be a brief, but valuable, practical summary of all that ought to be acquired on the matter under consideration.

Dr. Hassall, in his preface, observes, that the speedy exhaustion of the first edition of his work on the urine "may be accepted as evidence that the idea entertained by the author, that a work containing a simple exposition of the composition of the urine, and of the treatment of urinary disorders, would prove acceptable to the profession, was based upon a right foundation." The first edition certainly did little more than convey the *idea*, and its rapid "exhaustion" must, we surmise, have been in a great measure attributable to Dr. Hassall's deservedly large reputation as an analytical chemist. It was, as the author admits, imperfect, so imperfect, indeed, compared with its successor as to have only that resemblance to its predecessor that the acorn has to the forest oak. The present work spreads its reach over the whole extent of the fields of the chemistry, micrology, and pathology of the urine in health and disease. Its arrangement is marked by simplicity, its details are not encumbered by needless iterations, its descriptions brief but comprehensive and clear, its literature copious, but not overloaded. It would be a waste of our readers' time were we to attempt a critical examination of a work which would demand a space far exceeding the limits of a review; suffice it to say, that in our estimation it has no rival in the ready assistance it is capable of daily rendering to the practitioner, both from the fulness and clearness of the text, and the profuseness of accurate delineations of the microscopic appearances presented by urinary deposits.

ART. V.—*The Microscope and its Revelations.* By W. B. CARPENTER, M.D., F.R.S., &c. &c. Third Edition. Illustrated by 10 Plates and nearly 400 Wood Engravings.—*London*, 1862. pp. 792.

WE agree with Dr. Carpenter, that the sale of five thousand copies of this manual quite justifies the belief "that it has not inadequately supplied an existing want." The new edition contains, in some important points, modifications of formerly-expressed opinions, considerable augmentation of references to the sources of information, and a large increase of first-rate illustrations. Positive additions to the amount of nearly seventy pages also have been made; and amongst them we notice the interesting results arrived at by Mr. James Salter, "On the Structure and Growth of the Tooth of Echinus;" the observations of Dr. Hicks, "On the Eyes of Insects," and "On a New

Structure in the Antennæ of Insects;" those of Mr. Whitney, "On the General Circulation in the Tadpole;" and those of Mr. Rainey, "On Molecular Coalescence." We also find extended remarks and recommendations on the mode of injecting and preparing the tissues of the higher animals, which will not fail to be of much value to the histologist, and still further to ensure that reception which we ventured, in a previous number (Oct. 1856), to predict as awaiting the work.

ART. VI.—*Medical Psychology; comprising a brief Exposition of the leading phenomena of the Mental States, and of the Nervous Apparatus through which they are manifested, with a view to the better understanding and elucidation of the Mental Phenomena or Symptoms of Disease.* By ROBERT DUNN, F.R.C.S. Eng., &c. &c.—*London,* 1863. pp. 87.

WE have quoted Mr. Dunn's title-page in full, as it gives an accurate idea to the reader of the scope of the book. Instead of medical psychology the treatise might have been called applied psychology, or psychology in its relations to physiology and pathology. The author is so well known to the profession as an indefatigable worker and sound thinker, especially on psychology, that his name might of itself be taken as a guarantee of the sterling merit of the work.

His first endeavour is to portray the normal phenomena of the mental states and the "three phases of consciousness successively developed—the sensational, the perceptive, and the intellectual—marking three different stages in one mental progress." The phenomena of each form of consciousness are examined and illustrated; and then the author takes up the examination of the nervous apparatus through which the mental phenomena are manifested, confining himself, as the subject required, to the cerebro-spinal system, as the only division of the nervous system concerned with the three several phases of consciousness. This chapter shows how deeply Mr. Dunn has studied the configuration and structure of the brain, and exhibits his thorough acquaintance with the principal authorities on those subjects. He distinguishes those parts of the brain with which each phase of consciousness is especially concerned in its manifestation; and in the section on "the nervous apparatus of the intellectual consciousness," so far sides with the disciples of Gall as to support the opinion of separate regional centres, or distinct segments for different intellectual actions. Recognising the two kinds of cerebral convolutions, the longitudinal and the transverse, he is disposed to refer to the latter "the manifestation of the highest psychical activities;" in other words, "that the transverse series, as an aggregate or whole, is the nervous apparatus of the intellectual consciousness." Moreover, in a subsequent part (p. 75), he adopts the hypothesis, that the anterior cerebral lobes are especially the organ of language.

The third, and more purely medical portion of this treatise, is "on the psychological phenomena or symptoms of disease," including "abnormal subjective phenomena," delirium and coma, sleep, and loss of speech, and closes with a notice of "the phenomena of memory in disease."

No one can rise from the attentive perusal of this little book without feeling that he has derived instruction from it, nor without a wish to further prosecute the line of thought and investigation suggested by its pages.

The word psychology is neither very euphonious nor very inviting in itself; and the science it gives name to is rather a bugbear to many men's minds, who dread what they call metaphysics as a something very perplexing to the wits, clothed in hard words and crabbed sentences, and altogether bewildering. And there is some reason for such impressions owing to the manner in which mental science has been discussed by certain authors; but the reader will find Mr. Dunn's psychology very readable, and the "purely practical man" may easily digest the third portion, as it is so well garnished with practical remarks and illustrative cases from medical observation and experience.

ART. VII.—*Sulla azione dello Zucchero e di alcune sostanze Acide sui Denti.* Richerche sperimentali del Dottor PAOLO MANTEGAZZA, Professore ordinario di Patologia nell' Università di Pavia, &c. &c. *Experimental Researches to discover the action of Sugar and of certain Acids upon the Teeth.* By Dr. PAOLO MANTEGAZZA, Professor of Pathology in the University of Pavia, &c. &c.—*Pavia*, 1862. 8vo.

WHATEVER experiments originate from Dr. Mantegazza recommend themselves no less for the ingenuity and exactness which they exhibit, than for the interest which attaches to their subject-matter. The experiments before us, undertaken as they are with a view to ascertain the action of sugar upon the teeth, afford for their principal conclusion the result that weak, easily fermentable solutions of sugar act upon these structures in a destructive sense more rapidly than its stronger solutions, which being more concentrated, are not equally liable to fermentative change.

Solutions of lactic acid are shown to act upon the teeth in direct proportion to the quantity of acid material employed; its disintegrating power, he affirms, is greater than that of lemon-juice and ordinary vinegar. Whether the proportion of sulphuric acid permitted to be added to the vinegar of this country would vary the result we leave to home investigators to inquire.

We shall confine ourselves to stating the conclusions at which Dr. Mantegazza has arrived :

I. Sugar has no chemical action on the teeth by which they are injured or subjected to decay.

II. Sugar may affect the enamel of the teeth just as any other hard body may do; but for this to occur to any great extent, it would be necessary to be constantly masticating loaf-sugar.

III. Sugar is not detrimental to the teeth, except in a state of lactic or acetous fermentation.

IV. Concentrated and dilute lactic acid, vinegar, and lemon-juice affect injuriously the enamel of the teeth. Possibly, however, the better class of teeth may be able wholly to resist their action.

V. Vegetable acids are so sparingly mingled in our usual dietary that they cannot operate as a notable source of injury to healthy teeth. It is only persons with carious teeth, and those who, when they take acids, have a sense of painful constriction, who ought to refrain from them.

VI. A too acid secretion of the mucus of the mouth is one of the most common and best ascertained causes of disease in the teeth; this is the reason that alkaline dentifrices, and especially vegetable carbon, immersed in saturated solution of bicarbonate of soda, and then dried, have proved so useful in practice.

VII. Possibly the abuse of sugar and sweets may tend to increase acidity of the secretions in the mouth, and act thus indirectly on the teeth; this, however, remains to be proven.

ART. VIII.—*The Australasian Medical and Surgical Review.* Edited by JAMES KEENE. Vol. I. No. 1.—*Melbourne*, 1863.

WE have to welcome this addition to our periodical medical literature from the fast-rising colony of Victoria. The first number has just reached us. It is brought out by the branch of M. Baillière's enterprising firm in Melbourne, and its publishers deserve great credit for the manner in which it is got up, in the matter of printing and paper. The contents occupy two sheets, and it is proposed to publish it monthly, at the cost to subscribers of one guinea per annum, payable in advance. The editorial department is entrusted to Mr. James Keene, late Surgeon of the West of London Hospital, who in his prefatory remarks states his intention to include in the Review original communications, reviews of books, extracts from journals, notes and queries, reports of medico-legal cases, meteorological observations, and mortality tables. Under the head of notes and queries, "correspondence" seems also to be intended.

The collection of papers, extracts, &c., in this first number is very good. The editor contributes some "Clinical Observations on the Use of Nitric Acid in the Treatment of Piles," Mr. Rudall gives the history of "Two Cases of Traumatic Cataract treated by Linear Extraction," and Mr. Beaney, Surgeon to the Melbourne Hospital, recounts "a New Method of Treating Acute Orchitis, as suggested by its Pathological Condition." Going at once to his subject, Mr. Beaney opens his paper by writing: "My plan of treating all stages of acute inflammation of the testicles and their coverings is by evacuating as early as possible the effused fluid contained in the tunica vaginalis by means of a small trocar and canula." From an examination of about twenty cases of acute orchitis after death, whilst serving with the British army in the Mediterranean, Mr. Beaney has come to the conclusion "that it is the serous covering of the testicle that is primarily affected, the vascular condition of the testes being merely a secondary lesion." Both his pathology and practice are confirmed by "the fact, that immediately an outlet is made for the escape of the effused fluid, all pain in the testicle at once subsides, and if examined, the organ will be found very *slightly* enlarged."

A longer contribution than either of the foregoing is from Dr. McCrea, M.B. Lond., Chief Medical Officer of the Prison, "On the Treatment of Delirium Tremens and Mania with Tincture of Digitalis." This paper will be read with much interest, as confirmatory of the recently-revived practice of administering large doses of digitalis for the cure of delirium tremens. Dr. McCrea gives half-ounce doses of the tincture, and has pursued this practice in as many as 80 cases, the history of 37 of which is recorded in this his first paper, whilst that of the remainder is to follow in a subsequent number of the Journal. Of the 37 cases narrated, 3 died; in one of these extensive effusion was found; in another, congestion of the brain and its membranes, extensive arachnitis and effusion of serum at the base of the brain; and in the third, similar conditions, along with large effusion into the ventricles and within the spinal canal. With reference to this last, moreover, it appeared on the inquest that she had been committed over forty times for drunkenness. Among those treated for mania, the digitalis appears to have induced sleep and quiet, but in no other way to have been curative of the disorder. Most of these maniacal cases were very rightly sent to the asylum as soon as their state permitted.

The extracts from journals appear well chosen, and under the head of Medical News we find the examination-papers for the M.B. degree of the Melbourne University, which has just entered on its career as a medical examining body, backed by an organized medical school. Two candidates only appeared, and their fate remained unrevealed at the time the Review was published. The examination-papers are very satisfactory as tests: they include the usual subjects in medicine and the collateral sciences, together with the "Pharmacopœia and Medical Terms," Latin and Greek. The Greek paper was from Xenophon's "Anabasis." From a footnote we learn that "either Greek or Latin is required, but not both."

This first sample of the 'Australasian Medical and Surgical Review' must be considered as most creditable and satisfactory, and as calculated to ensure it a high position and extensive circulation throughout the Australian and other English colonies in the Southern Ocean; for our part, we hope to encounter it as a well-established and long-surviving journal, from which we may often cull valuable observations and teachings for the use of our own readers.

In conclusion, we would suggest to the editor the desirability of inducing some of his contributors, well acquainted with diseases in this country, to study and record the variations in their form and course, and in the treatment required, dependent upon the climate of Australia. We should also be glad to get a trustworthy account of the effects of the climate upon immigrants, and especially upon those labouring under disease, as, for instance, of the lungs, concerning which contradictory opinions are afloat in this country.

ART. IX.—*Cours de Médecine Comparée.* Par P. RAYER, Membre de l'Institut et de l'Académie Impériale de Médecine, Professeur et Doyen de la Faculté de Médecine de Paris, &c.—*Paris,* 1863. pp. 52.

Course of Comparative Medicine. By P. RAYER, Member of the Institute and of the Imperial Academy of Medicine, &c., Paris.

COMPARATIVE anatomy and comparative physiology have at the present time taken their place among professional scientific studies, as essential to the correct interpretation of the structure and functions of man; but comparative medicine has never yet established its claim to systematic investigation—at least, in this country. This will appear strange on reflection, for no question can exist that much knowledge may be gained respecting disease and its results by their careful observation in the lower animals, whose tissues being identical or homologous with those of man, are doubtless affected by cognate diseases. The neglect of this field of research in this country can be traced to several causes, prominent among which is that the diseases of the lower animals and their treatment have been generally entrusted to uneducated, ignorant men, capable only of administering nostrums and of empirical treatment. Much progress has, however, been made of late years, and several able veterinary surgeons have arisen and published valuable treatises on the diseases of most of our important domesticated animals. Still, their labours have been restricted to the practical study of disease in particular animals, and we know of no attempts made to deduce general pathological truths from a survey of the history, course, and consequences of disease in the inferior animals — in other words, to construct a system of comparative medicine.

The Paris Faculty of Medicine has taken the initiative in remedying this defect, by founding a chair of comparative medicine, and appointing to it M. Rayer, whose celebrity as a physician will do honour to the office. The desire is evidently to bring comparative pathology within the circle of studies of medical men, and to turn the lessons derived from it to the benefit of mankind. The aim is, as M. Rayer states (p. 6), to bring the science of medicine in man in relation with that in animals, and to advance and elucidate the former by the latter; to collect a series of facts in comparative and in experimental pathology, which may extend the basis of human pathology. "The new chair will serve as a centre for the exposition and discussion of everything in the pathological study of animals, which may benefit the pathological study of man."

Hitherto M. Rayer has been unable to fulfil the objects of his new post, as he tells us in his brief preface that the multitudinous duties of his office, as Dean of the Faculty of Medicine, have so taken up his time as to prevent him preparing his course of lectures. At the same time, he says, the omission of the lectures has given him more opportunity to collect valuable material relative to his subject, and in order to prove that he has not forgotten the task entrusted to him, he has published an introduction to the contemplated course, which constitutes the *brochure* under notice.

The dimensions and character, therefore, of this production render an analytical review of it unnecessary. It is readily procurable, and will be read with interest and instruction by all.

Having defined the scope of his subject, the author presents a brief history of the labours of others bearing more or less immediately upon it. He shows himself to be well acquainted with the writings of his own countrymen and the Germans upon diseases of the lower animals, and on researches made by them in experimental comparative pathology; but seems unacquainted with English writers upon such subjects; at least he quotes none. Yet we in this country are unsurpassed in our knowledge of the management of the lower animals useful to man, and a vast mass of knowledge has been collected relative to their diseases, which M. Rayer would do well to refer to, though the works conveying it are essentially of a practical character, and scientific comparative pathology be little thought of in them. But besides books on diseases of horses and cattle, we can at the present time refer him to various papers in medical and other journals, especially to those of Professor Gamgee, which would furnish him with valuable pathological facts scientifically recorded and discussed.

Having completed his historical sketch of comparative and of experimental pathology, M. Rayer sets forth his plans for the course of lectures to be given in the summer session of the present year, and proposes first to take up the consideration of the diseases of animals transmissible to man. He will, he tells us, enlarge on the history of cow-pox and its varieties or allied disorders in the lower animals, and then consider the carbuncular diseases injurious to animals and to man. His next subject will be skin diseases, represented by itch, due to parasites, and afterwards eruptive fevers, syphilis, and worms. We hope that when M. Rayer gets well into his course, he will exhibit some respect for the writings of those Englishmen whom we, on this side the Channel, imagine to have very largely contributed to the scientific knowledge of cow-pox and its allied maladies, and in no inconsiderable degree to that of glanders, malignant pustule, and other diseases common to man and the inferior animals. We shall await with much interest the publication of the course, which, from the high character of M. Rayer, cannot fail to promote the progress of general pathology.

ART. X.—*The Cure of Clubfoot without Cutting Tendons; and on certain New Methods of Treating other Deformities.* By RICHARD BARWELL, F.R.C.S., Assistant-Surgeon to the Charing-Cross Hospital, &c.—*London*, 1863. pp. 224.

IT would seem to be the fate of every useful invention or material improvement to experience at first an almost unconquerable obstacle in the indifference and prejudice of routine existence, to be subsequently embraced with sudden ardour, carried on with praises and very indiscreetly applied, and finally to be shaped into proportion by experience and rival systems.

Tenotomy is at present so far identified with orthopædic surgery that they would seem almost convertible terms. The practice of it is in its zenith of reputation, and here comes a book, the moral of which is to bring it down rather closely to the earth; if what the work before us teaches is accepted, few opportunities will henceforth occur to the surgeon of cutting or snicking tendons. With the most favoured position and opportunity, the numbers will soon decrease from five or six successive infants on the operating-stool, to one infrequent pledge brought to be "houghed" by the hand of a surgeon specially gifted and *né à la chose.* We would here rather insinuate than repeat the trifling indications which recur in this brief work of an estimate, not the very highest, formed of modern orthopædists and the present state of their art by its author. It is to be hoped that, like the brothers of Joseph, they will give him a hearing. To the general reader at least, he will appear to have written a very clever book, the fruit of much laborious application. With ideas clearly expressed, he attempts to show by nice facts of anatomy and physiology, how the advocates of tenotomy must be wrong, and also by "inductive reasoning," that the method he brings forward must be right. The thoughtfulness and professional aptitude which appear in every part of his pleasant and carefully-written treatise are sure to raise him into esteem, however we may desire a longer trial of his mechanical contrivances before being quite satisfied of their curative efficiency. Above all, a clear issue is joined, capable of being brought to the test of experience, and we look with some anxiety to results.

By referring all to paralysis, the author narrows very much the field of causation in the production of clubfoot. This forms the starting-point of his conclusions. He draws analogy from *laryngismus stridulus* and *strabismus*, from both of which complaints he sees right to exclude the idea of spasm. Lowness of temperature and feebleness of circulation in the deformed limbs is thought to be favourable to this opinion. Spasm, in his view, is not sufficiently persistent to produce talipes, except in extraordinary cases. The treatment he adopts is founded on his principles, the first of which is as follows: "That, as the loss of balance in muscular action which produces deformity is nearly always paralysis of a certain set of muscles, we are to restore that balance." And to this effect he uses a substituted power (the spring of india-rubber) as a cure for club-feet. It might suggest itself that the cure of the paralysis or innervation would form the first and great indication. This, however, is regarded by our author as generally of a transient and remediable nature; a fatty degeneration occurs in the muscles affected, and as motion is very necessary to overcome this, he objects to any apparatus which shall close the whole foot, and weaken every muscle which has not been previously disabled by tenotomy. Not the least interesting part of our author's arguments is that in which he enhances the value of each particular muscle, showing that the form of the foot is not maintained by ligaments and fasciæ, but to a great extent by muscles and their tendons, which, with one exception, are planted forwards in the foot, in advance at least of the medio-tarsal joint. The tibialis anticus lifts up and maintains the arch of the foot internally, and this muscle affords us the opportunity of illustration—how, in the first place, its section must destroy form and impair function, and again, how our author, when it is paralysed, by a supplementary or complementary band of mimic muscular force representing it, is able to relieve the foot of its flatness, or also to assist in the cure of talipes valgus, equinus, &c.

Similar appliances of indian-rubber springs applied in aid of defective muscle are dexterously arranged to combine in counter-balancing every predominant force which, unopposed, can produce deformity. A fixed point for these springs is found in plates or greaves of tin, firmly and closely adapted to the skin of the leg over the site of the paralysed muscle, by means of strapping with loops and eyelet-hole as required to receive the wire by which the indian-rubber springs are finished off at either end. When thus apparelled *secundum artem*, the patient can walk about with his muscles outside.

Our space allows us no more than to touch generally on a system of treatment which seems well adapted to lengthen out the contracted muscles and favour the manifestation of recovered power in those parts primarily affected.

It would be an unworthy thought which, after a perusal of this work, would subject the author to the detraction of being mechanical, though his superiority in this branch is very apparent; we consider such talent, to use a Shakesperian term, to be the "varnish of a complete" surgeon. Indeed, he follows close in the steps of Hippocrates,* *Neque magnâ vi, sed leniter cogendo.* The history of cases which complete the book, if sufficiently numerous, would strengthen our confidence in his method. The only point which affects us with mistrust is the long-continued application of strapping to the delicate and pulpy surface of the infant, an objection which applies only to that age. Of our friends the orthopædists we will say, that their cures, though imperfect and fallible, have hitherto been gratefully accepted by society. They relieve a deformity which, being apparently allied to monstrosity, detracts greatly from personal happiness by a deviation from comeliness rather than through a conscious sense of deficient power. If the writer of the present work can supply natural form, and

* De Articulis.

at the same time re-establish strength, he will certainly be in advance of the disciples of Stromeyer. His treatment of knock-knee, crooked shins, and other faulty and deficient osteal developments, as they form a separate subject, so they necessitate a separate praise.

ART. XI.—*Dairy Stock: its Selection, Diseases, and Produce: with a description of the Brittany Breed.* By JOHN GAMGEE, Professor in the New Veterinary College, Edinburgh, &c. With Plates and Woodcuts.—*Edinburgh*, 1861. pp. 316.

WE much regret that we have not had an opportunity of noticing this little volume before now. In it the author appears very satisfactorily to have carried out the objects which, in writing it, he proposed to himself; and we cannot but think that the general public may find much information in its pages which, at the present time, may prove abundantly useful. Although that portion of the work which refers to diseases of cattle will, no doubt, engage the attention of the medical man more especially, inasmuch as true pathology comprises the study of diseased action in all forms of animal life, human or otherwise, yet all sections of the community must be interested in subjects bearing upon the health and diseases of such animals as furnish our daily food; subjects which have with us, of late, to a considerable extent, obtained that recognition which they demand. In addition to chapters on the points connected with the selection and diseases of dairy-stock, Mr. Gamgee gives us a chapter on the advantages and modes of using the Roman bath for cattle; also one on the "spaying" of dairy-cows, and one on milk, including rules on the management of the cow in reference to this important secretion. Part II. is devoted to remarks on Breton cattle, necessitated by the late introduction into this country of the diminutive milch cow from the ancient and poetical province of Brittany.

ART. XII.—1. *Further Observations on the Distribution of Nerves to the Elementary Fibres of Striped Muscle.* By LIONEL S. BEALE, M.B., F.R.S. ('Philosophical Transactions,' 1862.)

2. *On the Anatomy of Nerve-fibres and Cells, and the ultimate Distribution of Nerve-fibres.* Abstract of Dr. Beale's Demonstrations. By G. V. CIACCIO, M.D., of Naples. (Reprinted from the 'Microscopical Journal.')

DR. BEALE traces the subdivisions of dark-bordered nerve-fibres on the surface of the sarcolemma until they have become too delicate and transparent to be traced farther, and concludes that these do not cease at the points at which they cease to be visible to us. "Many very fine fibres, which are undoubted nerve-fibres, and which," observes Dr. Beale, "form plexuses and networks, have altogether escaped observation, and their nuclei have hitherto been considered as belonging to the connective tissue." The dark-bordered nerve-fibre becomes narrower as it approaches its ultimate distribution, and is resolved into several fine fibres. These fibres become still more attenuated, cross the surface of a muscular fibre, or pursue a longitudinal course between two muscular fibres. Nuclei are connected with these terminal or pale fibres. Some of the fibres appear to be connected with the nuclei, others pass around them. The pale fibres give off branches which form a network. Nuclei are often seen at the point where the dark-bordered fibre divides. These pale fibres, which have thus been traced by Dr. Beale from the point where the white substance seems to cease, towards the periphery, are regarded by him as "*only the commencement of that portion of the nerve-fibre which influences the muscle.*" The fibres resulting from these subdivisions are in all cases so fine that many cannot be seen with a power magnifying less than a thousand diameters; and there is, adds Dr. Beale, evidence of the existence of fibres which could only be positively demonstrated by a much higher magnifying power. Dr. Beale further believes that these nuclei and networks in connexion with the fine pale fibres form complete nervous circuits, and that there is no such thing as a free termination of a nerve-fibre, nor any such ending as continuity with other tissues.

Dr. Ciaccio, in his communication to the 'Microscopic Journal,' combats the views of Kühne and Kölliker, and espouses the opinions of Dr. Beale. Kühne sees the pale nerve-fibres penetrate the muscular fibre, and terminate in peculiar oval bodies, which he considers to be special organs. Kölliker fails to find these special organs, but sees the fibres terminate in free ends outside of the sarcolemma.

Dr. Beale, it is evident, has followed the fine pale fibres much farther than Kühne or Kölliker. The peculiar bodies described by the former, Dr. Beale regards as the proper nuclei of the muscular fibre itself. The discrepancy of opinion upon what should be matters of fact is thus, apparently, reconciled. Dr. Beale, by his mode of preparation, and by the employment of higher powers, has traced these minute structures almost to their ultimate arrangements; but his observations and inferences require confirmation.

ART. XIII.—*The Causes and Treatment of Imperfect Digestion.* By ARTHUR LEARED, M.D., Physician to the Great Northern Hospital, &c. Third Edition.—*London*, 1863. pp. 222.

IN this edition, we find that additional observations on the important subject of diet appear, and that altogether this useful little book shows proof of careful revision. Our views of the work were fully expressed in reviewing former editions, in our Numbers for October, 1860, and for October, 1861.

PART THIRD.

Original Communications.

Art. I.

On the Geographical and Chronological Distribution of some Epidemic Diseases. By Gavin Milroy, M.D., F.R.C.P., &c.

As a preface to the sketch which I propose to give of the course and dispersion of the Oriental plague, yellow fever, and of malignant cholera in different countries since the beginning of the present century, the present paper will be occupied with some remarks on the subject of the chronology and geography of epidemics in general, and on the importance of greater attention being paid to this, as yet most imperfectly explored, branch of medical inquiry.

At the meeting of the International Statistical Congress held in London in the summer of 1860, I had the honour to read a paper—subsequently printed in the 'Transactions of the Congress'—on the importance of instituting a system of international registration of epidemics, by the regular notation and record, from year to year, of some of the chief diseases of this class in the principal countries of both hemispheres. "Hitherto," I then remarked, "but little has been done in the way of observing and registering the geographical and chronological development and distribution of these distempers over extensive regions of the globe. The researches of almost all inquirers having been confined to their own country, and very generally to only one division or district of their country, it is obvious that unless similar researches are being carried on simultaneously in other countries, contiguous and more remote, some of the most interesting problems of epidemiology—such as the migratory course of certain pestilences, their recurrence at irregular intervals, their subsidence at one time and in one locality, and their appearance at and in another, &c.—can never be hoped to be elucidated."

I had long been of opinion that there is probably more connexion between the development of the same epidemic disease in various countries, and also that there is more relation between the successive occurrence of different epidemic diseases, one following the other, than has been generally imagined; and certainly the extensive investigations resulting from the large inquiry into the subject of quarantine in all parts of the world, recently carried out at the instance of the National Association for the Promotion of Social Science, have tended in no small degree to strengthen this opinion. But whatever may be the truth upon these and such-like points, all, I think, will agree that the establishment of a systematic annual record of the principal epidemics occurring in the different countries of the world could not fail to shed much light on the natural history of these diseases, and lead to results useful alike to science and humanity. What has been done of recent years with so much advantage by the synchronous observation and record in diverse and distant regions of the phenomena of meteorology, magnetism, and other branches of physical science,* might in all probability be undertaken with similar benefit, if conducted on the same plan, in the still more important department of epidemiology. The points specially deserving to be noted in the history of epidemic diseases were stated to be the following:

1. The period of their commencement and their entire duration; the exact dates of the earliest cases in all fresh outbreaks to be particularly recorded, also the month or months of their greatest prevalence and mortality.

2. The total number of deaths—and, as far as possible, of the attacks also—arranged according to age, sex, and (when there are distinct races among the inhabitants) race. In the case of small-pox, the influence of previous vaccination would of course be stated.

3. The general sanitary topography of the towns and districts most affected, and the more detailed sanitary condition of the localities and dwellings where the disease first appeared, and where it chiefly prevailed. Large public institutions — as prisons, hospitals, lunatic asylums, barracks, poor-houses, &c.—which either suffered severely or notably escaped, should always be stated.

4. The precursory state of the public health for one or two months, or longer, prior to the appearance of the epidemic; also the meteorological condition, especially if this has been at all irregular or distempered, and the occurrence of vegetable blights, murrains among cattle, &c., when such phenomena have been observed.

5. It will always be very useful to ascertain the date of the last

* Professor Willis, in his recent presidential address to the British Association of Science at Cambridge, remarked: "The Association, aided by the Royal Society, effected the organization in 1840 of the system of simultaneous magnetical and meteorological observations, established as well by our own Government as by the principal foreign Governments, at different points of the earth's surface, which have proved so eminently successful, and have produced results fully equalling in importance and value as real accessions to our knowledge any anticipations that could have been formed at the commencement of the inquiry."

The system of extended and continuous observation and record has of late years been applied with advantage to various other branches of research, as for ascertaining the periodical phenomena in the lives of plants and animals depending on meteorological and climatic changes—the direction and force of tides and of winds—the temperature of the earth—the recurrence of earthquake shocks, &c. Nor has Government shown itself backward in giving countenance and pecuniary aid in conducting various scientific inquiries recommended by the British Association or the Royal Society on different occasions—e. g., in the preparation of astronomical catalogues, constructing of tide-tables, carrying out magnetic surveys, &c.

antecedent epidemic of the same disease or diseases to be now recorded, and the extent and fatality of that invasion, so that a comparison might be made between the two successive outbreaks.

The proposed scheme excited a lively discussion in the section of sanitary statistics at the Congress before which it was brought forward, and all the speakers, both foreign and British, agreed as to the great utility of the suggestion, if it could be carried out systematically and uniformly in different countries at the same time. By no one was it more warmly approved than by the late Dr. McWilliam, than whom certainly no member of the profession was better qualified to judge from the devoted attention which he had so long paid to epidemiological research. "Among other advantages," he said, "that would result from the adoption of the propositions in Dr. Milroy's paper, would be that of assisting us very materially in determining the origin and mode of propagation of certain epidemic diseases, whose qualities in these respects are not as yet clearly understood. Simultaneous observation is the most likely means of enabling us to seize the first cases of an epidemic; and all in any degree acquainted with etiological inquiries know how greatly the whole history of an epidemic hinges upon these cases, whether the epidemic shall have broken out in one locality only, or simultaneously in various localities far distant from each other. The present period appears peculiarly favourable for the investigation of epidemics, as we have of late years seen diseases of that class invading countries and reaching altitudes that had been from time immemorial exempt from them." Dr. McWilliam here alluded to the remarkable geographical range, in respect both of surface, extent, and of altitude above the level of the sea, as well as to the persistence of the yellow fever in the New World, since it first appeared in the Brazils about twelve years ago, from which time it has not only spread over the entire of the Gulf of Mexico, the West Indian Archipelago, and most of the southern and central provinces of the United States, but has also extended to the Pacific side of the Continent of South America, clinging to certain regions and localities with extraordinary tenacity. "This extension," he remarked, "of so fearful a visitant beyond its usual haunts is, humanly speaking, a great and serious calamity; but it may nevertheless help us to a better understanding of the nature and properties of that scourge of hot climates, by enabling us, through *simultaneous observation in different localities*, to grasp in each the occurrence of the first cases, and thus trace the disease in each locality to its origin."

The soundness of these remarks will be appreciated by every one who has considered the subject; they clearly and forcibly express the object of my proposal, the scope of which was at the very same time being illustrated in a striking manner, in another section of the Congress, by the remarks of Admiral Fitzroy on the kindred subject of meteorological statistics. He said:

"Much has been effected during the last two years by *simultaneous observation at many places*, in addition to the registration of atmospherical occurrences sedulously carried on at sea and on land in many parts of the world. Practically these extensive observations of facts occurring in various climates, and under a variety of conditions, from Arctic or Antarctic regions to those of the tropics, have directly tended to prove the uniformity of those laws by which our atmosphere is governed, and the differences of climates determined. Meteorology, which had been thought a complicated and vague subject, has approached the character of an exact science. It is now by no means difficult to describe the climate of any given place of which the geographical position is known. More than this, however, and more directly valuable is our confirmed knowledge of the laws of storms, and our further acquaintance with the nature and succession of the prevalent or various winds over the earth and ocean. The registers returned from numerous ships, among the finest of merchantmen besides men-of-war, now constitute a mine of valuable maritime and scientific information."

After mentioning various practical results of high value to navigation which have already been obtained from the system of accurate and extended observation now pursued, Admiral Fitzroy showed that all great disturbances or distempers of the atmosphere are preceded by barometric and thermometric indications, which to the watchful and intelligent observer serve as prognostications of what is approaching, and as suggestive warnings of what should be done in the way of precaution and defence. These atmospheric vicissitudes are not, as it used to be imagined, sudden and precipitate in their occurrence; but they take place gradually, and, so to speak, progressively, and it needs but the diligent notation and recording of appreciable signs and phenomena to follow their development, advance, and decline. The barometer affords almost infallible indications; and, by noting at the same time the states of the thermometer, the direction of the storm or of the quarter whence it comes may usually be predicted. The shape and character of the clouds, and the colour of the sky at morning and evening, also serve to assist the observer. Moreover, a knowledge of the state of the weather for some days previously gives much aid in foretelling any great or violent changes. When the indications of bad weather exist a long time beforehand, the gale will probably be of some duration; when they appear suddenly and at short notice, the storm will generally be short also. It is only by due attention to not one, but all the signs derived from the sky, from the past and present state of the weather, and from the indications afforded by scientific instruments, that an accurate foreknowledge of coming atmospheric disturbances is to be looked for.

Now, surely all this cannot but be of significant interest to those who seek to promote the successful investigation of other and not dissimilar departments of physical research. When we are told that within the last few years only meteorology has, from being not much better than a heap of guesswork and mere conjecture, now risen, in the hands of Maury, Fitzroy, and others, to such importance as to be a recognised branch of accurate scientific inquiry worthy of the support of great nations like Great Britain and the United States—contributing as it has done, not only to the improvement of navigation, and thus greatly shortening the length of voyages between distant parts of the world, but also to the saving of much life and valuable property, by providing the mariner with, so to speak, a system of signals by which he may forecaste the weather, and thus be prepared against storms before they reach him—may we not reasonably entertain the hope that, in the other fields of allied inquiry, similar results might be obtained by following a like comprehensive method of scientific investigation? It must, I fear, be confessed that epidemiology is still very much in the position in which meteorology stood not many years ago, consisting mostly of detached facts and statements, which are too often very imperfectly recorded, and apt to be mixed up with mere speculation and conjecture. There have been few attempts at anything like continuous and connected observation over a sufficiently extensive area, and the result is that as yet but little progress has been made in the firm establishment of large general truths. Much of the difference and discrepancy of opinion that still prevail among medical writers on various points connected with the rise and spread of many epidemic diseases is doubtless traceable to the common practice of reasoning from insufficient data gathered from a very limited field and over a very short period of time. And certainly it is not very creditable to the profession to find that in medicine, as in some branches of purely speculative inquiry, there is every now and then a tendency to something of a cyclical revolution of doctrine on questions which may be brought within the domain of accurate observation and strict logical induction. We may be assured, from the analogy of all the other departments of physical inquiry, that there is far less irregularity and variableness in the occurrence and movements of epidemic diseases than is generally imagined, that there are manifold links between them of which as yet we have no idea, and that all the seeming disorder and confusion in their course and career are due much rather to our purblind ignorance than to anything inherently fortuitous or accidental in their distribution. True, there is indeed "a maze, but" it is "not without a plan;" and sound philosophy will, it may be safely presumed, one day point to the same great truth which simple faith receives, that—

> "All Nature is but Art unknown to thee,
> All Chance, Direction which thou canst not see." *

By briefly noticing one or two points in the history of some epidemic diseases, it will be seen, I think, that there are sufficient grounds

* Not only has it been shown that there is an "art"—i. e., design and order in the course of many phenomena of Nature which were once deemed to be "chance" and irregular, but some most unexpected coincidences between the occurrence of certain cosmical appearances, between which no one could have conjectured any probability of agreement, have been discovered by the patient and continuous observation of independent inquirers. A remarkable instance of this is afforded in the case of magnetic storms, which have been shown to observe regular periodic intervals, while certain of these intervals have been found to coincide exactly with the periodic phases of increase and decrease in the spots observable on the disc of the sun.

for the expectation now expressed. And first as to the circumstances which ordinarily attend their manifestation.

An epidemic outbreak, at least of those diseases to which attention will be specially drawn in subsequent papers, is not, as has generally been imagined and often confidently asserted, a sudden or unheralded event. It is usually preceded by various signs or phenomena which the careful observer will seldom, if ever, fail to discover. The meteorological conditions are often irregular and distempered. There is, too, a greater amount of sickliness of different kinds than usual, and the common maladies frequently exhibit anomalous and peculiar characters. Generally, the prevailing sickness is only a milder and less developed form of the approaching pestilence. Thus the cholera has usually been preceded by epidemic diarrhœa of a choleraic type; the yellow fever by irregular and unusually severe forms of endemic malarial fever, often associated with troublesome bowel disorder; and the plague has almost universally been ushered in by typhus, which so gradually lapsed into the more malignant and dreaded disease, that it has been impossible to determine with accuracy when the earliest developed case of the latter took place.

As to the spreading of epidemics, all evidence seems to show that their diffusion is mainly affected through atmospheric agency, although other and more partial agencies may certainly aid in their dissemination. The diffusion by the atmosphere appears to take place in a twofold manner. Pestilences have often been migratory upon a great scale, travelling on from the country where they sprung up to other and distant lands, and this too by successive although irregular marches, very much after the similitude of the progression of insect swarms from one region or continent to another. In former times, the plague, as the "black death," steadily advanced from the confines of China—as epidemics of influenza have been known to do so in more recent times—across Thibet and Persia to Southern Russia, and thence spread itself over almost every country in Europe, extending even to Iceland and the shores of Greenland. In our own days, the pestilence from the delta of the Ganges has been seen to follow nearly the same track, and with like desolation; and within the last few years, as mentioned above, the yellow fever of the New World has exhibited a diffusive energy unknown before, extending its ravages from the thirtieth parallel or so of southern latitude to the fortieth degree of north latitude, and from the seaboard of Brazil to the western coast of South America along the shores of Chili and Panama. These wide migratory movements must be due to an impelling power present in and acting on the atmosphere, but which has hitherto eluded our knowledge. Is it, however, unreasonable to suppose that if accurate registers were kept of the exact dates of the development of the disease in various localities in the different countries visited, together with a reliable record of the simultaneous meteorological and other physical phenomena, some connexion might one day be traced between them, and some approach be made to the discovery of a law of epidemics, as there has been of recent years to the discovery of a law of storms?

All is certainly a mystery at present, from the utter want of trustworthy data respecting the phenomena in question, even upon the most limited scale, far less over a wide geographical area.

When the epidemic poison has reached a large district or region, its mode of atmospheric diffusion appears generally to be by a larger or smaller number of nearly simultaneous or quickly successive scattered spots of infection, or as it were of fermentative action,—these spots being at first irregularly detached and separate from each other, but gradually enlarging and extending by the development of new and more numerous spots, until at length they more or less completely coalesce, and the atmosphere of an entire district, or of a large portion of it, become the seat of morbific activity. The very accurate investigation, instituted by the General Board of Health, and conducted by Dr. Parkes, of all the early cases of the cholera when the pestilence appeared in London in the autumn of 1848, fairly leads, as it seems to me, to such an explanation of its mode of spreading over the metropolis upon that occasion; and the table of the dates of its appearance in different parts of England and Wales during 1848 and 1849, given in the Registrar-General's valuable report on the epidemic, seems to point to the like conclusion. If we possessed many such reliable data and documents as these in respect of this and other diseases of the sort, we should not be so ignorant as we really are about the usual mode of the development of epidemics.

But besides the two modes now indicated of general diffusion through the medium of atmospheric agency, a pestilential disease is endowed with the property of increase and multiplication in the bodies of the sick, and of being, under certain favouring conditions, communicable from the sick to healthy persons around them,—these latter often becoming, under similar circumstances and conditions, the instruments of a wider propagation. This property is usually known by the term "contagion," hitherto a most fruitful theme of controversy and dispute among medical men, owing in a great measure to the defect of exact information as to the particulars of each case or set of cases, and also to the field of observation being in many instances far too partial and insulated. This subject, like many others, cannot fail to have light thrown upon it, when the topographical and geographical course of diseases, in connexion with their chronological appearance in different localities, comes to be more attended to. It was incidentally introduced into the discussion which followed on the reading of my paper at the International Congress, and on that, as on almost all other occasions, the great want of a comprehensive examination of the subject was but too obvious. Individual instances are apt to be regarded as general occurrences, and occasional and conditional phenomena to be exalted into facts of universal application.

Among the many other topics of epidemiological inquiry that still await authentic and accurate illustration, may be enumerated the usual duration of epidemic invasions in a district, a country, or over a still wider region—the ordinary intervals of time between epidemic invasions of the same disease—the synchronism or the sequence of different epidemic diseases, with the view of ascertaining if there be any inter-relation between their occurrence; the connexion, if any such really exists, between epidemics in man and epizoötic and epiphytic distempers in animals and in plants; the disappearance for lengthened periods or the total cessation of some diseases, and the increase and aggravation of other diseases; the occasional up-springing of entirely new or of long absent maladies. These, together with the geographical range and limits of different epidemic diseases, the influence of race, age, and sex, as well as of all external or physical agencies, in connexion with climate, locality, habitation and mode of living, food, &c., all require to be far more scientifically investigated, and on a wider and ampler field than has yet been attempted. Let me briefly allude, *en passant*, to the intervals between epidemic outbreaks of a disease in different countries. These intervals have doubtless varied much in duration at different times and epochs, but from the want of anything like exact information, we are unable to speak with any precision. Sometimes these intervals have been not more than three, four, or five years; more frequently, they seem to have been from ten or twelve to fifteen or twenty years. Occasionally, the intervening periods between successive visitations appear, judging from the very imperfect records of such events, to have been much more lengthened, as from eighty to a hundred years, and even more. Such was believed to have been the case with the visitations of the plague at Malta prior to the last outbreak of the fever in that island in 1813, and also with the outbreak of the yellow fever in Brazil before its reappearance in that country twelve or thirteen years ago. But in reference to these and such-like statements, we should ever keep in mind, that just as the date of the first-published description of a disease is by no means to be regarded as the true date of its first and earliest appearance in a country, so the want of any published record of subsequent visitations is far from being anything like a positive proof of its complete absence. Nevertheless, from the analogy in the history of blights in the vegetable world, and of other occurrences in physical geography, it seems not unlikely that occasionally very lengthened intervals may elapse between the recurrence of some epidemics.

The subsidence and cessation of certain diseases in countries at one time infested with them,—as, for example, of the sweating sickness in England, although it continues to exist to a partial extent in other and adjoining countries,—is a subject manifestly of importance to all persons, and should be one of surpassing interest to the physician, whose duty it is sedulously to examine into all the antecedent and concomitant circumstances with the view of discovering the causative relations of so notable an event; for that the agencies which have produced it are discoverable, we cannot reasonably doubt. In many instances, the cause or causes of the decline or total disappearance of a disease from a district are readily recognizable, as of dysentery, ague, and other allied maladies from wet and marshy localities after the thorough cleaning and drainage of the land, and the dietetic amelioration of the inhabitants; and also in the equally conspicuous case of typhoïd and typhus fevers from the foul and crowded lanes of large cities, prisons, workhouses, and ships after the due sanitary improvement of their condition. And, doubtless, what has been effected for the subjugation of these maladies, is capable of being done in respect of various other endemic diseases, which enfeeble and destroy the health of the people in every region of the world, and occasion nine-tenths of the physical wretchedness, and not a little of the intellectual and moral degradation among the working classes. In other instances, on the contrary, and this remark applies more especially to the class of the exanthematous fevers and some allied maladies, but little, if any, progress—save in the all-important matter of vaccination for the control of small-pox—has yet been made in the discovery of prophylactic or preventive remedies.

Then, again, in regard to another curious and highly-interesting topic of inquiry—viz., as to the possible inter-relation, or, in other words, the connexion in point of sequence, of the epidemic invasions of different diseases, what a large field for investigations is still unexplored! The first visitation of cholera to Europe in 1830–31 was immediately preceded by a memorable epidemic of influenza, and on the second invasion in 1848, a like antecedence again occurred; but then this seeming connexion was not observed in 1853–54; and, moreover, epidemics of influenza have repeatedly taken place without being followed by any form of choleraic distemper. On several occasions, an epidemic of small-pox has followed immediately upon the heels of epidemic cholera, as in the visitations of this pestilence in Jamaica and in several other West India islands, and also in the Mauritius and elsewhere. It has been stated of recent years that at New Orleans and various other countries where yellow fever is apt to prevail, epidemic outbreaks of that disease have been far more frequently preceded by epidemics of scarlatina than of any other exanthematous fever; but whether this antecedence has been only incidental and fortuitous, or whether there be any connexion whatever in the prevalence of the two maladies, it is at present impossible to say. That measles and hooping-cough very often go together, or follow immediately one upon the other, is of common remark; and the same thing may be said in regard of these diseases and of the various forms of cynanche. Prior to the introduction of vaccination in Scotland, epidemics of small-pox are said to have been usually followed closely by measles, which was then very generally more fatal than when it appeared under other circumstances.

Whatever doubt there may be as to the synchronous or sequential connexion of different epidemic disorders, there can be none as to the frequent antecedence of a sickly state of the general health in a district or country before the developed appearance of certain pestilences. Outbreaks of continued fever in this country have, over and over again, been preceded by an unusual prevalence of diarrhœa and other forms of intestinal disturbance, with or without the concurrence of catarrhal ailments. This point is frequently mentioned in the history of epidemic fever in Ireland, and also in the medical Reports of the army in connexion with the sickliness of certain regiments and of particular barracks and cantonments in different places; and, what is highly interesting, the very same remark has frequently been made in respect of the yellow fever of hot climates, whether it occurs among a population on shore or on board of ship—viz., that, before any severe outbreak of this deadly disease, bowel disorders have generally been observed to prevail among the inhabitants or the crew for some time previously. In the carefully-observed epidemics of this fever at Bermuda in 1843 and in 1854, this fact was markedly observed. The strikingly-increased prevalence of diarrhœa in London and in England generally, for several years prior to the epidemic of cholera in 1848–9, was ably pointed out by the late Dr. Southwood Smith; and without mentioning other illustrations of a like nature, I will only allude to the notable change, within the last thirty years or so, in the general constitution or basis of disease to a more decidedly asthenic type than previously existed in many countries on the continent as well as in Great Britain, ever since the first European visitation of the malignant cholera. Whether this change in the prevailing type of disease had been noticed prior to that remarkable epidemiological event, we have not the means of ascertaining. It would be a matter of no small interest if we knew with any degree of precision the state of the public health, more especially over the Eastern portion of Europe, as in Poland and the adjoining provinces of Russia, from 1827 to 1830, during the lull for two or three years in the onward westerly march of the great epidemic from the plains of Asia.

From these and such like considerations it will be obvious, I think, that it is not possible to form anything like accurate opinions on the principal features or attributes of a spreading pestilence from the experience, however large, obtained in one locality or district alone, and that error can scarcely be avoided by him who endeavours to build up a doctrine on data derived from his own limited sphere of observation. The commander of a regiment may narrate more accurately than any other person the operations in a battle where his own men were engaged, but then he is apt to attach undue importance to what came under his own immediate notice. The staff-officers who were moving to and fro, and were thus acquainted with what was going on in almost every part of the field about the same time, will better appreciate and more truly describe the bearings and results of the various movements upon the general issue. And so it is in a great measure with the history, to be correct, of an epidemic invasion. The disease requires to be seen in different localities, districts, and countries, among the different classes of society, and under the numerous differences of local peculiarity. It is indeed most necessary for the advancement of scientific truth that a careful examination be made, and recorded at the time, of the facts connected with the origin and spread of the distemper in the individual spots where it appears; but it is no less necessary, before any generalizing deductions are hazarded, that the observer should know what was taking place about the same time in different places, whether adjacent to or more remote from the spot where he was placed; otherwise he will almost infallibly be misled in forming his conclusions, just as the hydrographer would be who should attempt an account of the tides in a particular harbour, without any reference not only to their rise and fall at other points on the same and neighbouring lines of coast, but also to the general currents of the great ocean streams. There must therefore be a system of accurate geographical, as well as of topographical, record established before epidemiology can attain to the position of a true branch of physical science.

Now the question comes to be, how should this desiderated information be sought for, and what existing machinery is there in this country by which we can most readily and most usefully do our part in the great scheme of an international registration of epidemics? From the statements of Dr. Berg and of Dr. Neumann, the delegates from Sweden and Prussia at the Statistical Congress, it would seem that much more attention is paid in these countries than in our own to the regular registration of epidemic diseases; and the same remark holds true of France also, where a systematic investigation of all severe outbreaks of these disorders has long been practised. But none of these countries possesses anything like the facilities for the work and for the prosecution with advantage of this important part of natural science as Great Britain; and this, too, in respect not only of its own people, but also of foreign lands, and indeed of every part of the world. Through the machinery of the parochial medical officers dispersed through the United Kingdom, it would be most easy to establish a system of accurate notation and record of all domestic epidemics. It was stated by M. Quetelet, the distinguished statistician of Belgium, in presenting to the Congress a proposition from Captain Maury, a proposition for instituting a still more extended observation of meteorological phenomena in different countries than yet exists, that there were more than two hundred observers conducting such investigations throughout England alone, and that the results obtained by a large number of these gentlemen were regularly transmitted to the Registrar-General, and published at the expense of Government. Moreover, there is at the Board of Trade an established meteorological department or office, under the direction of Admiral Fitzroy, and with assistants under him, for the express purpose of utilizing for the public good the information obtainable from systematic and sedulous attention to the ever-shifting phenomena of the atmosphere.* Now, why should there not be some arrangement of the kind for the observation and record of epidemic phenomena in the manner and through the machinery indicated above, and in connexion either with the Poor-Law Board, which has the general supervision of all the parochial medical officers, or, if deemed better, with the medical department of the Privy Council, entrusted as it is with the care of the public health of the

* "In 1854, in consequence of representations originating with the British Association, our Government created a special department in connexion with the Board of Trade, under Admiral Fitzroy, for obtaining hydrographical and meteorological observations at sea, after the manner of those which had been for some years before collected by the American Government, at the instance and under the direction of Lieut. Maury."—Professor Willis, loc. cit.

country? That a vast amount of public good would be effected by the early discovery of zymotic disease in different localities, and by determining with precision the districts of the kingdom where they most prevail, will not be questioned by any one in the present day, when the importance of prophylactic and preventive medicine is so generally acknowledged. It is thus alone that the surest means for the mitigation and diminution of much pauperizing sickness and fatal disease among the working classes of the community can be reached; and on this ground alone, apart from other considerations, the subject well deserves the earnest attention of our governmental authorities.

Then as regards the opportunities possessed by Great Britain in acquiring reliable information respecting the prevalence of epidemic disease in foreign and distant countries, how readily and promptly they might be had through the medium of our consuls located in every land, and of the governors of our numerous colonies dispersed over every region of the globe. It requires but directions to be given by our Foreign and Colonial Secretaries of State to these functionaries to add to their annual reports sent home on the trade, commerce, &c., of the place or country, and on the general condition of their populations, a short statement from a resident medical man as to the public health, and the principal diseases which have prevailed during the preceding twelve months. That such information would be willingly given, and that our consuls and colonial governors would themselves feel an interest in procuring it, was shown by the large amount of most valuable materials procured in this way by the Quarantine Committee of the National Association; and the plan is now being followed in the comprehensive inquiry into the subject of leprosy by the College of Physicians, at the request of the Government. In addition, too, to these varied sources of authentic information, foreign and domestic, the annual health reports of our army and navy, such as no other nation in the world possesses, will be found to afford much highly interesting knowledge respecting different epidemics. From such manifold channels, what an amount of precious raw material for scientific elaboration might readily be had; and with such information in hand, how easy it would be to construct charts and maps illustrative of the diffusion and course of a set of cosmical phenomena which as yet have scarcely been thought of!

ART. II.

Statistical Analysis of Cases of Chorea. By THOMAS B. PEACOCK, M.D., F.R.C.P., Physician to St. Thomas's Hospital, and to the Hospital for Diseases of the Chest, Victoria Park.

THE cases of which the following paper contains the statistical analysis, were treated by myself within the last few years, and, with the exception of three or four seen in private practice, all but one occurred at St. Thomas's Hospital.

Distribution of Cases as to Age and Sex.

Ages.	Total.	Males.	Females.
5 years	1	1	0
5 to 10 „	13	6	7
10 to 20 „	14	4	10
20 to 30 „	1	0	1
30 to 40 „	0	0	0
40 to 50 „	1	0	1
57 „	1	0	1
	31	11	20
Mean age	14·6	10·7	16·8

Predisposing Causes.

Occurred during rheumatic fever, with cardiac complication	1
Supervened a fortnight after recovery from what was regarded as diaphragmatic pleurisy, but which may have been some cardiac affection, though unconnected with any obvious physical signs	1
Supervened six months after attack of rheumatic fever, not obviously attended by any cardiac complication	1
Supervened shortly after an attack of rheumatic fever, and attended by a systolic murmur heard at the base	1
Stated to have been preceded by pains in the shoulder and thigh, but so slight as not to receive medical attention	1
Accompanied by a systolic murmur at the base, but no statement as to previous attack of rheumatic fever	1
Supervened shortly after an attack of erysipelas of the face	1
Followed, after an interval of a year and a half, an illness said to have been inflammation of the kidneys	1
Out of health three months before, but not labouring under any definite illness	1
Connected with retention of the catamenia	1
Connected with irregularity and defect of catamenia	1
In the sixth month of pregnancy, in a person who had had chorea in early life	1
No predisposing cause ascertained	19
	31

Connexion of Cases with Rheumatism and Cardiac Affections.

Never had had any form of rheumatism	9
Stated to have the heart's sounds quite healthy	5
" " " loud and flat	3
No report as to rheumatism and state of heart, but in all of which it may be inferred there was no cardiac disease . .	14
	31

Exciting Causes.

No exciting cause named, but in all of which it may be inferred that inquiries were made, and either resulted in negative information or in a denial of any obvious cause .	8
Expressly stated that no exciting cause could be ascertained	10
Present or first attack ascribed to alarm	9
Referred to sympathy from seeing another person affected with chorea	1
To chagrin at having had money stolen	1
To joy at seeing her father return, who was supposed to have been lost at sea	1
Without obvious exciting cause, but greatly aggravated by a slight fall	1
	31

Proportion of Previous and Subsequent Attacks.

There had been no previous attack in	22
One or more previous attacks in	9
	31

Duration of Attack for which admitted before the commencement of the Treatment.

2 days	1
7 days	3
10 days	1
2 weeks	5
3 weeks	1
1 month	6
6 weeks	1
2 months	1
9 weeks	1
10 weeks	1
3 months	2
4 months	1
	24
Not precisely ascertained	7
	31

Period at which the previous Attacks had occurred in cases where there were two or more.

In early life, the patient being twenty-two years of age . .	1
Five years before, the patient being nineteen years	1
Two years before	1
One year and a half before (the patient having had three attacks during that period)	1
One year before (the patient having had five attacks in that period)	1
One year before (the patient having had only one attack) . .	1
Three months before (but two previous attacks at thirteen and fifteen years of age, the patient being seventeen) . . .	1
Very short time previously (subsequently a third slight attack)	1
Five previous attacks, but period not named	1
	9

Result.

Cured, 25; relieved, 2; died, 2; total, 31.

Residence in Hospital.

Duration of residence in 27 cases (26 cured and 1 relieved).—Mean period 52·7 days, and extremes 15 and 153 days; or, exclusive of the case only relieved, 48·7 days, and extremes, 15 and 134 days.

2 to 3 weeks	4	
3 to 4 "	3	
4 to 5 "	1	
5 to 6 "	4	
6 to 7 "	5	
7 to 8 "	1	
8 to 9 "	3	
9 to 10 "	1	
11 to 12 "	2	
17 to 18 "	1	
19 to 20 "	1	
21 to 22 "	1	relieved.
	27	

In two cases the date of discharge is not reported.

Duration of Residence as to whether first or subsequent attacks.

Duration of treatment in 19 out of 22 cases of first attack, 52·5 days, and extreme periods 15 and 153 days; or deducting the case in which relief only was obtained, 46·9, and extremes, 15 and 134 days.

Duration in 8 out of 9 cases of relapse or subsequent attack, mean 52·8, extremes 21 and 122.

Duration of Residence as to Age.

In 17 cases (including three of relapses or subsequent attacks), from five to fourteen years of age inclusive), mean age 10·1 years, and mean period of treatment 48·1 days, extremes 15 and 134.

In 8 cases (including 3 of relapses or subsequent attacks), from fifteen to twenty-two years of age inclusive, the period of treatment was 49·8 days, mean age 19·2, extremes 20 and 68.

In 2 cases, forty-eight and fifty-seven years of age, the first having shortly before had a previous attack and again relapsed after this; in the other, in which the attack was a first one and was never entirely cured, rather lapsing into paralysis agitans, the duration of the treatment was 57 and 153 days.

Treatment.

The treatment consisted in the employment of aperients with the view of relieving any disorder of the digestive organs which may have existed, and the repetition of such as often as required throughout the continuance of the affection; the exhibition of nervine tonics, anodynes, and occasionally counter-irritants, to remove the spasmodic action; and at a later period, the use of the tepid or cold shower- or sponge-bath with a nutritious diet and frequently wine or malt liquor.

The nervine tonics employed were sulphate of zinc, given in doses at first of one or two grains, in infusion of valerian, and gradually increased by one or two grains at a time, generally twice a week, and repeated three times daily; the remedy being suspended if sickness was produced; or when the symptoms subsided. The doses required for the cure in cases where zinc only was employed were four, six, eight, nine, eighteen, and twenty grains.

Iron was chiefly given in the form of the saccharine carbonate, in doses of five, ten, fifteen, twenty, and thirty grains, three times daily; occasionally the sesquioxide was employed, and rarely the sulphate of quinine and iron with an excess of sulphuric acid; or the citrate of quinine and iron.

Arsenic was given, in the form of the liquor potassæ arsenitis, in doses gradually increased from three or four to eight minims, three times daily.

The anodynes employed were morphia or hyoscyamus exhibited at bedtime or more frequently during the day.

Blisters were occasionally applied either at the back of the neck or down the spine.

Tepid or cold sponging and the shower-bath were also frequently had recourse to.

The results of the treatment by these different remedies were as follows:

The affection was cured by the employment of zinc alone in 7 cases, including three cases of relapse or subsequent attacks.

The mean age of the patients so treated was 12·1 years, and the extreme ages 8 and 22.

The period of time occupied in the cure averaged 35·5 days, and ranged from 21 to 63 days.

The cure was effected by iron alone in 6 cases, 1 being a case of relapse or subsequent attack.

The mean age of the patients was 10·4 years, and the extreme ages 10 and 13.

The period of treatment (the date of discharge in one case not being reported) was 28·6 days, and the extreme periods 20 and 46 days.

Zinc failed, but iron effected the cure in 6 cases, the mean age of the patients being 13·3, and the extremes of age 5 and 19.

The average period of treatment was 53·6 days, and the extreme periods 38 and 84 days.

Iron failed and zinc proved successful in 2 cases, the ages of the patients being 13 and 19, and the periods of treatment 122 and 68 days; both were cases of relapse or subsequent seizure.

Zinc and iron both failed, and the cure was effected by arsenic, in 1 case; the age of the patient was 9, and the duration of treatment was 78 days.

Zinc and arsenic failed in 1 case; but the patient was cured by iron and morphia, the latter exhibited first every night, in doses from one-sixth to one-third of a grain, and subsequently every night and morning. The patient was 14 years of age, and the duration of the treatment extended to 134 days.

Iron and arsenic were both used with little benefit in 1 case; but the patient recovered considerably under the use of zinc. The symptoms, however, never entirely subsided, but lapsed into an affection of the face and head, somewhat resembling paralysis agitans. The age of the patient was 57, and the duration of treatment 153 days.

Arsenic aggravated the symptoms, and zinc effected a cure in 1 case, the patient being 10 years old, and the treatment extended to 37 days.

Quinine and iron were successfully used in 2 slight cases of relapse, ages 48 and 12, and periods of treatment 57 and 41 days.

Of the two fatal cases, one terminated on the 6th day from admission, the 26th from commencement of treatment, and about the 34th from seizure; the other on the 15th from admission and 30th from seizure. Both the patients were 11 years of age; one was a boy, the other a girl. One patient took zinc, iron, arsenic, and morphia; had blisters, &c., applied: the treatment of the other case consisted in the employment of the sesquioxide of iron and sulphate of zinc, opiates, and the application of blisters.

CONCLUSIONS.

The general conclusions deducible from this small number of cases correspond with those which have been arrived at by observations on much larger series:

1st. The disease is shown to be much more common in females than in males, the proportion being 20 females to 11 males.

2nd. It occurs at all ages, but much the most frequently in early life or youth and girlhood, or from 5 to 20 years of age. The cases at this period of life constituted 27 out of the 31 analysed.

3rd. The affection appears to be about equally frequent in young boys and in young girls, the number of cases at from 5 to 10 years of age being in boys 6, in girls 7.

4th. At a later period, or in youth and girlhood, or from 10 to 20 years of age, there is a great difference in the frequency of the disease in the two sexes, the cases in males being only 4, and in females 10. After this period also the female sex seems to be very much more exposed to the disease than males; these facts pointing to catamenial irregularities, or other uterine disorders, as being influential in predisposing to the disease.

5th. By far the most frequent co-existence with chorea is, however, some form of rheumatic or cardiac affection. In 3 cases, the chorea supervened during or after attacks of rheumatic fever, and in a fourth the affection commenced with slight rheumatic symptoms. In one of the former cases there was obvious cardiac complication, and in a fifth, there were signs of cardiac disease, but without any history of rheumatism. In 9 other cases it is stated that the patients and their friends had no knowledge of their ever having had rheumatism. It would thus appear that, in 14 cases, rheumatic or cardiac symptoms had existed in 5. It is, however, probable that this proportion is too large, as almost certainly full inquiry was made in reference to all the other cases, but failed to elicit any information. The co-existence or near connexion of rheumatic and choreic attacks has, however, been so frequently observed as to have completely established the existence of some relation between the two affections, probably as coincident effects of a common cause.

In two other cases, not included in the report, in which the symptoms were mainly cardiac, there had been, or existed, symptoms of chorea. In one of these, a girl 9 years of age, the patient died of regurgitation through the mitral orifice from dilatation of the aperture without valvular disease, and the heart was very large—ten ounces and a half—double the natural size; the pericardium was adherent in places, and the endocardium thickened. The patient had had chorea three years before her death, and suffered first from cardiac symptoms after an attack of influenza about two years after. In the other case, a boy of 14, there were chorea, spasms, and paralysis of the left hand and arm, with rheumatic pains in different parts of the body, and cardiac symptoms, which had supervened three months before. The symptoms were apparently brought on by mental excitement. After death, the heart was found generally enlarged; there was some effusion in the pericardium, and vegetations on the aortic and mitral valves. The patient had never been quite well since a severe attack of scarlet fever seven years before.

6th. The most frequent exciting causes of chorea are more or less violent mental impressions—fear, grief, excessive joy, &c. Of the 31 cases analysed, 11 originated in some impression on the mind, and in the 12th, though the patient had had a slight fall, fear was probably the cause of the attack. It thus appears that mental causes operated in exciting the disease in 38·7 per cent. of the cases, and it is probable that the common origin of the disease in mental impressions explains in part its greater frequency in females. For such causes, however, to give rise to the symptoms, it is doubtless necessary that the individual should be unduly susceptible, for in several cases the assigned impressions were of too trifling a nature to have operated injuriously upon a healthy person. In 18 cases also, or 58·06 per cent., it may be inferred that no exciting cause could be detected.

7th. The indications for treatment are to relieve gastro-intestinal irritation, quiet the nervous system, and improve the general power and the condition of the blood. The remedies which were employed for these purposes have already been mentioned. In the selection of the kind of nervine tonic to be exhibited in any given case, I have generally been guided by the existence or absence of evidences of chlorosis. When the patient is very anæmic, as indicated by the colour of the skin and mucous membranes, the presence of continued venous murmur in the neck, &c., I have usually, after the employment of aperients, had recourse to chalybeate medicines; when, on the contrary, the patient was free from such symptoms, the zinc has generally been first employed:—the other remedies—arsenic, opium, &c.—only being had recourse to when the iron or zinc had failed. Under all forms of nervine tonic, the patients occasionally fail to derive benefit, but, as a general remark, zinc seems to prove more rapidly useful and to afford more permanent relief than iron. The remedies act with greater efficiency, according to the rapidity with which the doses are increased and the full extent of the increase. Thus I have generally increased the doses twice or three times in the week, adding in the case of the zinc at first one grain, and subsequently two or more each time. Though in the cases of chorea no patient took more than twenty grains of the sulphate of zinc for a dose, I have in other affections given thirty grains three times daily, and without producing sickness or any evidence of gastro-intestinal irritation.

When any given remedy has been fairly tried without marked amendment, or when the improvement at first noticed has not been continuous, it is better to substitute some other medicine, and the improvement which follows the employment of the new remedy is often very marked; and I have sometimes noticed this when a second or a third change has been made.

A good indication of the advantages of treatment may be derived from the condition of the pupils; generally in the active stage of chorea they are very large and nearly insensible to light, and as the patients recover, the pupils diminish in size and resume their proper motion. The shower-bath is often very beneficial, but in children it is apt to create alarm and to aggravate the symptoms. When this has been the case, I have preferred to employ ablution, and have used cold or tepid water, according to the state of the weather and the feelings of the patient.

8th. It does not appear that there is much difference in the facility of cure in cases of first attack, or of relapses or subsequent seizures. The age, also, does not influence the result, provided the patients be not in advanced life. In the only two cases at middle age and advanced age the attacks were, however, very prolonged, and in the elderly person the cure was only partial.

9th. Under any course of treatment chorea is very apt to relapse; and even after recovery is complete, attacks for a second or third time or even oftener, frequently occur. It will be seen that in 9 out of the 31 cases, or in 29·03 per cent., the patients had had previous attacks. I am disposed to believe, without, however, being able to bring conclusive proof on the point, that relapses occur less frequently in the cases which are treated by zinc than in those in which iron has been used; I have therefore not unfrequently given zinc to patients after the active symptoms have been relieved by the use of iron, and apparently with benefit in preventing relapse.

Fatal Case of Chorea.—John Adkins, aged 11, admitted into the Royal Free Hospital, August 2, 1849.—The symptoms were first observed about a month before; he was observed to twitch the hands and arms, and the spasms increased, so that he was brought to me as out-patient on the 12th of July. He was directed to have a calomel and rhubarb purge, and to take ten grains of the sesquioxide of iron three times daily. When admitted into the hospital he had constant spasms of the muscles of the face, arms, and legs; he had difficulty in protruding his tongue, and could not retain it for even a very short time; he could not walk, and tossed himself about on the bed in every direction, and he had a peculiar vacant stare. He was directed to have a purgative, and take ten grains of sulphate of zinc three times daily. Anodynes were prescribed, but did not procure rest or sleep; and he was blistered at the back of the neck and down the spine. The convulsions became more constant and general, his intelligence declined, and he died comatose on the 8th—the sixth day from admission into the hospital, the twenty-sixth from the commencement of the treatment, and about five weeks from the first occurrence of the symptoms. On examination after death, the arachnoïd membrane on the surface of the hemispheres was found opaque, and a copious effusion of serum existed beneath it. Much fluid was also effused in the ventricles and existed at the base. The spinal cord was not examined. The cerebrum weighed 50 ozs., the cerebellum 5 ozs. and 4 drs., the pons variolii and medulla oblongata 1 oz., making the whole encephalon 56 ozs. and 4 drs. avoirdupois. The pericardium and heart were healthy, except that two of the folds of the aortic valves were somewhat united at the angle of attachment, and a loose band existed at the margin of one of them. This condition, however, was evidently a congenital defect. The heart weighed four ounces.

Art. III.

Contributions to Therapeutical Statistics. By Thomas K. Chambers, M.D., Hon. Phys. to H.R.H. the Prince of Wales, to St. Mary's and the Lock Hospitals, &c.

CONTINUED FEVER.

Since the opening of St. Mary's, in June, 1851, to August 12th, 1863, there have been registered, as under my care, 230 cases of continued fever. Of these cases, 109 have been treated on what may be termed "general principles;" that is to say, they took neutral salines three or four times a-day, with small doses once or twice a-day of hydrargyrum cum cretâ at first, and later in the disease, bark, ammonia, ether, and wine, when these remedies seem required by the symptoms. Leeches and cupping were employed to the exterior of inflamed viscera as occasion called, and food was administered at the ordinary four daily meal-times. The other 121 have been treated on an uniform plan of continuous nutrition; animal food, in a liquid form, has been given every two hours, day and night, while the patients were awake, and between every dose of nutriment a dose of hydrochloric acid. They have been sponged two or three times daily with tepid water, when the skin was hot and dry; and, in a few cases, leeches or cupping have been used to the exterior of inflamed localities in the abdomen or chest.

These two classes of cases offer a fair basis for a statistical comparison of the therapeutical agencies brought into play:

1. In the first place, each series is very nearly continuous: all the first-named 109, with 5 purely accidental exceptions,* occurred in the six years before September, 1857, and all the latter 121 in the six years since. No fallacy can, therefore, arise from a selection of cases for special treatment having been made intentionally or unintentionally.

2. They are spread over a considerable period of years; thus both sets include sporadic cases of all sorts, as well as the produce of epidemics.

3. They were all treated by the same physician in the same wards of a general hospital (where the cases are usually more severe than in special fever hospitals), and they nearly all come from the same group of districts of which St. Mary's Hospital is the centre.

4. The diagnoses have been made and the cases recorded by registrars who have nothing to do with the treatment, and are independent of the physician in attendance.

The only opening for error that I can discern is the bare possibility of a change of type in the disease having taken place at the very time I changed the treatment, and having lasted for six years—a possibility which the records of other metropolitan hospitals during the same period reduces to nothing.

That the severity of the cases in the two classes differed but little may be shown by the near equality of the periods of convalescence. The mean time of stay in the hospital of the patients who recovered was, in the first series, 29·2 days; in the second, 26·7 days, being a difference of but $2\frac{1}{2}$ days. The ages, also, of the two series differed but little, the mean age of each being between 22 and 23 years.

These means are cited merely to show the general similarity of the two series of cases, and not to exhibit any pathological fact.

* Three of these cases were treated on general principles by a colleague taking my duty during my absence, and unaware of the experiment I was trying; in one case I made a wrong diagnosis; of the fifth I have no record, the patient having died within two days, and the clerk's notes being imperfect.

Of the first series of cases (viz., those treated on general principles),

9 are entered as	Typhus, and of these there died		4
44 „	Typhoïd „ „		16
56 „	Of doubtful or unrecorded type „		3
Total 109		Total	23

Of the second series:

25 are entered as	Typhus, and of these there died	0
52 „	Typhoïd „ „	2
44 „	Of doubtful or unrecorded type „	2
Total 121		4

For purposes of comparison in a therapeutical inquiry, it will probably be considered right to exclude from the first table two deaths, and from the second table one death, which occurred within two days of admission; for the exhaustion caused by the journey to the hospital in severe cases allows little scope for judging of the action of treatment during that period. This leaves the average mortality under general treatment 21 in $107 = 19\frac{1}{2}$ per cent., or nearly 1 in 5;* under the second method of treatment, by continuous nutriment and hydrochloric acid, 3 in $121 = 2\frac{1}{2}$ per cent., or only 1 in 40.

I cannot, therefore, avoid the conclusion that the means employed in the cases on the second list are very efficient in preserving life; and that out of every 100 persons attacked by continued fever, from 16 to 17 more may be saved thus than by treating them on general principles.

The continuous liquid nutriment given every two hours consisted of strong beef-tea and milk, of which together about six pints were administered in the twenty-four hours. The hydrochloric acid was given every two hours in doses of twenty minims of the Pharmacopœal dilute acid in water or eau sucrée.† Both food and drugs were seen by the nurses to be swallowed, and not left to the discretion of patients, who, from nausea and occasional delirium, cannot be trusted to help themselves.

The most immediate result of the hydrochloric acid is the more natural condition of the digestive mucous membrane, as shown at its two extremities, by the cleaning of the tongue at the one end and the cessation of diarrhœa at the other. The more natural condition of the mucous membrane enables the greatest possible quantity of nutriment to be absorbed to take the place of the tissues poisoned and interstitially destroyed by the virus which is the cause of the fever. That the acid in any chemical way neutralizes, counteracts, or evacuates

* This mortality is higher than is usual at special fever hospitals, being about the same as at the other general hospitals in London.

† The more elegant fever-drink of the late Dr. Maton consisted of muriatic acid dissolved in barley-water and syrup of mulberries. I find most patients like plain water best as a vehicle.

the virus is, I think, unlikely. For the mean period of convalescence in the above cases was shortened only by 2½ days; and certainly it would be shortened more than that if the virus were removed or rendered inert.

Whether hydrochloric acid would be equally useful in all climates I cannot say; but in Shanghai, a climate as different as possible from England, Dr. Henderson states that its employment diminished the mortality of continued fever from 28 per cent. to 7 per cent.*

The action of wine and of emetics in continued fever is not attempted to be tested in this inquiry. In both classes of cases they were given to a good many; the wine, as usual, in accordance with the age of the patient and the condition of the nervous system; the emetics, whenever the history we could elicit made us suppose the fever to be in its first week. My impression is, that a vomit, when administered within the first five days, materially lessened the severity of the fever; and in some few instances it seemed to cut it short; convalescence, with its accompanying weakness and emaciation, being entered upon immediately, without the prodrome of fever. But I do not know how to obtain numerical evidence of the fact. I have never found an emetic do harm unless there were antimony in it, when it sometimes caused or increased diarrhœa, and failed of its intended purpose. For this reason I prefer plain ipecacuanha.

RHEUMATIC FEVER.

Between June, 1851, and August 12th, 1863, I have had under my care at St. Mary's 247 cases of rheumatic fever. Of these cases—

26 were treated with ʒj. of nitrate of potash three times a day.

174 were treated with bicarbonate of potash—viz.,

- 141 with ℈j., or more, every two hours.
- 33 with a less quantity.

32 were treated, during the first year, in various other ways.

15 (that is to say, all since May of the current year) have had no special drugs; only a little opium when the pain was very bad, and a purgative when the bowels were costive.

No selection of cases was made.

1. *Results on the Duration of Illness.*

Of those treated with nitrate of potash the mean stay in hospital was 40·0 days.

Of those treated with ℈j. bihoral doses of bicarbonate of potash, the mean stay in hospital was 34·3 days.

Of those treated with less quantity of the same, the mean stay in hospital was 40·0 days.

Of those treated without special drugs (the mean of 11 only taken, 4 being still under treatment), the mean stay in hospital was 31·0 days.

If we exclude the last class, the number of which is too small for

* Medical Times, March 21, 1863.

statistical purposes, it would seem that, though smaller doses exert no effect, yet that full doses of the bicarbonate of potash have some influence in shortening the duration of the illness from the time of commencing the treatment to that of the patients being sufficiently convalescent to return to their usual occupations with safety.

I may remark here, that any other measure of the duration of the disease is quite untrustworthy for statistical purposes. The different degrees of susceptibility to pain exhibited by different patients, the desire of some to extenuate, of others to exaggerate their sufferings, makes it impossible to register truly even the exact day when the pain ceases. Whereas, in such a short period as it lasts after the commencement of treatment (namely, two or three days usually), the exact hour would require to be noted. It is equally impossible to measure when, or even whether, the swelling or redness is all gone. Those who have set clinical clerks to observe these facts know how little the case-books are to be relied on.

2. *Results on the Consequences of the Illness.*

In respect of their several preservative powers against the consequences of rheumatic fever—

Of the 26 *treated with nitrate of potash,* there were attacked with acute inflammation of the heart whilst under treatment (carefully excluding all those admitted with it already existing as a result of the current attack) 5, or 19·2 per cent. (4 cases of pericarditis, 1 endocarditis only); 4 have died—2 of inflammation of the heart, and 2 of sloughing back.

Of the 174 *treated with bicarbonate of potash,* there were attacked with inflammation of the heart 9, or only 5·3 per cent.; none have died.

It would seem from this, at first sight, as if bicarbonate of potash had some preservative force. But the fact is, that nearly all of those treated by the alkaline method have been subjected also to what both rational physiology and the statistics following seem to show has a much more powerful influence than any other drug in keeping the heart free from inflammation. I refer to blanketing the patients.

3. *Effects of Blanketing.*

Up to May, 1855, no difference was made in the bedding of my patients with rheumatic fever from that of others in the ward; but after that date they were ordered to be rolled up in blankets, and no linen was let touch the skin. In nearly every case the orders were strictly followed.

Of 63, *either bedded in sheets, or who had wilfully thrown off their blankets,* 6 contracted newly pericarditis at least, if not endocarditis as well; 3 had relapses of pericarditis on old cardiac lesions; 1 had endocarditis alone; on the whole 10, or nearly 16 per cent., had inflammation of the heart, and 4 died.

Of 184 *in blankets,* none have contracted newly pericarditis; none have died; 1 had a relapse of pericarditis on old cardiac lesions; 5

had endocarditis alone; 1 a relapse of endocarditis on old cardiac lesion.

One of these included cases of pericarditis was brought on during convalescence by the patient being dowsed with cold water for an accidental hysteric fit.

Not 4 per cent. have had any acute affection of the heart; when it came it was of a milder character, and was generally to be accounted for by some imprudent exposure.

That is to say, that *bedding in blankets reduces from* 16 *to* 4, *or by a good three-quarters, the risk of inflammation of the heart run by patients in rheumatic fever, diminishes the intensity of the inflammation when it does occur, and diminishes still further the danger of death by that or any other lesion; and at the same time it does not protract the convalescence.*

ART. IV.

Examination of the Spinal Cord in a Case of "Wasting Palsy," with Remarks. By J. LOCKHART CLARKE, F.R.S., &c.

THE patient was a man under the care of Dr. Brown-Séquard, at the Hospital for Paralysis and Epilepsy. Unfortunately, and from accidental circumstances, the history of the case is exceedingly scanty. The following are the notes supplied to Dr. Brown-Séquard by Dr. Hughlings Jackson:

"A man, forty-eight years of age, came to the hospital in November, 1862. He was only seen once, and was told to come to the hospital on a certain day, when his case would be carefully investigated. He did not attend, as he became worse, and died about two or three days after Dr. Brown-Séquard saw him. The patient walked to the hospital from the neighbourhood of Finsbury-square, and took off his shirt in order that he might be examined carefully, but unfortunately no notes were made of this examination. He was a thin, spare man, and had been for some time half-starved, but this was in consequence of his illness, as he could not work at his trade (tailoring) on account of loss of power in his hands. He found a difficulty in shifting the needle, and could not grasp it tightly.

"When Dr. Brown-Séquard saw him, there was wasting of the muscles of the arm and of the chest. He could not expand his chest freely. Although, as stated, he could partly undress himself, yet he could not pick up a pen. His right foot was afflicted in some way, although, as stated, he could walk, and did walk in my presence, apparently quite well. He said his right foot would go down with a 'clap;' in walking, 'there was no spring in the instep.'

"He said that twelve months ago he received an injury to his side which caused pain for a few days, but he went on with his work. Three weeks later he had stiffness in the neck, and then a difficulty in 'putting back his head, so that in getting up he felt as if he would lose his balance.' Then the fingers began to fail, so that at length he could not use the needle properly."

So much for the history of the patient. The cord was forwarded to me by Dr. Hughlings Jackson at the request of Dr. Brown-Séquard. In consequence of some difficulty in obtaining a post-mortem examination, it was not removed until, I believe, six days after death; so that, as may be supposed, it was in a very unfit state for examination, and I almost despaired of making anything of it. In this condition there was no possibility of determining how far the softening was due to disease or to post-mortem changes. But in difficulties of this description we may often obtain a correct estimation of the truth by a careful process of hardening in chromic acid; for after this process is completed, if any one portion of the columns remain softer than any other portion at a corresponding depth from the surface, we may infer, with great probability, that its softening was the result of disease. In the upper half of the dorsal region, the cord, for the most part, was quite broken up, apparently from mechanical injury; but it must evidently have been previously softened to the consistence of thick cream, and presented to the naked eye all the appearances observable in cases of *red-softening;* and yet, on examining small portions under sufficiently high magnifying powers, there was no satisfactory evidence of any granular exudation-corpuscles, and nothing more than altered and broken-up nerve-fibres, with numerous bloodvessels, could be seen.

The methods employed for examining the cord in section were those which I adopted and recommended in my previous cases.*

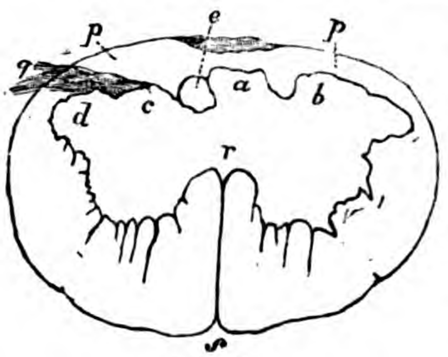


Fig. 1.

Through a space of about three-fourths of an inch below the middle of the lumbar enlargement, transverse sections presented very abnormal appearances. Such a section, through both the grey and white substances, is represented in fig. 1. Here we find that the antero-posterior diameter of the cord is considerably shorter than in health; and that this diminution in depth arises from atrophy or reduction of the posterior white columns (p, p), and from what might appear at first sight to be almost total obliteration of the posterior cornu. In each lateral half there is little more than a single large mass of grey substance, which looks like the anterior cornu; but then we find that it is rather larger than the anterior cornu ought to be; that the posterior roots (q) enter its posterior surface through the diminished white columns ($p\,p$); and that the central parts of the grey substance (r) between the bottom of the anterior median fissure (s), and what ought to be the posterior median fissure, or, in other words, the anterior and posterior commissures, had not their usual appearance, and were much deeper than in health. These facts led me to infer the probability that the substance of the posterior

* See particularly Brit. and For. Med.-Chir. Rev., July, 1862.

cornua was not actually destroyed, but that by some means or other, in conjunction with atrophy of that portion of the white columns which lies between them, the posterior cornua had sunk, as it were, forward, and become fused into one mass with the *anterior* grey substance.

The probable correctness of this explanation was rendered almost certain by sections made higher up near the *middle* of the lumbar enlargement, where the grey and white substances were beginning to resume their normal appearance, as represented in fig. 2. On comparing this figure with fig. 1, we see that in the latter, the posterior cornu (*a*) on the right side, appears to have sunk directly downwards (or forwards) into the anterior grey substance, until nothing but its dilated extremity remains prominent above the common mass; while on the left side, the posterior cornu (*c*) has been almost wholly fused into one mass with the anterior, so as to form scarcely any prominence separate from *d*. At the same time, the posterior white columns (*e*) between the cornu have been reduced to at least one-sixth of their normal size; and this reduction, together with the forward pressure of the cornu, as just described, is the cause of the increase in depth of the central grey substance (*r*). The remaining portions of the posterior white columns (*p*, *p*) were soft, and friable when hardened, but closely applied to the membranes of the cord. At the lower part of the dorsal region, for a short distance, the posterior cornu, in some places on the right side, in others on the left, was *entirely* wanting; while at many of the intervening points, as shown in fig. 3, the cervix cornu posterioris (*f*) and anterior cornu (*g*) on the left side were wasted to a considerable degree. The wasting was most remarkable in the anterior cornu, which was reduced, in fact, to a mere streak. On ascending, however, towards the *middle* of the back, the grey substance on both sides resumed its normal appearance.

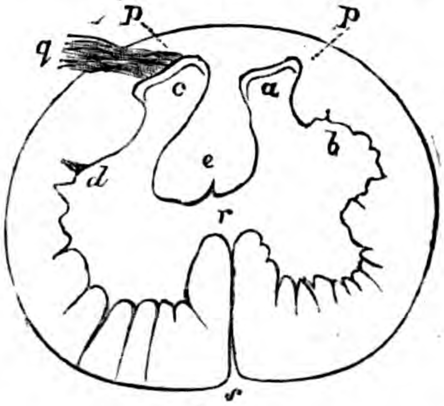


Fig. 2.

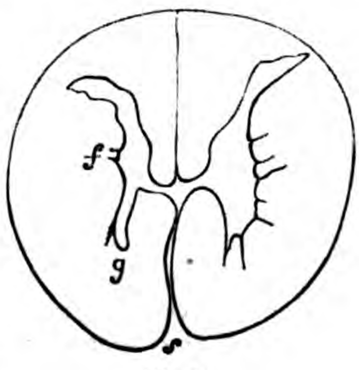


Fig. 3.

The *upper* half of the dorsal region, as already stated at the beginning, was completely broken up and lying as a pulp in the membranes. This condition appeared to be a consequence partly of disease and partly of mechanical injury received on its removal.

The cervical enlargement was much increased in diameter from a swelling of both the white and grey substance, but especially of the former. This swollen condition of the nerve-substance is a usual consequence of inflammatory action; but still I was unable to find any well-marked granular exudation-corpuscles. Whether the absence of these bodies might be due to their decomposition during the long interval that elapsed before the part was examined, I cannot say. In the same region, also, the cord was considerably altered in shape, for instead of being flattened from before backward, it was perfectly cylindrical. Its grey substance, likewise, was in many places strangely misshapen, or unsymmetrical, or deficient to a variable extent. In the upper half of the enlargement, on a level with the fifth cervical nerves on the right side, it was almost wholly obliterated, only the caput cornu, or dilated extremity of the posterior horn, remaining, with one or two slender streaks of grey substance in place of the *anterior* cornu. In this, as in other regions, wherever there was any evidence of disease, the *posterior* columns were much softer than the others; and when a section of the cord, wet with spirit, was placed in water, they immediately separated *en masse* from between the posterior horns and from the posterior commissure. At one spot, higher up, on a level with the middle of the third cervical nerves, the whole of the *cervix* cornu, and therefore the *tractus intermedio-lateralis*, on the right side, were wanting. A little above this point the grey substance gradually resumed its normal appearance; but on a level with the highest roots of the same (third pair of cervical) nerves, it was quite destroyed at the junction of the cervix cornu posterioris with the anterior cornu, and therefore at the part that ought to be occupied by the *tractus intermedio-lateralis*, which is traversed—as I have elsewhere shown—by the lower roots of the spinal-accessory nerve, in its way to the cells of the anterior cornu.* On the same level, the inner part of the anterior cornu adjoining the transverse commissure was likewise wanting.

Opposite the *lower* roots of the *second* cervical nerves the whole of the posterior cornu (both caput and cervix) on the *right* side, as far forward as the transverse commissure, was completely obliterated; and a few sections higher up, a similar obliteration, to nearly the same extent, was found on the *opposite* side; while on a level with the *middle* roots of the same nerves a transverse section of the cord presented the curious appearance of an almost perfectly blank or white area, scarcely a vestige of grey substance remaining. Opposite the *upper* roots of the same pair of nerves, however, the grey substance began to reappear, but at first in more or less isolated and irregular portions. At the first cervical nerves it had nearly regained its healthy appearance; but the posterior white columns were much softer than the rest.

The nerve-cells of the cord did not appear to have suffered degeneration to any appreciable extent. Of the pathological condition of the nerve-roots I am unable to speak with confidence, as they were too much affected by post-mortem changes to allow of my arriving at any certain conclusion on the question of disease.

* See my Researches in Philosophical Transactions for 1859, Part I. pp. 450–51, and Plate xxv. fig. 12, *f*, F, *f*.

So far I was able to proceed upon sure ground. Although it must be regretted that the history of the patient is not more complete, and that the cord was not in a condition more favourable for examination, the case is still, in many respects, one of great interest and importance. If it presented no other points of interest, it would be important from the fact alone of its affording another instance of the connexion of "wasting palsy" with structural disease of the spinal cord. Although the muscles of the upper extremities and of the chest were not actually paralysed, they were much wasted, and the patient experienced, to a certain extent, a loss of power not only over their contraction, but over their co-ordination. He found a difficulty in shifting his needle, and could not grasp it tightly. Although he could partly undress himself, he could not pick up a pin. Accordingly, we found that the brachial enlargement was extensively injured by disease; that at the part of it which gives origin to the *upper* roots of the brachial plexus, the grey substance was almost wholly obliterated; and that the posterior columns, particularly their deeper portions, were considerably softened.

In the lower part of the lumbar enlargement we saw the strangely abnormal appearances which the cord presented. The grey substance was compressed, as it were, into a single mass, and the posterior white columns were almost entirely wanting. From these appearances I inferred that there must have been some lesion of motion, if not of sensation, in the lower extremities; but to my astonishment, on inquiry, I was informed that they seemed to be unaffected, and that the man walked apparently quite well. In the detailed history of the case, however—which I only subsequently received—we learn that "his right foot was affected in some way. He said it would go down with a 'clap' in walking—'there was no spring in the instep.'"

We have no record of symptoms corresponding to the lesions that were found in the dorsal region of the cord. Such symptoms, with the lesions of structure on which they were dependent, may have made their appearance only or chiefly during the patient's last attack of illness. I was quite unable to ascertain the nature of this attack, and the manner in which he died. He died probably from paralysis of the respiratory muscles; for we have seen how extensively the disease had invaded also the cervical region of the cord. At the origin of the thoracic nerves supplying the pectoral and serratus magnus muscles, almost every vestige of the grey substance except the caput cornu posterioris had disappeared. The *lateral* part of the grey substance between the anterior and posterior cornua—a part which I have named the *tractus intermedio-lateralis*—and with which the spinal accessory nerve is intimately connected, was also, as we have seen, in many places wholly destroyed; while at all other parts from which these nerves collect their rootlets the grey substance was more or less damaged.

The case, in regard both to many of the symptoms and some of the lesions of structure, has a strong resemblance to the disease which has recently been termed by the French—"Ataxie locomotrice progressive."

Art. V.

On Tumours in Voluntary Muscles; with an Analysis of Sixty-two Cases, and Remarks on the Treatment. By W. F. Teevan, B.A., F.R.C.S., Surgeon to the West London Hospital, Demonstrator of Anatomy at the Westminster Hospital, and formerly Demonstrator of Anatomy at University College, &c.

Tumours affecting muscles, in common with other parts, are not infrequent, but it is exceedingly rare to find them developed and isolated in the body of a muscle. For the surgeon they possess great practical interest, and, so far as I have been able to ascertain, but little mention of them is to be found in any author, no statistics have been collected regarding them, and no definite rules laid down for their treatment.

T. W. Chevalier, who obtained the Jacksonian prize essay in 1822, for his dissertation on the 'Injuries and Diseases of the Muscular System,' has made a few remarks on 'Tumours, as affecting Muscles.' "They are, for the most part, independent of the muscles, unless being confined under them they encroach and irritate and so form adhesions, or unless they may have begun in the cellular substance which partly composed those organs." Nearly all his evidence is entirely of a negative character.

According to Rokitansky,* "The muscular system is rarely the seat of morbid growths, except when it is involved in those which have originated in other tissues."

Gross† is of opinion that "Various morbid growths occur in and among the muscles."

In Holmes's 'System of Surgery,'‡ it is stated that "Tumours of different kinds are occasionally found in muscles."

J. C. Warren,§ in his well-known work, when considering "muscular tumours," states—"These are formed in the substance of the muscles. They are not very common. On external examination they are less distinctly defined than steatomatous tumours, and less moveable. When the muscles in which they are situated are perfectly relaxed they possess a considerable mobility; when they are firmly contracted the tumour is quite fixed, and these circumstances constitute their most remarkable character. The diseased part is not very easily distinguished from the healthy; so that in operation it is necessary to trench deeply into the surrounding muscle. They are more frequently accompanied with pain than cellular tumours; and more disposed to degenerate into malignant affections. Their origin is often traceable to an accidental injury, a blow, strain, or continued pressure. After removal they are apt to reappear." He also relates a few cases, the nature and origin of some of which must be considered very doubtful.

* Sydenham Society's edition, vol. iii. p. 312. † Gross's Surgery, vol. i. p. 747.
‡ Holmes's Surgery, vol. iii. p. 539. § Warren on Tumours, p. 64.

Bouisson* makes some general remarks—"Les tumeurs qui se développent dans l'épaisseur des muscles, et dont on possède une connaissance exacte, sont encore fort peu nombreuses. Le cœur est le seul organe musculaire dans lequel on ait signalé des productions morbides variées ; mais ces lésions n'intéressent qu'au point de vue de l'anatomie pathologique, et souvent elles ne sont reconnues qu'au moment de l'autopsie. Quant aux muscles de la vie animale, ils ont à peine été compris dans les investigations qui pouvaient éclairer ce sujet ; et si l'on excepte quelques tumeurs inflammatoires, quelques hypertrophies limitées, divers kystes contenant des liquides ou des entozoaires de différentes espèces, c'est à peine si l'on a recueilli quelques cas de tumeurs, resultant d'une lésion spéciale du tissu musculaire, et dans lesquels la symptomatologie et la thérapeutique aient été l'objet d'une attention fructueuse."

Parmentier† has made some remarks on cancerous tumours in muscles, and Demarquay‡ has written a chapter on erectile tumours in muscles. To both these authors I shall have occasion to refer.

Liston§ stated, "Structural disease of any kind in muscle is indeed but rarely encountered."

Various writers have discussed the subject of syphilitic tumours in muscles. It is not my intention to include them in the class of cases under consideration, for I think they scarcely come under my definition, and ought rather to be looked upon as inflammatory swellings or exudations. I may mention, however, that to Bouisson belongs the merit of having been the first to point out their pathology and treatment.

I have from different sources collected the records of upwards of one hundred cases of tumours in muscles, but as, in many instances, the tumour affected several contiguous muscles, it might be fairly objected, that the growth did not originate in the body of the muscle, but in the intermuscular space. I have therefore excluded all such. Although the tongue is often the seat of tumours, yet its muscles are so small that it would be exceedingly difficult to single out any one as affected in a given case. For the same reason many other muscles will also escape notice.

In selecting the extracts of cases I have endeavoured, as briefly as possible, to state only some of the more important facts, leaving the reader to refer to the authorities named for the details. In several instances the cases were wanting in particulars. The following are arranged according to the frequency of each description of growth :

1. *Medullary cancer* in the *pectoralis major* of a female, aged thirteen. The tumour, which was the size of the fist, and supposed to have been caused by a fall, was excised. The recurrence took place before the wound was closed, and death ensued a few months afterwards. (Compend. de Chirurgie, tome ii. p. 205.)

* Tribut à la Chirurgie, tom. i. p. 538.
† L'Union Médicale, Août 29 and 31, 1861. ‡ Ibid., Dec. 26, 1861.
§ Med. Chir. Soc. Trans., vol. for 1843, p. 127.

2. *Medullary cancer* in the *pectoral* muscle of a male. It was of large size, of eleven months' duration, supposed to have been caused by a sprain, and quickly proved fatal. (St. George's Hosp. Mus., Series 5.)

3. *Medullary cancer* in the *pectoralis major* of a man, aged thirty-four. It was as large as a melon, and of two years' duration. The tumour and entire muscle were excised, and death ensued on the twelfth day. (Ed. Med. and Surg. Journ. vol. ii. for 1861, p. 612.)

4. *Medullary cancer* in the *gluteus maximus* of a man, aged sixty-six. It was as large as a hen's egg, and of two months' duration. Excised. (L'Union Médicale. Août 29, 1861.)

5. *Medullary cancer* in *gluteus maximus* of a woman, aged fifty-five. The tumour was of the size of an adult head. It was excised together with part of the muscle. Recurrence took place in five weeks, and death followed three weeks after. (Med. Times and Gaz., vol. xxxvii. p. 655.)

6. *Medullary cancer* in the *sartorius* of a man, aged fifty-eight. The growth, which was as large as an adult head, and of two years' duration, was supposed to have been caused by laceration of the muscular fibres some years previously. Excision, followed by recurrence. (Lancet, vol. i. for 1861, p. 287.)

7. *Medullary cancer* in the *sartorius* of a young man. There were several tumours of a similar nature round the hip-joint of the same side. (St. George's Hosp. Mus., Series 5.)

8. *Medullary cancer* in the *deltoïd* of a female, aged twenty-three. The growth was of small size and was excised. Fungoïd tumours were reproduced in the vicinity and attained an enormous volume. Death took place two years after the operation. (St. Thos. Hosp. Mus., No. 32.)

9. *Medullary cancer* in the *deltoïd* of a man, aged forty-two. The tumour was as large as an orange, of six months' duration, and grew in the same place as that from which a recurrent fibroïd tumour had been removed some time previously. Excised. (Lancet, vol. i. 1861, p. 315.)

10. *Medullary cancer* in the *rectus abdominis* of a female, aged fifty. There was also a cancerous tumour in the left labium. At the post-mortem all the internal organs were found quite free from malignant disease. (Dub. Hosp. Gaz., April 1, 1846, p. 254.)

11. *Medullary cancer* in the *rectus abdominis* of a female, aged forty. Death took place from melanosis. (L'Union Médicale, Août 31, 1861.)

12. *Medullary cancer* in *biceps humeri* of a man, aged thirty-seven. The tumour, which was of one year's duration, was excised. Recovery. (Lancet, vol. ii. 1862, p. 700.)

13. *Medullary cancer* in short head of *biceps humeri* of a male and of two years' duration. Was excised and followed by recurrence. (St. George's Hosp. Mus.)

14. *Medullary cancer* in *soleus* of a female, aged fifty. The tumour was very large, and of two years' duration; was excised, and death

ensued from consecutive hæmorrhage. At the post-mortem, no trace of cancer could anywhere be found. (L'Union Médicale, Mars 6, 1851.)

15. *Medullary cancer* in the *rectus femoris* of a male, aged seventy. This tumour, which was of the size of the fist and of two months' duration, was supposed to have been caused by a fall. It was excised, and recurred four times. (L'Union Médicale, Août 29, 1861.)

16. *Medullary cancer* in the *biceps femoris* of a young woman. Amputation at hip-joint, followed by recovery. A previous growth had been excised. (Exhibited at Path. Soc., Jan. 6, 1863.)

17. *Medullary cancer* in the *gastrocnemius* of a male, aged eighteen. The tumour, which was of nine months' duration, was excised. It rapidly grew again, and was excised a year after the first operation. Three weeks afterwards fungoïd tumours appeared in the sore, and the leg was amputated above the knee. Death took place ten weeks from date of amputation. At the post-mortem no cancerous deposit was found in any other part of the body. (St. Thos. Hosp. Mus., No. 30.)

18. *Medullary cancer* in the *crureus.* There were two tumours, each the size of a walnut, contained in cysts. (Bull. de la Soc. Anat. de Paris, vol. for 1859, p. 10.)

19. *Scirrhus cancer* in the *brachialis anticus* of an old woman, of three years' duration and the size of an orange. The arm was amputated, and patient recovered. (Lancet, vol. i. 1860, p. 118.)

20. Several small, "oval, hard, and white" carcinomatous tumours in a *pectoral* muscle. The fasciculi were described as healthy. (Mus. R. C. S., No. 345.)

21. *Melanotic cancer* in the *rectus femoris* of a young female. The tumour was encysted, of the size of a hen's egg, and of six months' duration; was excised, and patient recovered. (J. C. Warren on Tumours, p. 65.)

22. *Fibrous* tumour in the *biceps humeri.* It was of the size of a walnut, and was taken from the body of a man, aged sixty-five, who died from chloroform when about to have the thigh amputated for a large tumour of the same nature. (Trans. Path. Soc., vol. vii. p. 340.)

23. *Fibrous* tumour in the *deltoïd.* (Lancet, vol. i. 1857, p. 186.)

24. *Fibrous* tumour in the *gastrocnemius* of a girl, aged nine. It was of ten months' duration, and the size of a walnut; was partly excised, but recurred, and leg was amputated above knee-joint. (Trans. Path. Soc., vol. vi. p. 345.)

25. *Fibrous* tumour in the *pectoralis major* of a female, aged forty, of the size of a hen's egg, and followed a blow. Excision; recovery. (Lancet, vol. i. 1861, p. 264.)

26. *Fibrous* tumour of *trapezius.* It occurred in a female, aged fifty-five, was of nine months' duration, and was excised. A previous growth had been removed. (Mus. R. C. S., 222 *a.*)

27. *Fibrous* tumour from the *vastus internus.* (Bull. de la Soc. Anat. de Paris, vol. xix. p. 78.)

28. *Fibrous* tumour in a muscle not named. (Mus. St. Barth. Hosp., No. 305.)

29. *Fibro-adipose* tumour in the *pectoralis major* of a male, aged twenty-nine, was as large as an orange, of four years' duration, and was excised. (Lancet, vol. i. 1857, p. 186.)

30. *Fibro-adipose* tumour in the *biceps humeri* of a man, aged nineteen. The growth, which was four ounces in weight, and of one year's duration, was excised, and followed by recovery. (Lancet, vol. i. 1854, p. 518.)

31. *Fibro-plastic* tumour in the *pectoralis major* of a male, aged twenty-three. It was as large as a fœtal head, of six months' duration, and was excised with nearly all the muscle. Two years later it recurred. (L'Union Médicale, Août 29, 1861.)

32. *Fibro-plastic* tumour, with osseous walls, in the *latissimus dorsi* of a young woman. It was excised, and patient recovered. (L'Union Médicale, Nov. 10, 1861.)

33. *Fibro-cartilaginous* tumour in the *semi-membranosus* of a boy, aged fourteen. It was of the size of a fist, and was excised. (Holmes' Surgery, vol. iii. p. 539.)

34. *Fibro-cartilaginous* tumour in the *deltoïd* of a young man. It was the size of an egg, and was excised. (Holmes' Surgery, vol. iii. p. 539.)

35. *Recurrent fibroïd* tumour in the *masseter* of a man. It was as large as a walnut, of four months' duration, and was excised. (Holmes' Surgery, vol. iii. p. 540.)

36. *Fibro-nucleated* tumour in the *rectus abdominis* of a male, aged twenty-seven, of the size of a turkey's egg, and supposed to have been caused by an injury. Excision; recovery. (Med. Times, vol. xxxii. p. 321.)

37. A tumour described as *albumino-sarcoma* in the *triceps humeri* of a boy, aged twelve; was as large as half an orange, of three years' duration, and was excised. (Med. Times, vol. xxxiii. p. 211.)

38. *Myeloïd* tumour in the *deltoïd* of a female, aged twenty-seven, of great size, and two years' duration. The entire muscle, with the acromion and scapular extremity of the clavicle, was excised. Recovery. (Med. Times, vol. xxxiii. p. 334.)

39. A *cyst* in the *biceps humeri* of a female, aged twenty, as large as a walnut. (Holmes' System of Surgery, vol. iii. p. 540.)

40. *Cysts* in the *biceps humeri* of a female, aged twenty-two. Were cut open, and allowed to suppurate. (Lancet, vol. ii. 1859, p. 662.)

41. A *serous cyst* excised from the *gluteus maximus.* (St. George's Hosp. Mus., Series 45.)

42. A *serous cyst* in the *rectus abdominis.* During lifetime the tumour was supposed to be a chronic abscess. (Lond. Med. Gaz., vol. xli. p. 217.)

43. A *cyst* in the *pectoral* muscle. (St. Barth. Hosp. Mus., No. 202.)

44. A *sanguineous cyst* in the *gastrocnemius* of a male, aged thirty-three. It contained half a pint of blood, and was of long duration. (Trans. Path. Soc., vol. viii. p. 363.)

45. A *cystic* tumour in the *adductor magnus* of a boy, aged four-

teen. It was as large as the adult head, and of nearly two years' duration. Excision of part, followed by death in forty-eight hours. (Lancet, vol. i. 1856, p. 371.)

46. A *cyst* in the *sartorius*, containing cretaceous matter. (Bull. de la Soc. Anat. de Paris, vol. for 1859, p. 10.)

47. A *hydatid* tumour in the *biceps humeri.* (L'Union Médicale, Août 31, 1861.)

48. A large *acephalocyst hydatid* in the *gluteus maximus* of a female, aged forty. It was of five years' duration, was excised, and patient recovered. (Mus. R. C. S., No. 345.)

49. A *hydatid* cyst in the *rectus abdominis* of a man. (Comptes Rendus de la Soc. de Biol., vol. for 1852, p. 6.)

50. A *hydatid* in *rectus abdominis.* (Mus. R. C. S., No. 597.)

51. A small *hydatid* tumour in the *deltoïd* of a man, aged seventy. It was the size of a walnut, and of six months' duration. (Gross' Surgery, vol. i. p. 747.)

52. *Erectile* tumour in the *semi-membranosus* of a male, aged ten, of eight years' duration. Excision; recovery. (Med.-Chir. Soc. Trans., vol. for 1843, p. 120.)

53. *Erectile* tumour in the *external oblique.* It was of congenital origin, and was excised. (Med. Times, vol. xxxii. p. 321.)

54. *Erectile* tumour in the *supinator longus* of a female, aged twenty-eight. It was of nine years' duration, and was excised. Recovery. (L'Union Médicale, Dec. 26, 1861.)

55. *Erectile* tumour in the *semi-tendinosus* of a female, aged seventeen. Excised. (St. Barth. Hosp. Mus., No. 118.)

56. *Erectile* tumour in the *sterno-mastoïd.* The growth was of four years' duration, and as large as a turnip. Its excision was followed by recovery. (Med.-Chir. Soc. Trans., vol. for 1843, p. 128.)

57. An *osseous* tumour in the *adductor longus.* (Bull. de la Soc. Anat. de Paris, vol. xv. p. 396.)

58. An *osteo-calcareous* growth in the *extensor communis digitorum.* (Bull. de la Soc. Anat. de Paris, vol. xxiii. p. 15.)

59. An *osseous* tumour in the *rectus femoris* of a male, aged fifty-six. Death from cancer of the stomach. (Bull. de la Soc. Anat. de Paris, vol. xxx. p. 3.)

60. Tumour, described as *muscular*, in the *sterno-mastoïd* of an old woman, and supposed to have been caused by a blow. It would appear to have been a cyst. (Lancet, vol. ii. 1861, p. 349.)

61. Tumour, described as *muscular*, in the *masseter* of a man, aged thirty-five. Excision; recovery. (Med. Times, vol. xxxiv. p. 243.)

62. Tumour, described as *innocent*, in the *sartorius* of a female. Excision; recovery. (Amer. Med. Jour., April, 1853, p. 363.)

It will thus be seen that out of 62 cases the relative numbers of each growth were—cancerous, 21; fibrous, alone and in combination, 16; cystic, 8; hydatid, 5; erectile, 5; osseous and osteoïd, 3; doubtful nature, 3; myeloïd, 1. The cancerous tumours were the most numerous, being rather more than one-third of the whole; but, from an examination of the other cases that I have excluded, I am convinced that their proportion is very much greater than this. It would also appear that the tumours affected the muscles of the lower extremity almost as often as they do those of the upper limb; but with this difference, that in the latter they are almost entirely confined to the pectoralis major, deltoïd, and biceps, whereas in the former they are very equally distributed. The muscles of the trunk, and head, and neck were rarely the seat of tumours, with the exception of the rectus abdominalis, which would appear very subject to them.

The above facts are somewhat opposed to the opinions of the authors already named; for Gross states that he had only seen one case of hydatids, that melanosis may occur, but that encephaloïd, colloïd, and scirrhus are very rare. Tatum thinks the fibrous tumour is the most frequent, that encephaloïd may exist, but that scirrhus seldom obtains. Rokitansky is of opinion that cysts, with the exception of those that enclose entozoa, are very rare, that bony growths not unfrequently exist, but that cancerous formations are extremely uncommon. Chevalier was not aware of any case of hydatids in the voluntary muscles, and thought that cancer never began in them. Erectile tumours must be exceedingly rare. I have only been able to meet with three cases, in addition to the two described by Liston,* who states, in reference to one of them, "The author is not aware of its having been found in muscular substance. In the preceding case, however, it is more than probable that a small mass of erectile tissue had originally existed in the muscle, and had gradually become developed, till, at two years of age, it attained such a size as to attract attention." Rokitansky mentions that a growth composed of true bone is often found in the left deltoïd muscle of recruits, and hence named the drilling bone. I believe the earliest recorded case is to be found in the 'Journal der Chirurgie und Augen-Heilkunde,' for 1830, p. 141. This curious pathological specimen does not seem to have been observed in this country.

It is a remarkable fact, that lipoma, which is so frequent in the subcutaneous cellular tissue, has never as yet, I believe, been found in the interfibrillar connective tissue. Parmentier is, I think, the only author who has pointed out this singularity, and he goes so far as to say that, when diagnosing a tumour in a muscle, we may entirely discard the chance of its being lipomatous.

Is there any such growth in the voluntary muscles composed of striped fibre which would merit being called a true "muscular tumour"? In the prostate and uterus there are, without doubt, new formations made up of unstriped fibres, but I have not been able to meet with any case to which the above name could be applied. The few that I have met with described as muscular admit of great doubt. Paget states,† "I have not, indeed, seen such a specimen as would quite justify the name of 'muscular tumour,' assigned by Vogel." Still, I think there is no reason why it should not exist, for Rokitansky has

* Trans. of Med.-Chir. Soc., vol. for 1843, p. 127.

† Paget's Surgical Pathology, p. 476.

met with transversely-striated cells in a tumour of the testis, and both Virchow and Kölliker have discovered "elongated, fusiform, transversely-striated cells" in an ovarian tumour. Virchow* expressly states that, in pathological formations, "those elements are most rarely imitated which belong to the more highly organized, and especially to the muscular and nervous systems. Still, these formations are by no means excluded; we find pathological new formations of every description, no matter to what tissue they may be analogous, provided it possess distinctive features. It is only with regard to their frequency and importance that a difference prevails."

Most writers are of opinion that the muscular system is only secondarily affected with cancer. Paget† states, "I have never seen a primary scirrhus cancer in a muscle." According to Rokitansky,‡ "in whatever form this disease presents itself, it is scarcely ever the primary cancerous affection in any muscle of animal life except the tongue. One or more cancerous growths are almost always found elsewhere, and that in the muscular system is the secondary affection." Walshe,§ however, inclines to an opposite belief. "Bayle states that the muscles of locomotion are not observed to be affected with primary cancer, in which respect they differ from those of organic life. The fasciculi of Cruveilhier, however, prove that small cancerous masses may be developed secondarily in the muscles of animal life, without any direct continuity with the original disease; and I have myself seen primary encephaloïd infiltration of muscular substance." He is also of opinion that primary scirrhus may likewise occur. I think there can be no doubt that medullary cancer may exist in muscles as a primary growth or infiltration. Most of the cases of encephaloïd that I have enumerated were primary cancers, and three of them were proved post-mortem to have been such. Primary scirrhus, however, must be excessively rare. I have only been able to meet with two cases of it, and they are both exceedingly doubtful. The preparation in the Museum of the College of Surgeons of a pectoral muscle, with several small, "oval, hard, and white" carcinomatous tumours in it, is, in all probability, an instance of that muscle secondarily affected in scirrhus of the breast.

Whence do tumours in muscle arise—from the fibril or the interfibrillar connective tissue? It would seem to have been the opinions of most writers that they arose in the interstitial connective tissue.

Gross‖ expressed the general belief when he wrote: "The probability is that none of these heteroclite formations are developed in the muscular substance, properly so called, but that they begin in the interfibrillar tissue, from which, as they increase in size, they gradually encroach upon the fleshy fibres, which they thus displace, alter, or destroy." There can, I think, be no doubt that the non-malignant tumours originate in the interfibrillar tissue. The microscopical examinations of cases would seem entirely to point that way; and fibrous tumours can often be demonstrated as arising in the interstitial tissue,

* Cellular Pathology, p. 63.

† Op. cit. p. 608. ‡ Op. cit. p. 317.

§ Walshe on Cancer, p. 97. ‖ Op. cit. vol. i. p. 748.

the muscular fibrils being wholly intact. Regarding the occurrence of cancer in muscle, it used also generally to be maintained that it had its origin in the same connective tissue. However, Cruveilhier and Lebert were the first, I believe, to point out that it sometimes arose in the fibril itself. They both brought forward cases in which they had discovered cancer "dans l'intérieure des cylindres musculaires;" and, in the last edition of Paget's work, it is stated that Weber, Neumann, and Turner have all demonstrated the development of cancer-cells in muscular fibril. It thus appears that cancer may commence either in the interfibrillar tissue or in the fibril, and that it may affect the latter as a distinct tumour, or as an infiltration, or as a degeneration, merely replacing the fibril, and not causing any alteration in the shape of the muscle. However, the fibril is far more likely to be diseased than the connective tissue, as is proved by the fact of the latter so often remaining wholly unaltered in fatty and cancerous degenerations of muscle, and the sheath of the muscle is the last to give way to disease, for nothing is more remarkable than the fact that in psoas abscess of many years' duration the sheath will be often found strong and resistant, and perhaps greatly thickened. It would, indeed, seem to be a conservative provision of nature, for it is very rare to find that pus has escaped into the abdomen. The sheath would seem to have fulfilled the office of a sewer, and to have conducted the pus to a point whence it might be harmlessly discharged. These facts, as will hereafter be seen, have a most important bearing.

Regarding the treatment of tumours in muscles, I desire to draw attention to two important points. Firstly, there is a class of swellings in muscles, the result of constitutional syphilis, which may simulate other growths, and so lead to probable error in diagnosis. It would seem that this description of tumour is very much more common than is perhaps supposed, for since attention was drawn to this point numerous cases have been, and constantly are being, recorded; and, when its cause is kept in mind, it is quite sufficient to put us on our guard lest a given tumour that we may happen to meet with be of this nature, and to remember always, though there may be no evidence or history of syphilis, yet that its effects may make themselves manifest, from time to time, when the constitutional taint may long since have faded away.

The greatest experience, and the most consummate diagnostic tact, will sometimes fail to discriminate softer forms of encephaloïd tumours from syphilitic swellings, when the skin is intact; and when we consider that operations have often been needlessly performed for growths supposed to be cancerous, but which turned out to be syphilitic, we cannot be too careful lest we also commit like errors. These swellings had long ago been observed by Astruc; and in Casper's 'Wochenschrift' for 1845, a tumour in the sterno-mastoïd was described which created considerable interest. In the same year also, Mr. Tatum read a paper at the Medico-Chirurgical Society, entitled, "Three Cases of hard circumscribed Tumours in Muscle disappearing under the influence of Iodide of Potassium;" and Nélaton states, that although he had himself often observed these swellings before Bouisson wrote, yet that to the latter must be ascribed the merit of having been the first to clearly point out their pathology and treatment in the 'Gazette Médicale' for 1846. Bouisson was not the first, however, to cure tumours with iodide of potash, for in 1836 Dr. Andrew Buchanan, then surgeon to the Glasgow Royal Infirmary, wrote a paper on the use of the drug for their absorption, and recorded that, by administering the medicine internally, he had cured a case in which the growth was situated in the calf of a woman's leg, although some surgeons had previously condemned the limb to be amputated. In 1839, Robert described a similar case. It is therefore of the greatest utility in doubtful tumours to administer this remedy for a few weeks, for it will either cure the disease or greatly facilitate the diagnosis. In most instances the delay of a few weeks can make but little difference; and it is well not to take imaginary evils into calculation. Some surgeons have great objection to make an exploratory puncture, but I think the great and valuable evidence so often afforded by it far outweighs any disadvantage attending it. A cold abscess with thick walls, when seated in the body of a muscle, will often puzzle an able surgeon. Bérard once cut down on to an abscess in the biceps, thinking that it was a solid growth. It is well known that the diagnosis between cancer and syphilis, when seated in the tongue or lip, is often by no means easy. When syphilitic swellings first form in the muscles they are generally fluid, and, if punctured, will at once subside, but if they have passed from the stage of induration to that of ulceration, they may very much resemble a malignant tumour which has eaten through the skin. I think, therefore, that the exploring-needle and iodide of potash will be found useful auxiliaries in cases of difficult diagnosis.

Secondly, what operation, if any, ought to be performed for a cancerous tumour in a muscle?

Surgeons generally extirpate innocent tumours seated in the soft parts of the limbs, and not involving the bones. I think, however, when the tumour is very large, has deep connexions, or is situated in the thigh, that the practice is very questionable. On looking over the records of cases for some years past, I find there have been many instances of large innocent tumours in the thigh, especially in the inner side. They have generally been excised, and recovery from the operation has been quite the exception. Occurring, as they usually do, in young healthy subjects, the great mortality is still more remarkable. Some of the French surgeons have found the above line of treatment so uniformly fatal in their own practice for similar cases, that they have been led to adopt a different course, and I think their opinions are worthy of serious consideration. I believe the reasons for the great mortality are to be found in the facts of a prolonged dissection (often violent), the production of an enormous wound (often lacerated in its deeper parts), and the necessarily lengthened period that it is requisite to keep the patient under chloroform. Now in amputation the opposite conditions obtain. Sanson and Robert* are both of opinion that in these cases amputation is preferable to excision. "M. Robert se rapelle avoir observé plusieurs cas de ce genre. Les tumeurs occupaient toute la partie moyenne et interne de la cuisse, elles ne paraissaient pas adhérer aux os. Quatre fois il fit l'extirpation et quatre fois la mort s'ensuivit. La première malade était couchée à la Pitié dans le service de Sanson. Ce chirugien, instruit déjà par des cas observés auparavant, proposait la desarticulation coxo-fémorale plutôt que l'extirpation, qu'il considérait comme nécessairement mortelle. . . . . En resumé, M. Robert aujourd'hui conseillerait plutôt l'amputation que l'ablation qui donne naissance à une plaie beaucoup plus grave." Maisonneuve extirpated the calf of the leg for a large tumour in it; the operation was fatal, and called forth the following remarks from Lepelletier, who witnessed and recorded the case :—"Après ce resultat il est permis de se demander si l'amputation de la cuisse n'aurait pas presenté plus de chances de succès. Telle est mon opinion; et peut-être M. Maisonneuve hésiterait-il à pratiquer de nouveau cette opération dans un cas semblable. Je ne crois pas que le désir de conserver un membre aussi important que la cuisse autorise une opération dont les suites peuvent être plus graves que celles de l'amputation. Si on considére en effet, l'étendue et la profondeur de la plaie produite par l'extirpation, on comprendra facilement à combien d'accidens graves la malade était exposée. Les suites de cette opération l'ont prouvé."

Chassaignac and Verneuil are also in favour of amputating rather than excising in these instances.

Not long ago, it was a doubtful point whether life were prolonged by operating in cases of cancer. Now, however, I think the matter must be considered to be set at rest by Mr. Sibley's and Mr. Baker's statistics, which clearly show the increased average duration of life that is gained by operating. It is therefore the surgeon's duty to operate, unless, in a particular case, some condition or fact forbid it.

No doubt from year to year the belief of the profession will oscillate between the local and constitutional origin of cancer till the question is finally settled. At present, however, the opinion would seem to be strongly setting in favour of the belief that cancer is a local disease, and Dr. Wilks states that it has a majority of evidence on its side. The teachings of such men as Virchow and Hughes Bennett will tend strongly to strengthen this view, which is certainly the more utilitarian of the two, for who would devote his time and energies to find out a cure for the incurable? The *onus probandi* of the entire controversy clearly rests with those who assert that a cancer is a constitutional disease, and, until such affirmation be proved, no surgeon is justified in its belief.

What operation, then, ought to be performed for a cancerous tumour in a muscle? On searching the works of past surgeons, I come to the conclusion, that in cases of malignant disease affecting the soft parts of limbs, they generally amputated. If, however, the present English

* Gazette des Hôpitaux, June 28, 1856.

surgical works be examined, it will be found that, in nearly all, no directions whatever are laid down as to what kind of operation is to be done in the above cases. Those that make any allusion to the subject recommend the disease to be thoroughly cut out; one author only states that, "In the limbs, as a general rule, amputation is preferable to a local, which is often necessarily a partial, extirpation." The American surgeon is, however, very clear on this point. "A valuable rule in tumours is to excise the benign and to get rid of the malignant by amputation;"* and, "when the disease is seated in an extremity, especially the distal portion, the proper operation is amputation, not excision."†

I find, from an examination of the different journals for many years past, that surgeons generally excised cancerous disease when seated in the soft parts of limbs, and not involving the bones, and unvaryingly so when the skin was intact and the tumour had no deep connexions. They seemed to have been actuated by the very laudable desire of preserving the limb, and to have founded their treatment on the same principle as that followed in a cancerous breast.

I shall now endeavour to show, that any operation, for the removal of a cancerous tumour in a muscle, which takes away the growth and leaves behind that structure in which the disease commenced is wrong in principle and contradicted by analogy.

If a malignant growth, however small, affected the mammary gland, no surgeon would ever think of excising that part of it which seemed diseased, leaving behind that which was apparently healthy; he would be content with nothing less than the excision of the entire breast; and if a tumour of the same nature affected the lower part of the femur, he would not amputate through that bone, but would disarticulate the limb at the hip-joint; in each case removing the whole of that structure in which the disease originated and was situated.

Why, therefore, should a surgeon cut a cancerous tumour out of a muscle, and leave behind the structure in which it commenced? It is clearly wrong; a malignantly affected structure ought always to be cut out and not cut through. The following observations apply almost verbatim to the subject: "Still, the ascertained fact of the encephaloïd tumour being occasionally combined with unsoundness of bone to an indefinite extent, is sufficient to warrant the rule, that in such cases the amputation should, if possible, be performed not through the bone in which the disease originated, but either through the contiguous joint or above it."‡

Sir B. Brodie§ states: "It is to be observed, that in this instance the whole of the humerus—that is, the whole of the organ in which the disease was situated—was removed. It is probable that the success of the operation in such cases depends mainly on that circumstance." And further on|| he remarks: "I have no sufficient evidence to offer

* Op. cit. vol. i. p. 621. † Op. cit. p. 329.

‡ Stanley on Diseases of Bone, p. 174.

§ Diseases of the Joints, p. 273. || Ibid. p. 278.

in favour of an operation performed for the removal of a joint affected with malignant disease, in which a portion of the bone in which the disease has originated, is allowed to remain. Cases may have occurred in which there was no recurrence of the disease under these circumstances; but there has been no such favourable result in any of those in which I have had the opportunity of learning the patient's history afterwards; and, as I have already observed, it is not what our experience of the effects of operation performed for malignant diseases in other organs would lead us to expect. I confess that it seems to me that the rule of practice is sufficiently obvious; though there may be some difficulty in the application of it to individual cases, on account of our having no certain marks by which we may at all times, and in every instance, distinguish diseases which are malignant, and diseases which are not malignant, from each other."

If it be asked, why cancer so often returns after operation, I think the answer is to be sought for, not in an hypothesis, but in the material and visible fact that a cancer is generally cut out of cancerously diseased structures, and hence a return of the growth is only an exemplification of natural processes. It rarely happens, that when a malignant tumour is brought under the surgeon's notice that it is confined to the structure or part in which it originated, and as the most frequent operation for cancer is the excision of the breast, and as that procedure must be called an imperfect operation on account of the surgeon being limited in his incisions, I believe, that where we can find a cancer confined to, and isolated in one given structure that is capable of being thoroughly excised *en masse*, we have every reason to hope for better results than have been attained in operations on cancerous breasts.

Now, when a cancer is seated in a muscle there are two important facts to be remembered. The muscle is as obnoxious to disease as its sheath is resistant to the spread of malignancy. Miller has pointed out that dense fibrous tissue resists the invasion of carcinoma longer than any other texture; and Virchow expressly states that it is very little disposed to become diseased by contagion, whereas the soft structure of muscle is well adapted for the conveyance of cancerous juices, and hence likely to encourage the formation of new foci of disease. When once a cancer has commenced in a muscle, there is every condition present to favour the transmission of its juices from one end to the other. Independently of all physiological action, I believe that capillary attraction, gravitation, and muscular action are all, more or less, capable of diffusing the elements of the disease. On the other hand, the sheath can for a long time serve as a barrier against the infiltrating fluids, and so protect the surrounding parts. If, when the cancerous tumour is still confined within the sheath, we excise the entire muscle, we may reasonably believe that we have thoroughly enucleated the disease.

When the growth is in the rectus abdominis muscle, and adherent to the peritoneum, I would recommend Parmentier's advice,* and it

* L'Union Médicale, Août 31, 1861.

must be remembered that the above muscle is practically several muscles: "Enfin, s'il s'agissait d'une tumeur développée dans le grand droit de l'abdomen, et offrant quelque adhérence avec le péritoine, il ne faudrait pas hésiter, contre l'avis de Boyer, à achever l'opération, car il vaut mieux avoir affaire à une plaie pénétrante de l'abdomen que de laisser dans l'économie un produit qui fatalement causera la mort du malade."

Therefore, whenever practicable, a cancerous tumour in a muscle should not only be excised, but the muscle in which it originated ought to be cut out from its origin to its insertion. But if the cancer be of large size, or if the skin be affected, or if the wound resulting from the excision of the muscle would be of great extent, then the limb ought to be amputated, and the remainder of the muscle in the stump excised.

I have thus ventured to propose an operation which is founded on definite principles, is supported by analogy, and is in unison with the views of some of the most original thinkers of our time.

Art. VI.

Notes on "Historical Researches on the Use of Forceps in Extraction of Cataract." By Q. E. D.

An article in the last number of this Journal,* intended to ascertain the origin of the mode of fixing the eye in extraction by means of forceps, which has lately come into use in this country, seems to require some comment. Apparently with every desire to deal fairly with his subject, the author has fallen into an error which invalidates the conclusion arrived at, and tends to deprive one no longer in the active ranks of the profession of the meed which is his due.

The writer, Mr. Windsor, of Manchester, in pursuing his inquiry, has not fully recognised the fact that a given surgical instrument may be used in a variety of ways, of which one may be advantageous, another detrimental; and, having found the employment of forceps in the operation in question advocated by two or three continental surgeons, he stops not to inquire *how* they applied the instrument, but assumes (wrongly, as we shall show) the identity of their methods with that advocated in this country, and consequently ascribes to them erroneously the credit of devising the latter. It will be necessary to fill up the gap thus left in the examination of the subject. For this purpose, we will review the directions given by the surgeons alluded to; and it will then appear, that only by confounding processes widely different can the English mode be regarded as a mere importation of the continental.

As it is the originality of the former which has been called in question, we will first quote the description given of it by Mr. France, who is admitted to have been the first to broach this subject in England, in order to afford the opportunity of comparison with the processes of

* Historical Researches, &c., p. 219.

MM. Bonnet, Petrequin, and V. Gräefe, and of MM. Desmarres and Ruete.

Mr. France wrote as follows in the 'Guy's Hospital Reports' for 1860: "A pair of toothed artery-forceps should be selected, the dentations of which close at the extreme point of the nibs; as, if the latter are rounded off so that their teeth do not project and bite at the very point, the forceps is likely to take hold only of the loose conjunctiva. The patient (being recumbent) is first desired to look upwards, while the operator at his head depresses the lower lid. The open nibs of the forceps are then applied upon the exposed globe beneath the cornea, and made by slight pressure to scrape along the surface as they close, in order to seize, together with a fold of conjunctiva, the tissues beneath (including, if possible, a few fibres of the inferior rectus tendon), and convert the forceps into a firm handle to the eye. The instrument is now delivered to an assistant, resting his hand upon the cheek, while the operator proceeds to raise the upper lid, and apply his fore and middle fingers above and on the nasal side of the globe, in the usual manner, thus consummating his command of it.

"Should any inconvenient reflection from the surface of the eye, the natural conformation of the parts, or other circumstance, render an alteration in the exact position of the cornea desirable, this can now be effected at pleasure by a word to the assistant, who can gently draw it into, and then (the surgeon's finger co-operating) retain it steadily in, the precise position that is required. Meanwhile the attachment of the forceps serves simultaneously to keep the lower lid depressed, and enables the contact of additional fingers to be dispensed with. All things being thus prepared, the knife can be deliberately inserted, carried in a uniform, undeviating course across the anterior chamber, and be brought out accurately at the nasal margin of the cornea. No irregular movements of the globe delay the commencement of the section; no spasmodic inversion, without previous warning, obscures its progress, and invests its completion with sudden difficulty and danger; but the cornea remains stationary and central, alike while the cutting instrument pierces its temporal margin, while it divides the texture continuously, and when again it emerges at the nasal edge.

"As soon as the cornea is fairly transfixed by counter-punctuation, and a narrow isthmus alone remains for division, the knife itself holds the eye still, the iris lies safely behind the instrument, and the forceps must be detached at the same moment that the pressure of the fingers is withdrawn. The section is then completed, and the operation from this stage (which is indeed the turning-point of the whole) proceeds in the ordinary manner."

The corneal section, we should observe, throughout a series of forty-one cases attached to the paper here quoted, was invariably the upper one.

With the above description of Mr. France's method, we will now compare that of M. Bonnet, of Lyons. This able surgeon (the author of the valuable operation of 'Enucleation,' as a substitute for extirpation of the globe as heretofore performed,) seems to have been the first to conceive the idea of using artery-forceps for fixing the eye in extraction, and has some forcible remarks upon the utility of the contrivance, but he was not fortunate in the mode of operating he designed. He has the palpebræ kept apart by a pair of Pellier's wire specula; he then draws the eye into the desired position by one pair of forceps, to enable him to affix another (a pointed spring pair, as shown in his plate,) at the interval between the superior and external rectus. The latter instrument is delivered to the assistant, who holds the upper speculum, while the surgeon himself, taking the lower speculum in one hand, makes the section of the cornea, a lower or oblique one, with the other. He studiously avoids the application of any fingers to afford additional support to the globe, but depends exclusively upon the forceps; and he regards the puncture or laceration of the capsule with the curette as an exceptional step seldom necessary.

We will give M. Bonnet's own words, to avoid any risk of misrepresenting his views:

"Je fais coucher le malade: si je l'opère du l'œil du côté gauche, je me place à sa droite. L'élévateur et l'abaisseur des paupières sont appliqués et confiés chacun à un aide qui a soin d'éviter qu'ils ne compriment l'œil. Je tiens de la main gauche une pince à crochet sans ressort, de la main droite une pince à crochet avec ressort; avec la première je commence à saisir la conjonctive en haut et dehors de la cornée, et avec la seconde je saisis aussi solidement qu'il m'est possible la conjonctive et le fascia sous-conjonctival à 4 ou 5 millimètres de la cornée, un peu au dessus de l'angle externe des paupières. Cette pince est alors confiée à l'aide qui maintient l'élévateur de la paupière supérieure, elle fixe l'œil et l'empêche de se porter en dedans; je saisis alors l'abaisseur de la paupière avec la main gauche, et au moyen de kératotome je fais la section de la cornée. . . . . Dans quelques cas rares, il est nécessaire . . . . de faire une piqûre à la capsule cristalline."*

Now, this is obviously a very different mode of proceeding from that previously described, with which in the article in our last number it is attempted to be identified; and it is open to serious objections, to which Mr. France's method is not liable. Thus, the insertion of specula to hold apart the lids must tend of course to excite spasm, and impede the quiet closure and re-opening of the eye as the steps of the operation proceed; to leave the management of the upper lid to an assistant is particularly undesirable, and pregnant with embarrassment; but the total absence of all control over the globe, except that exercised by the forceps, is a defect of the gravest kind, and, indeed, amounts to a fatal objection to the plan. It cannot be said, on a careful consideration of this proposal—especially when the lower corneal section which it involves, and the omission of so essential a step as the opening of the capsule, are added to the conditions—that it constitutes any advance upon the operation as generally conducted. The advantage aimed at in the fixation of the globe by an instrument

* Bonnet, Traité des Sections Tendineuses, p. 316. Lyons, 1841.

is (as in so many past inventions for the same purpose) more than counterbalanced by the accompanying drawbacks. As, however, this mode of operation undeniably comprises the adaptation of forceps as an ophthalmostat, and was promulgated before the idea arose in this country, it is only right to say in this place, as we have authority for doing, that Mr. France was unacquainted with M. Bonnet's suggestion until the reference was furnished in our last number. Had, then, the methods of operation advocated respectively by these two surgeons even been identical, instead of widely different, the circumstance would only have affected the priority in point of time, not the originality of Mr. France's method.

Petrequin, for the most part, follows Bonnet implicitly, with the exception of enjoining the proper laceration of the capsule, and does not develope the germ of improvement contained in his colleague's system.* Pithily and rightly he condemns the perilous support of a second person's fingers in contact with the globe—"Je renonce aux doigts dont on ne peut diriger l'application;" but having his own preoccupied, he can supply the instrumental substitute only in their room.

Professor V. Gräefe, according to the article which has elicited these remarks, introduced the method of Bonnet and Petrequin into Germany. The statement appears to rest on the rather slender foundation of an "account given by Dr. Ravoth of Gräefe's method of performing cataract operations," in which occur the sentences: "It is, however, advisable to always make use of an ophthalmostat, and thus fix the eye."—"It is best fixed by seizing a fold of conjunctiva with delicate spring forceps." This vague allusion is all the evidence adduced as to V. Gräefe's practice, and it is certainly too indefinite to argue from as to the details of his manipulation. But allowing the correctness of the conclusion just referred to, "that Von Gräefe introduced this method (that of Bonnet and Petrequin) in Germany," we have only as a result, that the defective plan recommended by them was adopted occasionally there; but there is no approach to a proof that Mr. France's method was anticipated in Berlin, no warrant for the statements to this effect which have appeared in "Historical Researches."

The plan of Desmarres,† with whom we may combine Ruete, who copied from him, remains to be noticed. It is not improbably derived from that of Bonnet, but differs considerably from the latter. He gives the preference to one or two other instruments, which have never found favour in this country, and speaks rather discouragingly of the use of forceps. He writes no specific directions, but illustrates the mode in which he would employ the instrument, and shows it affixed on the *nasal* side of the cornea. Perhaps struck with the most conspicuous defect in Bonnet's proceeding, he supports the globe with the fingers which hold the lids above and below, and then makes the lower oblique section. But these four fingers, intended to command the lids and assist in counteracting the motions of the globe (and conse-

* Annales d'Oculistique, vol. vi. † Maladies des Yeux. Paris, 1858.

quently, on the delicate tact and consentaneous action of which so much depends), are not those of the operator, but of his assistant; while the forceps held by the surgeon is attached at the inner canthus, and displaces the supporting finger which should be there.

It is hardly necessary to contrast this process at any length with Mr. France's method, in which, as we have seen, the surgeon himself retains control of the upper lid, and supports the globe with the fingers in the usual position above and at the inner canthus. He entrusts to the assistant the simple duty of keeping the eye stationary with the forceps, previously affixed by himself beneath the cornea, where, in addition to fixing the globe, they necessarily maintain the lower lid depressed. It is plain that though a nearer advance is made by Desmarres to this method than by Bonnet and Petrequin, the two still differ most materially, and are practically quite distinct. So fully did Mr. France evidently feel this, and so little did he desire to conceal the source from which his method had been developed, that in two publications on the subject* he referred freely to the plan of Desmarres, pointing out what he deemed its defects, how his own plan met those objections, and even printing on a single page, by way of comparison, the figure given by Desmarres and his own. Obviously he considered the two methods distinct enough to allow of their juxtaposition in contrast, and had no apprehension of their being confounded as identical.

On reviewing the whole matter, sufficient, we think, has been said to prove that Mr. Windsor somewhat hastily ascribed to others an improvement which is really due to our own countryman, and that the conclusions he arrived at, if re-cast as follows, would more truly represent the actual facts of the case:

"1. That in 1841 Bonnet first formed the idea of employing forceps to fix the eye in extraction; that he applied it in practice, and that his example was followed by Petrequin."

"2. That Professor Gräefe introduced this method in Germany," and Desmarres reproduced a modification of it in Paris; but that in both these plans there are inherent defects, which neutralize the value of the suggestion.

"3. That Mr. France was the first to write on this subject in England;" that he devised a distinct method of using forceps in extraction; that he tested the same, with the best results, in a series of cases; and was, in fact, the originator of that mode which has since obtained extensive adoption in this country.

* Guy's Reports, 1858 and 1860.

PART FOURTH.

Chronicle of Medical Science

(CHIEFLY FOREIGN AND CONTEMPORARY).

HALF-YEARLY REPORT ON MICROLOGY.

By J. F. Streatfeild, F.R.C.S.
Assistant-Surgeon to the Royal London Ophthalmic Hospital, and to the Eye Infirmary of University College Hospital, &c.

Part I.—Physiological Micrology.

On the Minute Structure of the Spleen of Birds.—Dr. A. Timm, of Altona, ends his paper on this subject in the following words: "From the observations detailed and the other researches which I have had the opportunity of making on the spleens of different birds, the conclusion may be drawn that in all birds the spleen is composed of pulp and Malpighian bodies. Sometimes preponderating over the latter, at others scanty, we find them, even in the same animal, in very different developments, as can be easily proved by comparison of a large number of the spleens of the fowl.

"These Malpighian bodies are placed at or round the arteries. They are for the most part wanting in a special envelope. Only in some of the larger ones is found a distinct limitation through a layer of spindle-shaped cells, passing round in a simple series, which appear to come from the adventitia of the neighbouring arteries.

"They consist of an accumulation of roundish nuclei and cells having a great resemblance to the lymph-corpuscles of the animals in question. Between these cellular formations an anastomosing network of fine brilliant fibres with nuclei at the knot-points is found. Besides the arterial twigs which pass through the Malpighian bodies capillaries are found in them. These appear like ordinary capillaries, except that towards the edge of the Malpighian bodies they commonly show a dense layer of the nuclei which are found in the wall, commonly preceding their transition into the capillary wreath of the pulp. The similarity in structure of these capillaries with those of other organs points out that they are to be considered as vessels of nutrition of the Malpighian bodies, and, as far as it may be permitted to draw any conclusions from their structure to their duration of life, are not subject to considerable changes. At the ends of these capillary arteries are found in the owl peculiar capsules, about the significance of which I am unable to say anything positively.

"The spleen pulp consists of a dense vascular network and the peculiar spleen parenchyma. The latter consists of nuclei and cells quite like those in the Malpighian bodies, between which is found a network of anastomosing fibres, which likewise perfectly corresponds with that in the Malpighian bodies. These elementary parts form a system of longish, rounded islands, found lying between the capillary meshes.

"The arteries bifurcate with an extended course of the branches; they thereby lose in diameter of the lumen and in thickness of coat, until at last there only remains a homogeneous, doubly-contoured membrane. These ends commonly, at an obtuse bifurcation, pass over into the real capillary system of the pulp, which, moreover, receives the capillary artery branches of the Malpighian bodies.

"The capillary system of the pulp is formed of numerous short, unequally wide, but generally by comparison thick, little stems, which anastomose at obtuse or right angles. Thus, through the shortness of the single, in part thick, little stems, the whole network receives that pointed angular ramification of the same through the peculiar enlacements, which on that account are frequently seen in the enclosed cavities—a very characteristic effect, and very similar in different birds.

"The walls of these capillaries consist for the most part of an extremely delicate, very pale, homogeneous membrane, furnished with nuclei, without double contour; in some cases the wall is only formed by nuclei, very close together, which are not essentially different from those otherwise found in the pulp. In accordance with the delicacy of this coat, these capillaries are very much inclined to extravasations; this occurs often enough in injection experiments. The injection penetrates into the real islands of the parenchyma, passing between the cellular formations found there, and with sufficient force even the intercellular spaces of the Malphigian bodies may be almost completely filled with injection. By this the capillary net becomes much narrower, so that only one or two lymph-corpuscles find room in the interstices, as I have often had the opportunity of observing in fowls' spleens. These preparations agree exactly with the descriptions and figures which have been given by Axel-Key and Stieda of the capillary system of the spleen of mammals. They, however, by no means agree with the preparations which have been made by mere imbibition without any previous injection.

"The extraordinary delicacy and peculiar structure of the capillaries of the pulp are a most striking phenomenon. This bearing, by which they are forcibly distinguished from the capillary walls of all other organs in the grown animal prompts the question whether they are permanent formations, which, once complete, continue to exist through the whole time of life, or whether, at the same time with the surrounding parenchyma, they are subject to certain changes, which, according to the physiological condition at different periods, occur in different degrees, and perhaps often proceed very rapidly. This question, which has been repeatedly discussed and answered, now in one sense, now in another, can only be brought near to its solution when comprehensive researches into the structure and functions of the spleen in the different classes of animals lie before us."—*Zeitschrift für rationelle Medicin*, 3rd Series, vol. xviii. parts 1 and 2, pp. 184–6.

On the Anatomy of the Kidney.—Henle, the Professor of Anatomy in Göttingen, gives the following report of his work: "Even in reference to the first ramifications of the open uriniferous tubules proceeding from the points of the papillæ of the kidney, the author finds the statements made hitherto inexact; they divide in man and all mammals just as much tree-shaped as this has been hitherto described as being an exception in the horse; in reference to man specially, the ramification resembles a creeping, somewhat knotty shrub, the stems of which pass on some distance, slightly undulating, beneath the surface of the papilla, sending branches upwards, which are immediately again divided. Thereby the calibre of the tubules is rapidly reduced from 0·2—0·3 to 0·05—0·06 mm., and retains this diameter in its further course through the pyramid, inasmuch as further divisions do not occur at all or only rarely. The medullary substance does not owe its conical shape to an increase of the open tubules by division, but to a decrease taking place, in the direction from the basis to the point, of the second kind of fine tubules, which fill up the spaces between the open ones, abruptly bending round into one another at different heights of the pyramid by two and two as loops, and therefore may be called preliminarily loop-shaped. In the vicinity of the point of the pyramid we distinguish open and loop-shaped tubules, besides being doubly-contoured through their course, also (in cross sections) through the calibre, which in the loop-shaped tubules amounts to little more than 0·02 mm.; moreover, through the form of the epithelium, which in the loop-shaped tubules is flat, tessellated; finally, through the nature of the external envelope, which is a basement-membrane for the loop-shaped tubules, insoluble in potash and strong acids, whilst the simply-contoured envelope of the open channels is destroyed by maceration in hydrochloric acid, together with the stroma. All these distinctions are effaced towards the base of the pyramid. Whilst the calibre of the open channels decreases, the calibre of the loop-shaped is increasing; and indeed, for the most part, rapidly in a certain portion of their course, so that, at least in man, both kinds of tubules in the basis of the pyramid come pretty near to each other. The finer branches of the open tubules are, moreover, not deficient in basement-membrane, and the contrast between the cylinder epithelium of the open and the tessellated epithelium of the loop-shaped tubules disappears, as in the same measure as those tubules are narrowing and these enlarging, the epithelial cylinders become lower, the tessellated cells more important. Thus there remains nothing left on the boundary of the cortical substance of the originally so characteristic differences of the two kinds of tubules than a somewhat granular nature, and less sharp limitation of the epithelium cells of the loop-shaped tubules, differences which do not, in the confusion of the cortical tubules, sufficiently strike one in order to distinguish from each other their several continuations.

"In this stage of the investigation, injection must be had recourse to, and it produced, in the kidney of the horse and pig, decisive results. From the ureter always only a few of the cortical tubules are injected, and if to this it is objected that the injection might accidentally have remained incomplete, then this is confuted by the fact that the tubules which have remained uninjected are all of them distinguished from those which are injected in reference to their course, calibre, and the kind of their epithelium. The injectable tubules of the cortex reach, without further division or perceptible change of calibre, as far as the periphery of the so-called Ferrein pyramids (pyramidal continuations, as I call them). Here, or but little before, begins a new and plentiful ramification, by which the open tubules also take a part in the formation of the cortical substance. It is a rule that in the point of the pyramidal continuations, and often quite close under the fibrous investment of the kidney, the tubules of a pyramid by twos or of two neighbouring pyramids pass over, arch-like, into each other. Out of these arcades, turned with the convexity towards the surface, arise finer branches, which frequently again unite in arches, by which the appearance is something like that of the mesenteric artery. Other larger and smaller branches go from the sides of the arch at an acute angle downwards, and these pass in a winding course, forming loops in the layer of true cortical substance between the pyramidal continuations. All these mentioned branches unite into a net, partly wide-meshed, partly narrow. It accords with this mode of extension that if, by maceration in hydrochloric acid, we isolate the cortical tubules of a kidney, injected from the ureter, the little tubules filled with injection appear branch-like and of variable diameter; the non-injected tubules appear in larger number, more winding, without divisions, and of more uniform calibre. Moreover, all the tubules containing injection are clothed with a peculiar, very flat, and clear epithelium, bounded by a proportionably wide lumen, and appearing to consist of cylinder cells, the height of which is reduced to a very small measure, without any simultaneous diminution of thickness. The non-injected channels have all of them a very small lumen, and a thick granular epithelium, not distinctly separated into cells, the nuclei of which are sometimes hidden among the little granules. The capsules of the glomeruli do not fill themselves from the ureter, and therefore are not directly connected with the injectable tubules; on the other hand, we obtain them in kidneys pulled to pieces, in connexion with the non-injected urinary tubules. In order to save the hitherto valid view, that the capsules of the glomeruli pass over into tubules, emptying themselves upon the papillæ of the kidney, there remained yet one thing possible. The uninjected tubules might be continuations of the injected, and the injection might on that account stop short exactly at the entrance of the second kind of tubules, because these are almost completely filled up by the granular epithelium. Even this could not be refuted. That is to say if this were so, we must perceive in the cortical part of a kidney, the cortical tubules of which have been macerated to separation in hydrochloric acid, the connexion here and there of an injected tubule with one not injected. I have examined a sufficiently large number of kidneys dissected in this manner to be able to affirm that this does not occur.

"Thus I believe I have shown with certainty a system of kidney tubules forming a net in the cortical substance, out of which extended tubes pass into the medullary substance in order, by means of a number of trunks, to end upon the papillæ of the kidney. In the meshes of the net of these tubules there lie others of a different kind, winding in the cortex and taking their origin from the capsules of the glomeruli. How do the tubules arising from the capsules of the glomeruli bear themselves in relation with the medullary substance? This question is the only one which I must answer by an hypothesis, which, however, as will be allowed, has great probability. I assume that the last-named tubules of the cortex stand in connexion with the loop-shaped ones of the medullary substance. The continuity cannot be shown in individual tubules; it follows, however, from the homogeneity of the epithelium, and from this, that we do not perceive a different kind of termination, either in the loop-shaped tubules of the medullary, or in the uninjectable tubules of the cortical substance. Moreover, I have several times seen fine tubules of the medullary entering into the cortical substance, and beginning to wind about. Consequently, the loops would be festoons, connecting capsules by twos, hanging down into the medullary substance, first in a winding, and then simply in an arched course.

"All these facts referring to the tubules of the cortex concern, as has been mentioned, the kidney of the horse and the pig. They are applicable, however, without doubt, to the human kidney, which I have not yet been able to obtain in a fit state—that is, sufficiently fresh for injection of the tubules. For in the cortical substance of the human kidney are also found two kinds of little tubules, although not so strongly contrasted: the first, with clear epithelium, distinctly composed of cells, the other with granular epithelium, indistinctly separated into cells. From the circumstance that the uric acid infarct of children is found in the open tubules, that fat and chalk depositions, on the other hand, as well as the well-known fibrin cylinders, are contained in the loop-shaped tubules, the author concluded that the former separate the essential, the latter the watery elements of the urine. Of the blind channels would be valid what Bowman assumes of the uriniferous tubes in general, that they are filled with blood serum from the glomeruli, whose albuminous products are successively received by the epithelium."—*Zeitschrift für rationelle Medicin*, 3rd Series, vol. xix. part 1, pp. 112–16.

On the Blood-Corpuscles of the Frog.—M. di Vintschgau, in his paper on the results of his investigations, came to the conclusion that these little bodies do not possess an exterior membrane in the strict sense of the words, but consist of a mixture of liquid and firm particles, the protoplasm of Schultze enclosing the nucleus. The author could not, even with the highest powers and best definition, perceive a double outer contour. From the crushed corpuscles the colouring matter will escape without a trace of a rent; portions of the periphery may be torn off, without the contents being lost; a corpuscle may be lengthened by pressure and at last divide into two, without a split showing itself. Even the change of form which the corpuscles suffer from the addition of water Di Vintschgau considers as a proof of their not being composed of a membrane and liquid contents; at least we should be obliged, in order to explain why the corpuscles in the plasma are not globular, or why they do not uniformly swell out in water, to have recourse to the hypothesis that the membrane in the direction of the greater axis, had other physical qualities than in the direction of the lesser. The changes which the blood-corpuscles undergo from being kept longer in concentrated solutions of sugar and salt, the contraction of the coloured substance round a nucleus, and the ray-like processes towards the periphery, do not, from the liquid nature of the cell contents, appear to be explicable to the author. If, by the addition of iodine to the corpuscles discoloured and swollen in water, one has thought to colour the exterior membrane and render it visible, Di Vintschgau objects that iodine also made the contents of the discoloured corpuscles more consistent, and that, in this way, the artificial condition that is produced, proves nothing for the existence of a cell membrane. The way in which the blood-corpuscles in concentrated solutions of urinary products dissolve from the periphery gradually and unequally, also goes against the latter. The indented forms which, according to Kölliker's observations, arise during the process of solution Di Vintschgau has fixed in different stages by the addition of iodine. When he reversed the experiment, and treated blood-corpuscles hardened in iodine for twenty-four hours with solution of urinary products, the corpuscles were swollen, some retaining their original, others in a variously altered form, having become more transparent; their edge and nucleus were more defined. In blood-corpuscles, which, by boiling in sugar and water, had coagulated, solution of urinary products produced no other change but to make them more transparent, and the nucleus more distinct. Consequently the urine acted only on the fresh blood-corpuscles, and not only on the superficial layer, but also deeply.—*Acts of the Venetian Institution*, vol. vii. 3rd Series.

On Smooth Muscular Fibre.—Anatomical and physiological observations.—Remak observed the sarcolemma in macerated muscle-fibre-cells as a thin, firm cuticle. He does not admit of a cross streaking of smooth muscular fibres, which, he says, has been thus understood because the substance of the muscular fibres, especially of the stomach, intestines, uterus, and veins, sometimes appeared, when losing the appearance of fibrils, to assume a very regular zigzag condition. On the other hand, he shows transition forms from organic to animal muscles with a partial, mostly one-sided fine furrowing. They are said to appear commonly only in the pointed or angular reticulated fibres connected with each other (of the arteries and of the ciliary muscles of the mammals), and also capable of an active shortening during life. Also in the veins of a diseased human liver, Remak saw this partial cross-streaking, and thinks it might stand in connexion with the *passive* respiratory movements of the liver.—*Wiener Sitzungsberichte*, vol. xxiv. p. 413.

The Organic Muscular System within different folds of the Human Peritoneum.—In the following manner Luschka describes the bearing of the muscular coat of the small and large intestines at the point of connexion of the two. Whilst the layer of circular fibres of the small intestine passes over into the valvuli coli, the layer of longitudinal fibres of the same is continued in numerous bundles, partly passing over into elastic tendon in the whole circumference of its point of inversion upon the coat of the large intestine. The bundles, which are mostly separated from each other by roundish interstices filled with connective tissue, disappear partly between, partly over the circular fibres of the large intestine. From the longitudinal fibres of the latter, and indeed, from those which compose the medial tænia, a number rise to the inner circumference of the small intestine. This occurs where this muscular strip, which here has reached an extraordinary thickness and firmness, is passing as a bridge over the upper end of the very deep enlacement which, on its side, marks on the medial circumference the boundaries of cœcum and colon. A part of its fibres radiate into the bottom of this enlacement; the greatest part of them, however, pass together under and over the root of the worm-like appendage with the lateral and with the posterior tænia coli.—*Arch. für Anat.*, part 2, p. 202.

The Muscular Structure of the Female Urethra.—In his work on the anatomy of the urinary passages, Uffelmann gives the following description: "The streaked animal fibre-layers at the orifice of the urethra are only microscopically perceptible. A few lines higher the layer becomes more evident, and may be separated into two positions, between which the great venous web of the corpus cavernosum urethræ lies: the inner one only belongs to the urethra, and disappears where the urethra and vagina meet; the outer one can be followed to the vagina, separated at an acute angle from the inner one, and unites laterally with the bundles of the levator ani, where they advance to the vagina. They are identical with Luschka's constrictor cunni profundus; nevertheless, Uffelmann gives the height of the whole layer, which he calls sphincter vaginæ et urethræ, larger, to $3\frac{1}{2}$—4‴. Above these, as far as the part of the urethra where the animal circular fibre-layer becomes complete, a tissue, a kind of raphe, fills up the space between the ends of the animal fibres, consisting of transverse connective tissue-patches, elastic fibres, and variously directed organic muscle-fibres. Where the ends of the animal fibres in the hinder urethral coat reach each other, they, nevertheless, do not close into a ring, but cross over each other by sending out rays fan-like from both sides. The thickness of the layer, which increased upwards from the external orifice, again decreases just beneath the sphincter vesicæ.—*Zeitschrift für rationelle Medicin*, 3rd Series, vol. xix. part 2, p. 123.

On the Increase of the Epithelial Cells of the Cornea.—In order to ascertain the law of growth of the stratified tessellated epithelium, Schneider, by means of a solution of potash, 35 per cent., isolated the epithelial cells of the cornea of various animals, and compared the cells of the deeper and more superficial layers. He distinguishes three different forms of cells: first, the cells of the lowest layer—they are the largest of all cylindrical ones—0·015—0·045 mm. in height; the lower ends, with which they are in connexion with the lamina elastica ant., are rounded, rarely pointed. The nuclei have 0·006—0·009 mm. diameter, are round, granular, always lying in the upper half of the cells, and often quite close to its upper end. This description appears to refer to the human cornea; in the rabbit, the author finds the cells of this layer somewhat smaller, 0·015—0·024 mm. in height, the upper end more obtuse, the nucleus rather longish.

Secondly, the cells of the middle series.—They are smaller than the lower ones, globular; in the calf most of them, at the lower end, have two or three processes, which are wanting in the rabbit.

Thirdly, the flattened cells of the surface.—In the lower cells, after having isolated them, the author observed all the phases of the division of the nucleus, beginning as slight indentations; nevertheless, it did not come to any considerable separation of the two nuclei; rarely were there groups of cells in which an origin by division in the longitudinal direction was indicated.

Schneider has also isolated the cells of the epidermis with potash, and never seen free nuclei; the nuclei were always surrounded by a sharply-bounded exterior layer, in which it is certain that a membrane could not positively be shown.—*Würzb. naturw. Ztschr.* vol. iii. part 2, p. 105.

On the peculiar appearances exhibited by Blood Corpuscles under the influence of Solutions of Magenta and Tannin.—The object of Dr. Roberts in this paper is to indicate that the cell-wall of the vertebrate blood-disc probably does not possess the simplicity of structure usually attributed to it. Human blood-discs, in a watery solution of magenta (nitrate of rosanilin), speedily lost their natural opacity and yellow colour; they became perfectly transparent and assumed a faint rose-colour, they also expanded sensibly and lost their biconcave figure. In addition, a dark red speck made its appearance on some portion of their periphery. The pale corpuscles took the colour much more strongly than the red, and their nuclei were displayed with great clearness, dyed of a magnificent carbuncle red. The blood of other mammals was similarly treated, and with the same effect on the red corpuscles. The same treatment of the nucleated blood-discs of the oviparous classes was followed by similar results. Tannin is a mordant used for magenta. It was thought that by its use the dye might be fixed. Its effects were in themselves remarkable: each corpuscle appeared to have thrown out a bright, highly refractive bud or projection on its surface. The projections were usually about a fourth part of the size of the corpuscles on which they were fixed; but they varied considerably. Some were only minute bright specks in the cell-wall; others were half, or even two-thirds as large as the corpuscle itself. Very rarely (in mammalian blood) two such projections were seen, and as rarely a corpuscle was devoid of any. The projections were commonly round or dome-shaped, bordered with a deeply refractive outline. Frequently a minute, apparently vesicular body, could be seen within this outline, and then the projection presented a curiously hooded aspect. The blood of various birds behaved similarly. In fish, the corpuscles had frequently two projections instead of one. In the dace, very rarely a third projection was seen. In the frog, even five were observed; the discs first enlarge, and become rounded, and the central nucleus comes into view. In thirty or forty seconds the pullulation begins, and each corpuscle, with instantaneous rapidity, and without previous sign, throws out its bud. The disc itself suffers not the least disturbance during this act; it preserves its symmetry unchanged, as if it had no concern beyond that of proximity with the sudden apparition on its surface. No visible rupture of the cell-wall took place. Acetic acid sometimes caused the pullulations gradually to subside and disappear altogether. The after effects of the tannin were, that the corpuscles and thin projections became solid, and could be cracked by pressure, or, more slowly, they cracked spontaneously, the cell and the projection breaking up separately. Parietal particles like that brought out by magenta, except that it was not coloured, were on one occasion observed in blood-corpuscles in the urine of a patient with acute Bright's disease.

From the preceding the author infers—1st. The exact identity of the appearances produced in the blood discs of the ovipara with those observed in the mammalian corpuscles, lends strong support to the view that these corpuscles are homologous as wholes; and that the mammalian blood disc is not the homologue of the nucleus of the coloured corpuscles of the ovipara, as was conceived by Mr. Wharton Jones. 2nd. The observations likewise lead to the belief that the envelope of the vertebrate blood discs is a duplicate membrane; in other words, that within the outer covering there exists an interior vesicle, which encloses the coloured contents, and, in the ovipara, the nucleus.

It might be conceived that the cells enlarged by imbibition, until at length the less distensible inner membrane gave way, and permitted an extravasation of a portion of the cell-contents between it and the outer membrane, its own continuity being in the meanwhile instantaneously restored by cohesion of the ruptured borders. In this way a microscopic drop of the cell-contents would be lodged between the outer and inner membrane, and completely severed from the general cell-cavity. The peculiar modification spoken of as the "hooded" appearance might be due to imbibition of fluid between this microscopic drop and the outer envelope.—*Quarterly Journal of Microscopical Science*, July, 1863, pp. 170-9.

On the Nerves of the Cornea, and their Distribution in the Corneal Tissue of Man and Animals.—In this paper, Dr. Ciaccio, of Naples, details his recent researches in the sparrow, eel, frog, mouse, and man. He observes, in the first place, that since Schlemm's discovery of the nerves of the cornea, nearly all have agreed that they terminate in a wide network, composed of non-medullated or pale nerve-fibres. From a knowledge of their mode of termination, we may infer that of other parts having only common sensibility. The author alludes to three great difficulties in this investigation: that the corneal nerves continually change their plane; that they can only be seen by the use of chemical agents which alter or may destroy them; that the processes of the cornea-corpuscles anastomosing resemble the finest nerve-fibres. The corneæ of small birds, of the frog and mouse, &c., are thinnest, and most fit for the investigation. The distribution is very simple in the sparrow. But *very little at a time* of the chemical reagents should be added. The nerves, if very carefully traced, will not be found to terminate in the cornea-corpuscles, and by *tracing them* the former may be distinguished from the processes of the latter. The nerves of the cornea, from the ciliary, pass from the sclerotic into the laminated structure. Their fine trunks are found chiefly in the posterior part of the cornea and the network they go to form in the anterior. The nerves vary very much in size and number. There are thirty trunks entering the cornea of the frog. They branch off variously, generally at an acute angle, not always dichotomously. The nuclei of the nerves *decrease* in number as they reach their termination.

"Besides these nuclei connected with the nerve-fibres, I have seen, especially in the frog, another kind of nuclei which lie on a more superficial plane than the former, and are spindle-shaped, and sometimes so bent on themselves as to exhibit the form of the letter S. They are not arranged in the same linear direction as the nerve-fibres, but incline to them obliquely. I have been able to see these nuclei in the trunks of the nerves and the largest branches; and I hold strongly to the opinion that they are the special organs upon which depend the growth and repair of that clear transparent material in which the nerves at their peripheric distribution are imbedded."

"It appears to me, that upon the progressive increase of nuclei in motor nerves as they approach their termination, and on the remarkable diminution of them in sensitive nerves, which besides are connected at their ultimate distribution with peculiar bodies, we can, with some degree of reason, establish a fundamental distinction between the terminal portions of motor and sensitive nerves. . . . . I firmly believe, that when comparative investigations have been more advanced than they are at present, we shall find something peculiar not only in the termination of the nerves of motion and common sensation, but also in that of every nerve of special sensation." "The nerve-fibres are imbedded in a transparent homogeneous substance, which forms not only a common covering to all fibres composing each separate nerve, but also a special one to each single fibre. The nerves, in their ultimate ramifications, are only separated from the adjacent parts by this material." "The number of the primitive fibres which compose the nerve-trunks of the cornea, is found to vary according to their size. But sometimes we observe in animals of different kinds nerve-trunks of the same size, containing various numbers of primitive fibres. This depends upon the different diameters of the nerve-fibres, as some of them are thicker than others. According to my observations in the mouse, the primitive nerve-fibres are larger than those of the frog and man, and in the sparrow they are much finer." "The nerves, from the entrance into the cornea to their ultimate distribution, do not appear to contain any fibre which could properly be called dark-bordered." "The nerves distributed to the cornea of different animals are not of the same degree of firmness. I have found that in the sparrow, frog, mouse, man, and fishes, the firmness of the nerve-fibres decreases in the order in which I have mentioned the animals." "I am convinced that the nerves distributed to the cornea are not separated from its fibrous elements by any other means but by that special transparent material in which they are imbedded."

"The results of many investigations which I have made upon the cornea of several animals, have led me to conclude that the nerves of the cornea terminate in a network or plexus. I understand by the arrangement of nerves in a *network*, when the different bands of fibres are not so interlaced with each other as to prevent us from recognising their respective origins, and by the term *plexus*, when such an intermingling of the bundles of nerve fibres exists, that we cannot distinguish the point of their derivation."

The author goes on to describe four varieties of ultimate distribution of the nerve-fibres of the cornea—

1. Network by the coalescence of nerve-branches with one another, found in the cornea of the sparrow, the branches frequently anastomose—the most simple and regular arrangement.

2. Network by the intermixture of the nerve-fibres of one branch with those of another, found in the cornea of the frog and fishes; the fibres of the branches are seen to interlace.

3. Plexus with wide meshes, found in the cornea of the mouse.

4. Plexus with narrow meshes, as in the human cornea.

"In connexion with the nerve-branches or bundles which compose both the networks and plexuses before mentioned, are observed several small bodies, triangular or quadrangular, or even of an irregular shape. These small bodies are not all of the same size, and some of them appear of a uniform granular structure, whilst in others I have found nuclei imbedded in the granular matter. These nuclei are prominently coloured by carmina, whereas the granular matter is not, or only very slightly, affected by this substance. From these bodies bundles of fibres are seen to proceed in three, four, or more directions, while some other fibres pass close by them without being absolutely connected with the same." "I believe that the nuclei are the agents which are concerned in the formation and repair of nerve-fibres, which are continually undergoing change during life; while, on the other hand, I hold that the above-named small bodies take an active and important part in the phenomenon of sensation, and are the only organs by means of which the finest branches of sensitive nerves are brought into relation with the tissues in which they ramify."

"Between the nerve-fibres and cornea-corpuscles there is no other relation but that of contiguity, because careful observations show that the nerve-fibres always maintain their individuality, and never lose themselves in another tissue."

"I have distinctly seen some very fine bundles of nerve-fibres distributed to the sclerotic. These bundles, which arise from the nerves destined to supply the cornea, separate from them at different angles before they reach the corneal margin, and after passing backwards, ramify in the fibrous tissue of the sclerotic, the bundles anastomosing with each other. I have a specimen from the mouse that evidently proves this fact."—*Quarterly Journal of Microscopical Science*, July, 1863, pp. 77–93.

Part II.—Pathological Micrology.

Clinical Remarks and Observations on the Diseases of the Skin said to be Parasitic.—Dr. Maurice Chausit, in the series of papers he has published on this subject, begins by saying that—"The number of cutaneous affections called parasitic, is continually increasing, and is only equalled by the facility with which new cryptogamic spores are discovered." The author, with M. Cazenave, "believes that the parasites observed, supposing they really have been, may exist accidentally, and constitute an abnormal phenomenon, but that they should not be considered as an essential cause of any one of the forms of diseases of the skin.

"The diseases of the skin which successive works of microscopists and physicians have made due to parasitism, in the actual state of the question, are—favus, herpes tonsurans, herpes circinatus, herpes squamosus, herpes iris, mentagra, vitiligo, pityriasis versicolor, the ephelides of pregnant women, acne punctata, acne molluscum.

"The vegetable parasites, which, according to the partisans of this system, play the part of inevitable pathogenic cause, belong to several species of the tribes of the Torulaceæ and Oidieæ, and are called by the names of Achorion Schoenleinii, Trichyophton tonsurans, Microsporon furfur, and Microsporon Audouini, to which they agree to add the Puccinia favi, discovered by Dr. Arndtsen, of Christiania, and of whose recent works the French parasitophilists make no mention at all.

"We must not forget to add to this nomenclature of vegetable parasites the new cryptogamic spores discovered by Mr. Hardy in acne punctata and in acne varioliforme, which we propose to call after the name of their inventor, the Microsporon Hardii, until this dermatologist shall make known the botanical tribe to which they belong.

"The diseases called parasitic are divisible into two classes—the first, A, comprises only one disease, favus, which has, moreover, served as the starting-point of the vegetable theory of certain affections of the skin, and which, to a certain point, explains how the presence of parasites may be seriously discussed and admitted; in the second, B, are arranged all the other diseases in which the existence of a cryptogam can only be made out with the eyes of a strong belief, or the tendency to view as vegetable spores all the rounded bodies which do not present the microscopic characteristics of cerumen, of the globules of pus or of fat.

"The appearance of the favus matter, seen in the field of the microscope, presents neither altogether, nor in its parts, that simplicity of organization which would leave no doubt, and which one has a right to expect before admitting that it is of a vegetable nature.

"The characters of the objects declared to be of a vegetable nature by microscopic inspection, become less and less visible as one quits the study of the favus matter; one might even say, that in some of the diseases one meets with no vestige whatever of cryptogamic germination."—*L'Union Médicale*, Aug. 22nd, 1863, p. 356 et seq.

Fluid of Spina Bifida Tumour.—Dr. Salisbury reports the examination of this fluid: "Three ounces of it were evaporated to dryness carefully over a water-bath. The fluid was slightly alkaline; cholesterine was demonstrated to be present in considerable quantity. It was thin, watery, and milky, with numerous white flocks floating through it; many of them were composed of nerve-tubuli. Occasionally was found a tuberculated cell filled with fluid, and met with in most animal and vegetable tissues, and resembling somewhat some varieties of pollen-grains. There were also found numerous spherical fungoid spores, aggregated in flocks, and among them was noticed a single elongated vegetating spore. Scattered through the fluid, quite abundantly, were flattish oblate spheroidal, highly refractive cells of various sizes, many of the larger of which had the appearance of being ruptured on the side. Occasionally, one was met with containing small, reddish-brown spherical cells. In the flocks mentioned above were many remains of disintegrating cells. There were also numerous minute linear bodies, moniliform in structure, all through the liquid, and which, in many instances, were in active motion, making the whole mass of the liquid alive with them, the fluid being still fresh and sweet. These are the so-called 'vibriones;' they have, however, no connexion whatever with the vibrii, either in structure or mode of development. From the numerous examinations made connected with diseased tissues, and fermenting and decaying bodies, I am strongly impressed with the belief that many of them are organisms allied to sperm-cells, being spermatozoid or antherozoid in character. There is evidence for believing some of them to be embryonic states of filamentous organisms belonging or closely allied to the confervaceæ. These latter are found in their mature state abundantly in the spleen, kidneys, and liver of animals, and of the human subject, wound together in various ways, and firmly attached to the glandular vessels. Occasionally was met a cell containing the sperm-cells (so-called vibriones), just described. Torula cells were frequently met with either single or aggregated in masses. There were also several asci and numerous highly refractive sporoid bodies noticed, besides a peculiar barbed filament that I have frequently met with connected with the glands and circulating fluids of animals."—*American Journal of the Medical Sciences*, April, 1863, p. 293.

Villous Cancer of the Urinary Bladder.—In this case the patient, three or four years before his death, had frequent micturition and pain afterwards, then hæmaturia. The bladder was explored, and much hæmorrhage followed. Pain was relieved by the use of the catheter. The urine became less bloody, but more fetid, and it contained more mucus; it frequently dribbled away, and there was much pain. The examination after death was made by Dr. Joseph Bell. The following is his description:—"When the bladder was cut open from above, after its complete removal, its whole cavity seemed to be filled with a loose, whitish, soft substance, streaked with tinges of pink and yellow. On letting a very gentle stream of water flow into the bladder, a quantity of this substance was washed out; but a far greater quantity still remained adherent both to the base and the sides, nearly up to the fundus. The bladder was very strongly contracted, and its muscular coat much thickened. It contained no urine. Looked at by the naked eye, this structure, which lined the walls, seemed to consist of innumerable slender filaments, about one-sixth to one-fourth of an inch in length, matted together, and moving in masses (like the pile of a very loose velvet or flock) when water was poured on them. The pale colour was apparently powdered over by a whitish frosting.

"Under the microscope, the soft structure was found to consist of immense numbers of delicate filiform villi, very long in proportion to their breadth; some terminated in club-shaped extremities; others, and these the larger ones, being prolonged into flask-shaped projections all along their sides as well as at the edge; others being markedly dendritic, with numerous branches from a single stem. The whole presented a densely granular appearance, as if from the presence of numberless very closely packed small cells, firmly attached to each other. *The frosted white appearance* scattered over the villi was at once explained by the microscope showing abundant and well-formed crystals (triangular prisms) of the triple phosphate, which lay entangled in the meshes of the villi. These villi appeared to grow, not from any special tumour or pedunculated growth attached to the bladder-wall, but from the very mucous membrane itself, or rather, perhaps, from a cellular stroma in which no separation could be traced, but which was attached to the mucous membrane.

"These villi were deserving of special notice as to three points—their relation to the mucous membrane, their vascular supply, and their contents. On making a section vertically through villi, mucous membrane, and muscular coat, the villi were seen to rise decidedly from the submucous areolar tissue; on adding acetic acid, the fibres of it were traceable into the roots of the villi, and both roots and fibres were infiltrated with numerous and closely-packed nucleated cells. It, to a certain extent, removed the granular haze, and enabled me to see the vessels running into the villi. They were best seen in the more delicate villi, which contained less opaque contents.

"On examining a villus before the addition of any reagent, with the exception of a few of the younger and more delicate flask-shaped projections, they all seemed closely packed with a densely granular-looking structure, which had always a slight, sometimes a very distinct, yellow colour. On adding ether, the yellow colour and granular appearance gradually disappeared; and when added in sufficient quantity, the loop of vessel (a colossal capillary), which was before only dimly visible, was quite clear, and seen to be coated with a dense, closely-packed layer of nucleated cells, while the rest of the contents of the villus were cleared up, the fat being dissolved and the yellow colour removed. The elongated nuclei in the capillary walls were distinctly visible.

"It is not to be wondered at that, in the case narrated, the hæmorrhage was so persistent and excessive, when we remember the very large amount of vesical surface affected, and the consequent amount of mechanical friction the villi would sustain from the very powerful, thick-walled, and firmly-contracting bladder."—*Edinburgh Medical Journal*, May, 1863, pp. 1036–40.

HALF-YEARLY REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIENE.

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I. Toxicology.

Bichromate of Potassa as a Poison.—One of the most important papers on toxicology published within the last few years is by MM. A. Chevalier and Dr. Bécourt, on bichromate of potassa. The report of these investigators, though not official, is of great public, not less than of scientific interest. It conveys to us a series of new facts in regard to a particular poisonous salt, and it proves that the effects of this poison tell on certain branches of industry, producing wholesale mischiefs.

By an accident, one of the reporters met with a man who was engaged in a manufactory of chromate potassa, and who was suffering from a peculiar ulceration of the face: from this case they obtained an idea that the manufacture was attended with serious mischiefs, but they failed to obtain from the man any very satisfactory evidence.

"At last, from the director of a manufactory at Graville, they obtained a series of facts from which they learned that the workers were subjected to disease. In transforming the neutral chromate of potassa by means of acid into bichromate, the vapour arising carries with it an infinity of pulverized molecules of the product, which spread through the workshop. The cloud of particles is easily visible in a ray of sunlight. The molecules, inspired in abundance, give to the palate a bitter and very disagreeable taste; but as profuse salivation is the result, the chromate is thrown off in the saliva, and has not time to inflict any permanent injury. If, however, the respiration be made by the nose, the molecules are dissolved in the layer of secretion which lies on the membrane of the septum of the nose, creating a violent pricking, suffusion of tears, and irresistible sneezing. In time the membrane begins to be thrown off, and portions of it are carried into the handkerchief used in blowing the nose; this process goes on, when once started, so rapidly that after a period of six or eight days the septum becomes thin, permeated with openings, and is ultimately detached altogether. At this point, all the symptoms that have been described cease, and the workman scarcely notices the loss of the nasal partition.

The reporters state that this process of ulceration of the septum of the nose occurs in every workman, except in those who take snuff. In these, owing to the layer of powdered tobacco which covers the membrane, and the frequent use of the handkerchief, the evil is removed, or rather prevented.

On the skin in its normal state, the epidermis being intact, the bichromate exerts no baneful influence: the hand may in fact be plunged into a concentrated and hot solution of the salt, without fear; the hand may also remain covered with the salt for an entire day, without any observed effect; but if the skin is torn or abraded, however triflingly—by the prick of a pin, for example—a sharp pain is felt on the exposure; and if the salt be left in contact with the wound, the caustic character of the salt is brought out intensely, the cutaneous tissue is decomposed, and violent inflammation is established. These symptoms are accompanied with intense pain, especially in winter, when the cold is severe; the action of the salt does not cease until the cauterization has penetrated to bone.

When a workman is clean and careful, he prevents these accidents; but if he is careless, and allows the bichromate, either in powder or solution, to touch abraded parts, or sores, or wounded surfaces, he must immediately use remedial measures, or suffer a severe penalty. In some cases, where the workmen are too lightly clad, they are attacked with violent itchings, followed by suppuration and ulceration of the moist surface of the penis, around the glans. This condition may progress until a disorder not unlike syphilitic ulceration may be presented.

The effects of the bichromate are shown on inferior animals as well as man. Horses employed in the manufactory, and which walk over the salt, are attacked in the feet; the hoof falls off, the inflammation extends to the upper part of the leg, causing the hair to fall off, even to complete denudation. From a series of facts related to the reporters by M. Clouet, a manufacturer at Havre, some important particulars are supplied. A horse employed in carrying the bichromate was attacked in one of his hind-feet. The wound became so painful as to necessitate absolute rest; the master of the horse not understanding the treatment, the suppuration went on, extending through the limbs, the skin of two legs fell off, and an enormous suppurated surface invaded the almost entire half of the body of the animal. Death occurred a month after the commencement of the malady. The writer remarks that it is as though, when the decomposition had commenced, it went on indefinitely; there is a veritable metamorphosis of the cutaneous tissues and of the flesh altogether, analogous to fermentative action.

Dogs and cats are also subject to similar accidents from the bichromate. These animals, from walking amongst the refuse of the manufactory, are frequently affected; the skin of their feet becomes laid bare, and suppuration follows. In one instance, a rat, killed in a manufactory, was found to have all his feet suppurating, and partially destroyed, as if gnawed.

M. Clouet—to whom, as we have said, the report is much indebted—supplied also to the authors certain facts relating to the internal exhibition of the bichromate of potassa. He says that in a small dose, a few centigrammes, it acts as a purgative; if in larger doses, say of one gramme, it acts as a poison. In large doses, it produces colic and purging, but no vomiting. In one manufactory, certain of the workmen placed some bichromate of potassa in a barrel of cider, as a joke. The cider was rendered of a dark colour; but, notwithstanding, other of the workmen drank the cider, and were all affected with severe colic and diarrhœa.

In a second account sent to the reporters by M. Clouet, the facts of his previous statements were confirmed, and certain further details were offered, which are of interest. Thus it was shown that both sexes were equally influenced, and at all ages; and that the affection of the septum of the nostril usually made its appearance within a week after exposure. The disease of the septum was very easily brought about in those workmen who, having stained their fingers with the chromate, put them into the nostril. M. Clouet also observed in every case, that in those men in whom the septum was entirely removed, nasal catarrh was unknown.

In respect to treatment, various observations have been carried out. As a preventive of the nasal disease, the use of snuff seems very effectual. In some cases the workmen apply a wet sponge before the nostrils when they are exposed to the vapour; and the plan seems to be advantageous. When the skin is abraded, and the chromate has produced ulceration, it is the best treatment to wash thoroughly with a feeble alkaline water; then, if inflammatory action follows, to poultice, and afterwards to apply freely subacetate of lead in solution.—*Annales d'Hygiene Publique*, Juillet, 1863, No. 39.

Poisoning by Cyanide of Potassium: Dangers to Photographers.—Dr. Davanne directs attention to the dangers to which those who are engaged in the art of photography are exposed. They use constantly, he says, two poisons of the most active kind—namely, cyanide of potassium and bichloride of mercury. Their hands are constantly in contact with strong solutions of these poisons; and often, in spite of warnings repeated over and over again, they run absurd risks. The author gives an account of a case in which a gentleman, who had stained his hand with nitrate of silver, endeavoured to remove the stain by rubbing over it cyanide of potassium freely. In the act, he slid under the nail of one of his fingers a small portion of the cyanide salt. At first he did not notice what had happened, but in a little time he felt a sharp pain in the part, followed, after a few minutes, by an intense vertigo, so that all objects appeared to be moving around him. To relieve himself promptly, he conceived the unfortunate idea of employing vinegar; the cyanide was quickly decomposed, and hydrocyanic acid was produced absolutely. The vertigo now increased, accompanied by shiverings, pallor of the face, loss of sight, and great exhaustion. The power of speech was lost, but the intelligence was preserved. The extremities were cold, and as the sight returned there was double vision. The symptoms did not pass away for ten hours.

The treatment adopted consisted of cold and friction on the spinal column, inhalation of ammonia vapour, and the administration of strong infusion of black coffee.—*Canstatt's Jahresb.*, tome vii. p. 64. 1862.

[We can confirm, to a considerable extent, many of the facts related by Dr. Davanne. A photographer, who was under our care a few months since, after using a solution of cyanide of potassium at a time when his hands were "chapped," was seized with vertigo, nausea, shivering, great prostration, double vision, and muscular tremor. These symptoms were repeated several times before their cause was discovered. On consulting us, we traced the symptoms distinctly to the local application of the solution of the cyanide. On removal of the cause, the patient lost at once all the acute and immediately dangerous indications; but he was left for some weeks very feeble, anæmic, and dyspeptic, and with the blood abnormally fluid.—B. W. R.]

Effects of Emerald Green on Paper-stainers.—The commissioners appointed to inquire into the health of children in manufactories have published some valuable remarks on the effects of arsenical preparations on the paper-stainers. The commissioners state that the colouring matter consists sometimes of emerald green, in greater or less proportions; and when it does so, it becomes, under certain circumstances, a source of danger. The evidence of Dr. Letheby on this subject is particularly deserving of notice, as showing not only the danger to the workpeople, but to persons whose rooms are hung with these arsenical papers, and to those who wear artificial flowers similarly coloured. Dr. Letheby relates a fatal case of a child who was thus poisoned from playing in a room in a gentleman's house which was covered with this green paper. He also states, as showing the amount of arsenical matter, that he has found about a grain of pigment for every square inch of the green leaves of these flowers; adding, "I have seen a wreath with enough arsenic in it to poison twenty people."

The commissioners add—The emerald green is dangerous in this trade:

1st. If it is badly manufactured. Mr. Cook states, "Our emerald green is peculiar. It comes from one particular place in London. It is much finer and softer to the touch—less granular, that is—than the ordinary Scheele's green, of which this is a specimen. It does occasionally vary in quality; but as a general rule, if ever we have to get some from anywhere else, our men know by the strong smell directly they open the cask that it 'wont work,' as they say—that is, that it will be loose and fly;" and Mr. Cook adds that brushing emerald greens by a machine, "if it were at all loose, must be very bad."

Mr. Smith, of Messrs. Heywood's, states that "the arsenic green is better than it used to be;" and other witnesses also are of the same opinion, that if it is well manufactured "no bad effects arise from it." It is the cheapest kind that is the most injurious. "The commoner the colour—the cheaper, that is —the worse for use. It is not properly prepared; the arsenic is not killed in it."

2nd. If it is imperfectly mixed with the size. It appears that this is more likely to be the case with machine-made paper than in block-printing, inasmuch as the colour for block-printing is more "set with size," being worked "almost in a jelly;" whereas "the cylinder in the machine must have a more or less liquid colour, or it would not revolve; and so there cannot be enough size to bind in the one as in the other."

3rd. If it is worked at continuously, especially with machine-made paper; the friction in rolling it up when dry, or in brushing it, causes the dust to come off.

4th. If cleanliness be not observed. J. Nail says: "The emerald green is worse, but cleanly persons are not affected by it. The boys, many of them, will not wash; but eat their meals with dirty hands, covered with paint and mess. I have seen them eat their dinner with hands smothered in lead."

That these sources of danger, being well known in the trade, are watched, and, to a considerable extent, guarded against, is shown, not only from the above evidence, but from the facts related by several witnesses connected with the large works in the North; where the printing is chiefly by machinery, and where, consequently, the greatest danger arises from the emerald green. J. Boden, at Messrs. Heywoods' works, states that he "has never known any permanent injury from working the emerald green."

The case of a boy at Messrs. Potter's, who is said to have died from the effects of emerald green, G. Aspden, is exceptional; and is, indeed, attributed by two of the witnesses, partly to detaching the chalk dust, partly to "the long hours and close confinement."—*Report of the Children's Employment Commission, Government Paper*, 1863.

A New Test for Cantharidine.—Mr. C. R. C. Tichborne supplies a new test for cantharidine. He says, truly, that the chemical tests for this substance are most unsatisfactory, and are to vague too admit of application. He therefore proposes to look to its vesicating properties as the best indication of its presence. His plan, which, he states, leads to the detection of an extraordinarily minute quantity of the poison, is described as follows:—Chloroform is the best solvent of cantharidine that we have, and he recommends its employment for the extraction. In his experiments, a tincture was used that represented three grains of cantharides. This was added to half a pint of wine or porter, and then, to this solution, which represented a suspected solution, was added one ounce of chloroform. The whole was repeatedly shaken during the day, and left to subside, until next morning. The chloroform was then carefully separated and passed through bibulous paper. Afterwards, a little of the chloroformic solution was allowed to evaporate spontaneously to dryness in a watchglass. A small pellet of lint, about half the size of a pea, was moistened with a drop of olive oil, and with this little pellet the whole of the film of extractive matter was mopped off the watchglass. The lint was then placed on the arm of the experimentalist and covered with a piece of goldbeater's skin. When taken off in three or four hours, "considerable rubefaction had taken place, and after wiping with chloroform a large vesicle was formed. As small a quantity as one grain of flies, equal to $\frac{4}{100}$ths of a grain of cantharidine, was detected in solution by this means.—*Dublin Medical Press*, and *Pharmaceutical Journal*, April 1st, 1863.

Lead as an Adulteration of Silk.—Dr. Eulenberg directs attention to the fact that lead is not uncommonly found, as an adulteration, in the threads of silk. It is added for the purpose of increasing the weight, and it is present in notable quantities, especially in the silks from Germany, Belgium, France, and Switzerland. He adds, that the adulteration is attended with danger, because the silk impregnated with the poison is often drawn through the mouth by workers with silken threads, such as tailors and dressmakers. In this way he believes accidents from the absorption of lead very often originate, the symptoms being unappreciated. A specimen of black silk, of German manufacture, which fell into the hands of Dr. Eulenberg, yielded, on analysis, not less than 17·74 per cent. of salt of lead.—*Pappenheim's Beitr.*, Heft 1. 1863.

Poisoning by Stramonium.—Cases of poisoning by stramonium are extremely rare, so rare that few reliable data are before us relative to the action of the datura in poisonous doses. We may therefore give with some care the details of three examples, where death followed from the use of this poison. The narrative is supplied by Dr. John Shortt, Zillah surgeon, Chingleput.

Dr. Shortt states, that the cases all occurred within six months; they were brought to the Civil Dispensary—in one instance for medical treatment, in two for confirmation of death. There are two varieties of datura growing in the district, the purple-flowered variety, which is very rare, *Datura fatuosa*, and the white-flowered *Datura alba*; the latter is very common, it grows, in fact, "on the dunghill of every village, in waste places, and by the road side." It makes its appearance soon after the monsoons and thrives with great vigour, pushing forth its large conspicuous white tubular corolla, and giving beauty to localities otherwise defaced by refuse and neglect, but masking in its showy form a deadly poison.

The first case recorded is that of a Brahmin, aged forty-one years. He left his house well at 6 A.M. in the morning of August 12th, 1862, and returned home at 8·30 A.M., when he grew giddy and fell to the ground. On being questioned, he stated that he had eaten "Oomatum elai," or stramonium leaves. Soon after he grew violently delirious, and died comatose at about

9·30 A.M. The body was brought to the Civil Dispensary on the following day; it was conveyed thirty miles in a common cart; it was that of a corpulent man, and was laid out on the back. A white foam escaped by the mouth and nostrils, the eyes were glassy, *the pupils were dilated*, the face was bloated, the surface of the body was free from marks; the bones were entire. There were no evidences of decomposition. On post-mortem examination, the *lungs* were found congested from gravitation; the *heart* was found flaccid and empty; the *stomach* contained nearly two ounces of a dark-green pultaceous mass, which, on washing, proved to be the *débris* of undigested leaves; *the mucous membrane of the stomach was greatly congested throughout its extent, more particularly along the great curvature;* the *duodenum* was empty; the *jejunum* contained a quantity of a pulpy mass of a green colour, and peculiar musty odour; the mucous membrane of the intestines was congested throughout its extent, but in a lesser degree to that of the stomach; all the other viscera were healthy; the brain was not examined.

The second case occurred on the 25th of October, 1862, in the person of a girl, aged fourteen years. She was of the Pandarum caste; she was brought to the Civil Dispensary for treatment at 7·30 P.M.; she was reported to have eaten "Oomatum kai," with the intention of self-destruction. On admission, she had a wild look and was stupid and unconscious of her state; when roused, she gave a vacant foolish look around; the pupils were widely dilated; a glairy saliva dribbled from the mouth; the lower lip was much swollen and bruised; the skin was warm and dry; the pulse was sixty-five, slow and full. The patient was actively treated with emetics and recovered. When she had come quite to herself on the following day, she said that, owing to her mother having beaten her on the morning of October 25th, at about 8·30 A.M., she plucked an "Oomatum kai pingee," a young fruit of the stramonium, crushed it on a stone, and collected the young seeds, of which she ate about one-third. She returned to the house about 9 A.M., staggered, and fell. She answered the question as to what she had done, but did not remember anything more from that time. The mother of the girl reported that when she heard the cause of the illness, she gave a native emetic in warm water (the "Eloopai poonak"), which caused vomiting, and afterwards the leaves of the "indigo;" but as the girl continued dull and stupid, she brought her to the dispensary in the evening.

The third case also occurred in a girl; she was of the Moodelliar caste, and of the age of eighteen. On the 14th of December, 1862, she was observed, early in the morning, to be giddy and to stagger. On being questioned, she admitted she had eaten "Oomatum kai," which she had procured from a plant. Another girl also stated that she had seen the patient early that morning, grinding down a vegetable substance, which she said was the green fruit of the tamarind.

When the symptoms were discovered, the mother of the patient gave as an emetic some "Eloopai poonak" dissolved in coujee water, and afterwards the juice of the red lotus leaves; both failed to produce vomiting. In an hour the girl became delirious, commenced tossing about, picking her clothes, and talking incoherently; the juice of the lotus was repeated, but without effect, and at about 9 A.M. convulsions set in; in one of the paroxysms the patient died. The mother thought the girl had eaten the poison to relieve herself of colicky pains to which she had long been subject. The body was brought thirty miles in a cart to the Civil Dispensary, where it arrived at 5 P.M. on the 14th of December. Dr. Shortt found the body to be that of a well-developed young woman; she showed no marks of injury; there were evidences of incipient decomposition, in the detaching of the cuticle of the hands; the abdomen was slightly distended; the face bloated; *the pupils were dilated.* A frothy fluid oozed from the mouth and nose. On opening the *head*, the scalp and membranes of the *brain* were found greatly injected; the brain was firm; on cutting into it the puncta vasculosa were large and prominent; the ventricles were empty. There was no fluid at the base of the skull, and the brain exhibited a dry firm appearance, and a peculiar greyish-white colour. The *lungs* were healthy, but there was much hypostatic congestion. The *heart* was small, empty, and flaccid. The *stomach* was nearly empty, and the mucous coat was much congested, especially along the great curvature; several pieces of the spring capsule of the stramonium were found imbedded in the mucous membrane. The *duodenum* contained twenty-five stramonium seeds, as also a good number of the seeds of the common red chili, with portions of its skin mixed up into a greenish pultaceous mass, having a strong musty odour. The *jejunum* contained a nest of thirty-five *lumbrici;* and the *ileum* a green softish mass, with a few seeds of the stramonium.

In a note to his very valuable paper, Dr. Shortt states that the poisonous properties of stramonium are well known in India, and that the plant has been used for the purposes of "hocussing." He gives a singular instance of its application this way.

The paper is unusually valuable, as supplying on reliable data the toxicological formula of stramonium. In brief, the symptoms are, staggering gait, entire loss of consciousness, restlessness, dilatation of the pupils, convulsions, and death. The post-mortem signs are congestion of the brain, hypostatic congestion of the lungs, an empty, flaccid condition of the heart, extreme congestion of the stomach, especially along the greater curvature, and slighter congestion in the course of the alimentary canal.

It is remarkable that the two last-named patients referred to in the report, took the stramonium with suicidal intent, from having learned its action through the narrative of the events of the first case.—*Madras Quarterly Journal of Medical Science*, April, 1863.

Nitrobenzole and Aniline as Poisons.—Dr. Letheby contributes an important essay on nitrobenzole. He remarks that it is on record that Thrasyas, the father of botany, was so skilled in the preparation of drugs, that he knew how to compound a poison which would remain for days in the living body without manifesting its action, and would at last kill by a lingering illness. Theophrastus speaks of this poison, and says its force could be so modified as to occasion death in two, three, or six months, or even at the end of a year or two years. The writings of Plutarch, Tacitus, Quintilian, and Livy are full of instances of what seem to be this kind of slow and occult poisoning. In fact, until recently there has been a common belief among the unlearned that a skilful poisoner could so apportion the dose and combinations of certain subtle agents that he could destroy the life of his victim with certainty, and at the same time measure his allotted moments with the nicest precision, and defy the utmost skill of the physician and the chemist. Even so late as the sixteenth century, this belief was shared by the learned of the medical profession.

The belief so long held is, according to Dr. Letheby, to an extent true. In every manufactory where nitrobenzole and aniline are prepared on a large scale, the peculiar narcotic effects of these poisons are often observed. The vapours escaping into the atmosphere are breathed by the workmen, and cause distressing headache and a heavy, sleepy sensation. For the most part, these effects are not serious, but are quickly relieved by fresh air and a mild stimulant, as a glass of brandy-and-water. Now and then, however, the workmen, from carelessness in their habits, expose themselves to the action of comparatively large quantities of these poisons, and then the effects are most dangerous. Two fatal cases of poisoning by nitrobenzole have been referred to Dr. Letheby by the coroner for investigation during the last two years, and in both instances they were the results of careless manipulation. In one case a man, forty-three years of age, spilt a quantity of the liquid over the front of his clothes, and he went about for several hours in an atmosphere saturated with the poison. In the other a boy, aged seventeen years, received a little of the liquid into his mouth while sucking at a siphon. The effects were nearly the same in both cases, notwithstanding that in one the poison was inhaled and in the other it was swallowed. For some time there was no feeling of discomfort beyond that of drowsiness; gradually, however, the face became flushed, the expression stupid, and the gait unsteady—the sufferers had the appearance of persons who had been drinking. Little by little this stupor increased, until it passed into profound coma, and in this condition they died. The progress of each case was much the same as that of slow intoxication, excepting that the mind was perfectly clear until the coming on of the fatal coma. This was sudden, like a fit of apoplexy; and from that moment there was no return of consciousness or of bodily power—the sufferer lay as if in a deep sleep, and died without a struggle. The duration of each case was nearly the same; about four hours elapsed from the time of taking or inhaling the poison to the setting in of the coma, and the coma lasted for about five hours.

Previous to death there were no appearances of convulsions, but rather of narcotism and apoplexy. The face was flushed; the lips were livid; the superficial vessels of the body, especially about the throat and arms, were gorged with blood; the dependent parts were turgid; the blood was everywhere black and fluid; the lungs were somewhat congested; the cavities of the heart were full; the liver was of a purple colour, and the gall-bladder distended with bile; the brain and its membranes were turgid, and in the case of the man there was much bloody serosity in the ventricles. Analysis discovered the existence of nitrobenzole in the brain and stomach, and also of aniline.

These effects were so remarkable, that Dr. Letheby determined to examine them still further by experiments on domestic animals. Dogs and cats were submitted to the action of from thirty to sixty drops of nitrobenzole which had been well washed with dilute sulphuric acid and water, to free it from every trace of aniline. The poison was generally administered by pouring it into the mouths of the animals, but sometimes it was given by means of an œsophagus-tube. When the nitrobenzole had come into contact with the mouth, it always caused discomfort, as if from unpleasant taste, and there was profuse salivation. Its local action on the stomach, however, was never very great, for there was rarely any vomiting until the setting in of nervous symptoms, and this seemed to be due to sympathy rather than to any local irritation of the stomach. Two classes of effects were clearly observed; there was either the rapid coma which characterized the operation of the poison on the human subject, or there was a slow setting in of paralysis and coma, after a long period of inaction.

When the effects were speedily fatal, the animal was soon seized with giddiness and an inability to walk. The weakness of the limbs first appeared in the hind extremities, and was manifested by a difficulty in standing; but very soon it extended to the fore legs, and then to the head and neck. There was complete loss of voluntary power. The animal lay upon its side, with its head drawn a little back, and with its limbs in constant motion, as if in the act of walking or running. The muscles of the back were occasionally fixed in spasm, and every now and then the animal would have a sort of epileptic fit. It would look distressed, would howl as if in pain, and would struggle violently. After this it would seem exhausted, and would lie powerless. The pupils were widely dilated, the action of the heart was tumultuous and irregular, and the breathing was somewhat difficult. For some time, however, the animal retained its consciousness, for it would look up and wag its tail when spoken to; but suddenly, and often at the close of a fit, it would become comatose—the eye would remain open, but the conjunctiva would be insensible to touch, and the movements of the limbs would nearly cease; the breathing would be slow and somewhat stertorous, and the animal would appear as if it were in a deep sleep. This condition would last until it died, the time of death varying from twenty-five minutes to twelve hours after the administration of the poison.

When the action of the poison was slower, there was often no visible effect for hours or days. At first there was always a little discomfort from the taste of the poison, but this soon subsided, and then for a day or more the animal appeared to be in perfect health. It would go about as usual, would be quite lively in its movements, would eat its food heartily, and in fact would seem to be in no way affected by the poison. Suddenly, however, it would look distressed, it would have an attack of vomiting, and it would tumble over in an epileptic fit. When this had subsided, it was generally found that the animal was weak, or even quite paralysed in its hind extremities; and after two or three of such attacks, the loss of voluntary power would extend to the fore limbs. The animal would lie upon its side perfectly helpless, and then the progress of the case was much the same as that already described, except that it was considerably slower. Consciousness, for example, would be retained for days after the animal was paralysed, and, although it was quite unable to stand, it would take food and drink when they were put into its mouth. The condition in which it lay was most distressing; the look was anxious and full of fear; the limbs were in constant motion; and every now and then there would be a violent struggle, as if the animal was in a fit, or was making fruitless efforts to rise. This would last for days, and then there would be either a gradual restoration of voluntary power with complete recovery, or death from exhaustion. The time that elapsed from the administration of the poison to the coming on of the first symptoms—namely, the epileptic fit, varied from nineteen hours to seventy-two, in most cases it was about two days, and the time of death four days.

In commenting on these facts, Dr. Letheby dwells on their immense importance to the medical jurist and physiologist. They indicate, he thinks, a reducing power in the animal body by the conversion of nitrobenzole into aniline. He has endeavoured to ascertain whether this is due to a living or a dead process; and he finds that while contact with dead matter does convert nitrobenzole into aniline, there is a great similarity of action between the physiological effects of nitrobenzole and of aniline. The post-mortem appearances are also much the same. He adds, that not only is there a probable conversion of nitrobenzole into aniline in the living body, by a process of reduction, but that there is also undoubtedly a change of an opposite character going on upon the surface of the body, whereby the salts of aniline are oxidized, and converted into mauve or magenta purple. The author gives a case of this character, in which a boy, aged sixteen, was brought into the London Hospital in a semi-comatose condition, owing to his having breathed an atmosphere charged with the alkaline vapour while scrubbing out the inside of an aniline vat. He was suddenly seized with giddiness and insensibility. After passing through stages like those of intoxication he rallied, but it was observed that his face had a purple hue, and that the lips and lining membrane of his mouth and his nails had the same purple tint. The next day even he had the hue of a person suffering from Asiatic cholera.—*Proceedings of the Royal Society*, part 3, 1863.

On the Separation of Arsenic from Copper.—Very important mistakes have been made in the use of Reinsch's test for the detection of arsenic. Dr. Odling therefore describes a method of detecting arsenic in the copper used in experiment, so that, in fact, the copper may be tested. He says: As even in the most satisfactory performance of Reinsch's test for arsenic—the deservedly favourite test of English toxicologists—there is always some, although but an extremely small quantity of the copper wire, foil, or gauze dissolved, and, as commercial copper is rarely quite free from arsenic, and sometimes contains a very notable proportion thereof, it is important that the copper to be used in medico-legal researches as a precipitant for arsenic should be specially tested as to its purity. But, as in the ordinary mode of experimenting by Reinsch's process, the amount of metal dissolved is scarcely appreciable, it is quite unnecessary to submit any considerable quantity of it to examination. If a solution of four or five grains of the copper does not yield any evidence of arsenic, it is quite pure enough for the purpose, even though a little arsenic should be recognised in the solution of a larger quantity.

As a means of detecting traces of arsenic in copper, the author believes that the following process is superior to any hitherto proposed in conjoint delicacy and rapidity of operation:

A few grains of the copper cut into fine pieces are placed in a small tube-retort, with an excess of hydrochloric acid, and so much ferric hydrate or chloride as contains a quantity of iron about double the weight of the copper to be acted upon. The mixture is then distilled to dryness, some care being taken at the last to prevent spurting.

The whole of the copper is in this way quickly dissolved, and any arsenic originally contained in it carried over in the form of chloride of arsenic, which may be condensed in a little water with the excess of aqueous hydrochloric acid. The resulting distillate is then tested for the presence of arsenic by treating it with sulphuretted hydrogen, or, preferably, by boiling in it a fresh piece of clean copper-foil or gauze. In some cases, the residue left in the retort may be treated with a little fresh hydrochloric acid, again distilled to dryness, and the distillate collected and tested along with that first produced.

Most oxygenants other than ferric chloride are objectionable, as by their reaction with hydrochloric acid they give rise to free chlorine, which passes over with the distillate, and renders it unfit for being immediately tested either with sulphuretted hydrogen or fresh copper. Cupric oxide, or chloride, on the other hand, is scarcely active enough for the purpose, while the dissolution of copper in hydrochloric acid brought about by mere exposure to the air is extremely tedious.

It may be as well to add that ferric chloride is rendered quite free from arsenic by evaporating it once or twice to dryness with excess of hydrochloric acid.—*Journal of the Chemical Society of London and Chemical News*, July 18th, 1863.

II. Summary and Miscellany.

The following summary includes brief references to important works and papers which, from want of space, cannot be given at length in this report.

Orders of the Madras Government on Reports on Vaccination for 1860–1861. —The order shows that there has been a gradual diminution in the number of persons vaccinated in Madras from 1855 to 1861. It states, also, the resolution of the Government to direct that no person shall be entertained in any public situation, or admitted as a candidate for the Uncovenanted Service Examination, unless he can produce a certificate to show that he has been vaccinated or has had small-pox.—*Madras Quarterly Journal of Medical Science*, April, 1863.

An Inquiry into the Action and Uses of Atropia. By Alexander Fleming, M.D.—Dr. Fleming infers that atropia is not a local paralyser, as has been supposed. Applied directly to the smaller arteries, atropia constricts them.—*Edinburgh Medical Journal*, March, 1863.

Traité des Désinfectants sous le Rapport de l'Hygiéne Publique. Par A. Chevalier.—Separate volume published by P. Asselin, Paris, 1863.

Report on Rabies. By M. Bouley.—This Report was made to the Academy of Medicine on a Memoir by M. Béviere. A very good description is given of the symptoms of hydrophobia; but the most important point advanced is that the idea of a mad dog having a horror of liquids is erroneous. A mad dog, says M. Bouley, has no aversion to water. On the contrary, he laps up the liquid and often swallows it. Although he may not drink, he nevertheless endeavours, and often with desperate efforts, to do so.—*Proceedings of the Séances of the Academy of Medicine of the 2nd and 9th of June: Journal of Practical Medicine and Surgery,* July, 1863.

Sull' idrato ferrico come antidoto dell' acido arsenioso. Di G. F. Lerov.—*Annali di Chimica applicata alla Medicina dal Dottor Giovanni Polli.* Aprile, 1863.

Des dangers qui peuvent résulter du séjour dans les localités ou l'essence de térébenthine ou d'autres produits analogues se trouvent en expansion. Par A. Chevallier.—*Annales d'Hygiène Publique,* Juillet, 1863.

De l'empoissonnement par les vapeurs de térébenthine. Liersch (de Cottbus).—*Ibid.*

Cas nombreux d'aliénation mentale d'une forme particulière ayant pour cause la perturbation politique et sociale de Février, 1848. Par Dr. Bergeret.—*Ibid.*

Du Mouvement de la Population en France en 1860. Par M. A. Legoyt.—This is a very valuable paper. It shows that the mean duration of life in France during 1860 was 37·8. This is an increase from 1817, when it was only 31·8, and from which time it has gradually risen to the present ratio. The male births were in the proportion 104·80 to 100 female, so the male sex preserves its predominance, although this proportion has been gradually diminishing, and if it continues doing so the sexes in France will very soon be equal. The number of natural children was 69,297; a proportion of 7·24 per cent. This is about the same as it has been for twenty years.—*Journal des Economistes,* April, 1863.

The Registrar-General's Twenty-fourth Annual Report of Births, Deaths, and Marriages in England in 1861.—The Registrar-General gives the following easy method of remembering the progress of population in England. Each given quinquennial period added a million or a little more to the account. In 1836–40 the population was rising through its sixteenth million; in 1841–45 through its seventeenth; in 1846–50 through its eighteenth; in 1851–55 through its nineteenth; and in 1856–60 through its twentieth million. In the middle of 1861 it is estimated to have reached 20,119,496.—*Blue Book* (price 1*s.* 6*d.*), 1863.

The Registrar-General's Annual Report of Births, Deaths, and Marriages for Scotland in 1858.—We gather from this Report the following interesting comparative facts: 1. That Scottish women are much more prolific than their English sisters; the proportion in the one country being at the rate of 345 births in every 10,000 of the estimated population, while in the other it was only in the proportion of 336, in the same number of the estimated population. 2. That the marriage-rate of Scotland is far below that of England, the proportions being 652 marriages in every 100,000 persons in the one, and 820 in every 100,000 in the other. 3. That education, as tested by the power of the brides and bridegrooms to sign the marriage-register, is far more advanced in Scotland; thus 88·2 in every 100 men were able to sign their names; only 11·8 being unable to do so, and affixing their mark; while of every 100 women, 76 were able to sign their names, and 24 only, unable to write, signed by mark; while in England 73 in every 100 of the men, and 62·4 in every 100 of the women, signed their names in full in the marriage-register; and 27 in every 100 men, and 37·6 in every 100 women, affixed their mark. 4. That though the death-rate for the year had increased considerably, it was still much lower than that of England—lower than the difference of race and climate can entirely account for; the mean annual death-rate in England being 220 in every 10,000 persons, and that of Scotland for the same period 208 deaths in a like population.—*Blue Book* (price 1*s.* 6*d.*), 1863.

First Report of the Commissioners of the Children's Employment Commission.—In this volume, the Commissioners treat of the pottery, lucifer-match, percussion-cap, paper-staining, lace and hosiery manufactures, and also of the finishers and hookers, and the fustian cutters. They describe a deplorable condition, physical and mental, of children engaged in these branches of industry.—*Blue Book* (price 4*s.* 6*d.*), 1863.

Report of the Sanitary State of the Army in India.—This is a remarkable document, and deserves careful study by medical readers who are interested in preventive medicine. The Commissioners show that in India, in the army, 100,000 men are reduced, in twenty years' service, to 9604. The following represents the causes of reduction:—Casualties of every kind, 90,396; by death in the service, 40,447; by invaliding, 14,627; by purchasing discharge, 3558; by the expiration of term of service, 8972; by promotion, 968; by transfer to Town Major's list, 5724; by transfer to other corps, 13,976; by desertion, 1818; and 306 by other causes. The mean term of service in India is said to be 8·6 years, and eleven recruits are required, annually, to every 100 men. The conclusions may be said to be this: in twenty years, 1000 men would be reduced by *death alone*, to 354; by death and invaliding, to 216; by *death, invaliding* and *other causes*, to 96.—*Blue Book* (price 3*s.*), 1863.

Further Observations on Death by Hanging. By Charles Croker King, M.D.—In this paper Dr. King gives the details of two cases of death by hanging in which he had the opportunity of examining the bodies after death. He combats the prevailing opinion that in all cases of hanging there is darkness and congestion of the face; or, indeed, any of that extreme congestion usually cited as diagnostic of this form of death.—*Dublin Quarterly Journal of Medical Science*, August, 1863.

Cases of Severe and Fatal Vomiting in Pregnancy. By J. Lang, M.D.—*Medical Times and Gazette*, August 1st, 1863.

Case of Poisoning by Turbith Mineral.—By Henry H. Lowndes, M.R.C.S.—*Medical Times and Gazette*, August 22nd, 1863.

Report of a Case of Death from Intoxication. By Donald C. Black, M.D.—*Lancet*, August 29th, 1863.

Case of Poisoning by Chloroformic Anodyne. By George Harley, M.D.—The patient, a girl, took six drachms of chlorodyne by mistake, and recovered after narcotism had continued, more or less intensely, for fifty-four hours. Coffee and nitric ether were administered internally; artificial respiration was employed, and cold affusion to the head. Dr. Harley describes the effect of cold to the head as almost magical. The recovery was perfect. Epistaxis took place in the course of the case.—*Lancet*, July 4th, 1863.

An Account of an Attempt to Restore to its Natural Appearances a Putrefied Dead Body, in order to prove its Identity. By Benjamin W. Richardson, M.A., M.D.—*Lancet*, May 16th, 1863.

Nouvelles Observations sur l'examen d'une Squelette dans les Recherches Médico-légales concernant l'Identitie, d'après le Docteur Antonio-Tarchini Bonfanti. Par A. Tardieu.—*Annales d'Hygiène Publique*, Juillet, 1863.

A Case of Moral Insanity in a Child. By J. Millar, L.R.C.P.—*Lancet*, May 23rd, 1863.

On the Practice of Vaccination in Galatz. By Isaac Harrinson, Esq.—*Social Science Review*, August, 8th, 1863.

Work and Overwork. By B. W. Richardson, M.A., M.D.—*Ibid.*, July 17th, 1863.

Homicidal Insanity—Is it True?—Ibid., April 4th, 1863.

Etudes Medico-Psychologiques sur la Folie. Par Dr. A. Vauze.—*Separate Volume published by V. Masson*, Paris, 1862.

Rapport Général sur les Travaux de la Commission des Logements Insalubres de Paris pendant les Années 1860, 1861. Par MM. Trebuchet et Robinet.—*Annales d'Hygiéne Publique*, Juillet, 1863.

On the Verdicts returned by Coroners' Juries in Madras in 1861-62.—*Madras Quarterly Journal of Medical Science*, April, 1863.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *A Case of Double Ovariotomy.* By Dr. Peaslee. (American Journal of Medical Science, April.)

A lady, aged thirty-five, prior to August 16th, 1862, had been tapped twenty-six times for ovarian dropsy. The amount of fluid removed varied from twenty to thirty pounds each time, and the intervals between the tappings for several months past had been only twelve or fourteen days. Ovariotomy was performed on the 30th August, the temperature of the apartment being kept at 80°, and its air rendered moist by the evaporation of water. The patient was kept under the influence of Squibbs' sulphuric ether, and the operator moistened his hands by means of an artificial serum, composed of chloride of sodium, four drachms; white of egg, six drachms; water, four pints. The tumour was found to consist of an aggregation of sacs, the largest of which was tapped, so that the tumour should become sufficiently diminished in size in order to admit of its being drawn through the incision. A double ligature was passed through its pedicle, which was then divided. The tumour being, however, extremely sessile, the ligature was applied with great difficulty. There were also many vascular tufts situated on its external surface, which bled profusely, even during gentle manipulation. This tumour having been removed from the right side, another was found to exist on the left side; but as the pedicle of this one was longer, and no hæmorrhage was present, this last was extirpated with ease. Every care was taken to remove all fluid which had gained admission into the peritoneum. The wound was closed by eight silver sutures, and water-dressing was applied. The chief feature in the after-treatment of this case consisted in the daily injection of the peritoneal cavity during the long period of eight weeks, commencing with the nineteenth day. A quart or two of the artificial serum already mentioned was thrown in once, twice or thrice a-day, and free issue was given to the fetid and turbid discharges; but as the albumen appeared to undergo decomposition by the peritoneal fluid, a liquid consisting of liq. sod. chlor., one drachm; sod. chlor., one drachm; water, one pint, was substituted. To these large peritoneal injections the author attributes his patient's recovery; for whenever they were delayed for too long a period, a certain and rapid recurrence of grave symptoms was observed. He believes that death has resulted in many unsuccessful ovariotomies in consequence of the presence of decomposed fluid in the cavity of the peritoneum. In all cases Dr. Peaslee prefers the ligature to the clamp, and he forms it of three threads of saddler's silk, waxed but not twisted. He regards the employment of large doses of narcotics during the first two or three days after the operation as unjustifiable.

II. *On the Agency of the Periosteum after Excisions.* By M. FORGET. (L'Union Méd., Nos. 61, 62; Gaz. des Hôp., Nos. 46–60.)

In this paper M. Forget passes in review a prolonged discussion which took place in the Paris Surgical Society, consequent upon a report which he presented to it upon Professor Rizzoli's memoir on "*sub-periosteal resections.*" During the discussion the whole question of the osteogenic power of the periosteum was handled, and the following are the ultimate conclusions which M. Forget believes to be fairly deducible: 1. The osteogenic property of the periosteum, brought into light by recent researches in experimental physiology, has been much more utilized of late years than formerly. 2. The part which surgery can derive from this property in the treatment of diseases of bone is limited, especially by the condition of the periosteum, the characters of these diseases, and the nature of the local and general causes which have produced them. 3. Sub-periosteal resections, applied to organic and traumatic lesions of bones, have not, thus far, furnished results resembling those derived from experiments upon animals. 4. Preserved in the midst of a resection, or of a fracture with loss of substance of bone, the periosteum may there become the generative element of a new ossification, which the surrounding tissues would themselves be incapable of producing in a like degree. 5. The bony tissue of this new formation is not a faithful copy of the physiological bone. It is only an incomplete production of its form, solidity, functional aptitude, and anatomical structure. 6. In those pathological cases in which clinical experience determines amputation to be necessary, no fact, to the present time, has demonstrated the possibility of avoiding this by means of sub-periosteal resection. 7. Nor has any clinical observation as yet shown the superiority and advantages of sub-capsular periosteal resection in the treatment of surgical affections of the joints, whether spontaneous or traumatic. 8. In the operations for facial autoplasty, the periosteum may be usefully comprised among the flaps serving as a basis for the production of osseous or osteiform tissue, capable of repairing loss of substance and solutions of continuity, undergone by the bones of the face.

III. *On Periostitis and Osteomyelitis.* By Professor ROSER. (Archiv der Heilkunde, No. 3.)

Professor Roser observes that with the disease termed periostitis, osteomyelitis almost constantly exists. The inflammatory exudation into the medullary cavity gives rise to the remarkable result of forcing out the medulla through the pores of the bone, so that fluid fat, often in large quantity, is found behind the periosteum. As this fact has been doubted by Demme and others, the author adduces the following proofs of its reality: 1. In a series of cases he has convinced himself by direct clinical observation, having found in recent periosteal abscesses drops of fat behind the periosteum. 2. In a patient, the subject of acute periostitis, and carried off during the first stage of inflammation of the brain, fat was found a line in thickness between the bone and the periosteum. 3. In the year 1854, Dr. Andrea performed the following experiment, which is related in his thesis *De Periostitide.* A piece of fresh tibia was laid in warm water for half an hour, in order that the fat might be rendered fluid. The diaphysis was sawn across at the upper part, and from its lower part, a portion of the periosteum was removed. A rod, which did not entirely fill it (so that too great pressure might not be exerted), was passed into the medullary canal. Whenever strong and rapid pressure was induced by means of this rod, a flow of fat took place from the Haversian canals; and when the pressure was but slow and gentle, the fat issued slowly, drop by drop. The experiment repeated with the other tibia furnished the same results. From these considerations the author is of opinion that many examples of inflammation and separation of the periosteum are only secondary to myelitis. Cases are met with in which myelitis has evidently preceded, and periostitis, situated higher up the bone, has followed.

IV. *On the Cause of Death from Superficial Burns.* By M. Baraduc. (L'Union Médicale, No. 60.)

M. Baraduc's observations are confined to extensive burns, accompanied by large phlyctænæ. Dupuytren ascribed death resulting from these to the shock consequent on the excessive pain; but the author believes it due to modifications in the condition of the blood. The serum of this fluid is abstracted in excessive quantity, and the accidents which result are proportioned in gravity to the amount of this abstraction. Besides reasoning from analogy, M. Baraduc adduces the results of autopsies in support of his theory. He has found that the viscera of persons dying from the effects of extensive superficial burns exhibit a surprising dryness of tissue, while there is an absence of fluid in all the serous cavities. The right cavities of the heart, too, are empty, while the left are gorged with dark, coagulated, non-fibrinous blood. The pulmonary veins and the aorta are filled with similar blood, while the venæ cavæ are nearly empty. The femoral, popliteal, and brachial arteries are gorged with blood of a gelatinous consistency, the corresponding veins being almost empty. Death in these cases is then to be attributed to the difficulty or impossibility which the blood, inspissated by the loss of its serum, finds in passing from the arteries into the veins. The indications for treatment are to increase and render fluid the mass of the blood, and arrest the exosmosis, which constitutes the vesication. For the fulfilment of the first of these, the patient should be kept in a bath (at a temperature of 80° to 82°) for ten to fourteen hours, and abundant drinks of a slightly diuretic or emollient nature should be given him. Enemata and tepid injections into the bladder should also be had recourse to, and the vapour of some emollient fluid should be inhaled. If the patient is very weak, chicken broth, or even a fuller diet, must be given. The fluidifying of the blood may be promoted by substituting alkaline baths, drinks, and injections. To prevent a continuance of the exosmosis, the parts, after the patient has left the bath, must be dressed with cerates, thickly covered with wadding, and submitted to gentle compression by means of a bandage. The bath should be repeated daily, its duration being gradually abridged. In two cases, the author has been successful in fulfilling these indications.

V. *Apparatus for Securing the Patient during Lithotomy.* (Gaz. des Hôp., No. 49.)

M. Charrière, the French surgical instrument-maker, has contrived an apparatus which admits of very ready application. The patient need not, in fact, be aware that he has been tied at all, as the manœuvre may be executed while he is under chloroform; and when the operation is over, the apparatus can be immediately unfixed. Padded leather bracelets are attached to the wrists and the lower part of the legs, and are rendered immovable by tightening a buckle. Those attached to the leg are kept in their place by a strap passing beneath the foot, so as to enable them to resist the traction made by the arms, an iron ring being also fixed opposite the external malleolus. On the radial side of the wrist-bracelet is also attached a moveable hook, which enters into the ring of the ankle when the patient is placed in the position suitable for the operation.

VI. *On the Treatment of Hospital Gangrene with Bromine.* By Messrs. STANFORD and WEEKS. (American Medical Times, July 18 and Aug. 1.)

Both these gentlemen, surgeons in the United States army, and having had abundant opportunity of witnessing cases of hospital gangrene, strongly recommend the adoption of the treatment by means of bromine, originally introduced by Surgeon Goldsmith. They report that its efficacy is most prompt; a single application, providing this be effectual, often sufficing to arrest the progress of the disease, and cause a rapid amendment in the constitutional symptoms. For the effectual application, all sloughs must be cut away down to the living tissues, and these washed with warm water, and dried. Pure bromine, which is preferable to any dilution, is then to be completely applied by means of a mop. Every portion of the wound must be saturated with the fluid; and to secure its being so, the bromine must be well pushed into the cellular tissue by means of a piece of stick, or the small round end of a small glass test-tube, while it may be thrown into any irregular cavities through a syringe. If after the fourth day any of the odour peculiar to the disease remain, a second application may be made to any parts which had escaped the first, and which will be discovered by removing the surface charred by the bromine.

VII. *On the Treatment of Enlarged Cervical Glands in Children.* By M. GUERSANT. (Bull. de Thérapeutique, No. 8.)

For the purpose of obtaining *resolution*, we do not now, as formerly, rely upon the application of leeches; and if these are still exceptionally employed, this can only be done when the adenitis is local, and when the subject is not lymphatic. In other cases, leeches only produce relief for a few days, while suppuration is retarded, and the patient's strength is wasted. Resolvents are more to be relied on, such as mercurial ointment, with or without extract of belladonna, and calomel ointment. Iodine ointments are also of use; but they require to be combined with glycerine, and to be watched, lest they irritate the skin, which is so delicate in children. Emollient poultices, and especially those made of mallow flower, are sometimes useful, or wadding may replace them with advantage. Tincture of iodine, applied every other day, may also hasten resolution, and is often preferable to cataplasms, which become sour and cold. When *suppuration* begins to manifest itself, resolvents are useless; although blisters may sometimes, though rarely, arrest the course of the abscess. Emollients, such as general baths, and maturative poultices, should be resorted to; and as soon as fluctuation is evident, before the skin becomes too red, the abscess should be opened, in order to avoid the numerous apertures, detachments of the skin, and scars, which ensue when it is allowed to burst. Without always rejecting the bistoury, M. Guersant, in most cases, greatly prefers the filiform suture. Three or four silk threads are passed through the abscess by means of a fine, flat needle, in such a manner that one of the punctures is in a dependent position, while the threads lie in the direction of the folds of the skin or the course of the muscular fibres. The cataplasms are continued, and the seton is moved daily, until the abscess is emptied. It should be retained in as long as any discharge or tumefaction remains, the disappearance of the latter being hastened by its presence. The two punctures heal up without leaving any traces. When the adenitis terminates in *induration*, resolvents must be employed, such as iodine ointment—*empl. cum hydrarg.*—but if these, with the internal use of iodine, do not prove efficacious, the seton should be applied at various points, in order to induce more active inflammation and suppuration.

The above observations apply to superficial adenitis; but sometimes these swellings are deep-seated, even in children at the breast, constituting abscesses by the side of the larynx, trachea, pharynx, or œsophagus. A retro-pharyngean abscess is sometimes, by reason of the change of voice it produces, mistaken for enlarged tonsil. Whenever these abscesses are sufficiently advanced, they should at once be opened; asphyxia is thus prevented, and the patient often does very well. Such abscesses must not, however, be confounded with those dependent upon carious vertebra, which are of much slower growth, and preceded by far more serious symptoms. Deep-seated, enlarged scrofulous glands, placed by the side of the air-tube and the large vessels, have been sometimes removed by operation; such operations, however, should very rarely be attempted—giving rise, as they do, to imminent danger, and frequently having to be left unfinished.

VIII. *Case of Asphyxia from the Fall of a Canula into the Air-passages.* By M. Champouillon. (Gazette des Hôp., No. 64.)

An Arab, aged twenty-five, had, in the year 1859, undergone the operation of tracheotomy, on account of impending suffocation. From that time he had continued to wear the canula, through which he breathed very freely. In March of the present year he came to the hospital at Tlemcen, exhibiting only the disc of the canula, which had been fractured, and indicating by signs that the rest had fallen the night before into the air-passages. The respiration continued just as easy as if the canula were *in situ*, and the passage of the entire length of a probe detected nothing. The Arab was very poor, and it was generally believed that he had sold the missing part of the canula; a supposition which was rendered the more probable by the regularity of his respiration and the calmness of his physiognomy. The missing portion, also, was at least twenty *millimetres* long, and yet could not be felt by the probe. On examining the disc of the canula, the walls of the instrument were found to be very thin, and the fracture was clean and shining; but how it was produced has never been ascertained. It was determined to postpone any active operations; but the patient, in a few hours, suddenly expired, without having given any cause of fear to those watching him. The lungs were gorged with black blood throughout their entire extent, and on their surface were a dozen emphysematous lobules. On opening the larynx, a polypus, the size of a small pea, was found attached to the lower surface of each ventricle. The lower vocal chords were thickened, and the apex of the triangle forming the aperture of the glottis was traversed by a fibrous bridle, which reduced the extent of the aperture about one-fourth. The trachea presented no trace of granulations at the point where the canula had lain during four years, there being a simple and inconsiderable thickening of the mucous membrane. The trachea had undergone, from the larynx to beyond the bifurcation, an uniform dilatation, giving it a greater diameter by five or six *millimetres* than in the normal condition. This remarkable dilatation was even continued into the bronchi. At the level of the bifurcation the tube of the canula was found engaged within the right bronchus. It is probable that had the laryngoscope been known at the period when tracheotomy was performed in this case, the polypi might have been detected and removed. The dilatation met with, consequent upon the obstructions here caused, facilitated the passage of the detached portion of the canula as far as the bifurcation. The author is at a loss to explain the suddenness of the death by what was found; but he is of opinion that attempts at extraction would have been useless, as the lightest displacement of the tube might have brought on fatal asphyxia. The canula was of faulty construction, being very thin in texture, and much worn at its point of attachment to its disc.

IX. *On Amygdalotomy.* By Professor Deroubaix.
(Presse Méd. Belge, Nos. 31 and 38.)

M. Deroubaix cannot agree with Begin, that this is the simplest operation in surgery, for even with instruments which render it of so much easier performance than heretofore, it still sometimes presents difficulties and danger when certain precautions are neglected. It is of importance to bear in mind that the tonsil is not an exactly defined organ, like a more perfect gland, but has a tendency to become confounded by a kind of transition with the glandular systems of portions of the neighbouring mucous membranes. In the normal condition, it makes but a slight projection between the pillars of the velum; but in the case of pathological change, the two tonsils may touch each other,—respiration, phonation, and deglutition becoming impeded. It is generally in predisposed subjects, as the result of repeated irritation, especially that arising from the action of cold and damp, that an indurated exudation into the follicles, and a sufficiently hypertrophied condition to call for the intervention of surgery, are observed. It is rare, indeed, when the affection has reached this stage, that any local treatment will spare the necessity of an operation; and the author has frequently in vain had recourse to the whole train of remedies, during a prolonged period, without obtaining any diminution in the engorgement or alleviation in the symptoms. It is far better in such cases to employ the appropriate treatment, without teasing the patient by these indifferent measures. In reply to the question whether the removal of the tonsils does not give rise to serious inconvenience, it may be said that to attempt their total ablation would be to risk the perforation of the wall of the pharynx and a lesion of the carotid. In fact, a little more only than the portion which projects beyond the level of the pillars is excised; and this is done without any inconvenience, for all the follicles being independent of each other, the same consequences are not to be feared which would result in the case of a more complicated gland, the different portions of which have mutual relations with each other. Almost always, too, the cure effected is permanent; and it is only in very rare cases that the engorgement is, after some years, reproduced. If, however, by reason of faulty instruments, a mere superficial slice of the tonsil or a portion of its upper or middle part be removed, relapse will follow without much delay. It is highly important to observe, that while at the upper part the pillars of the velum oppose a continual barrier to the tonsils, nothing arrests their development below; so that their chief volume, when enlarged, lies often in this direction. But as this region is not displayed when the mouth is opened and the tongue only moderately depressed, the portion of the tonsil which is then made visible is alone removed; and a part of the diseased tissue below remaining untouched, a relapse is certain to occur. It is from having at an earlier period met with these relapses, due to incomplete operations, that M. Deroubaix turned his attention to the improvements of the instruments employed in tonsillotomy. He rejects the bistoury as not only difficult, but even dangerous in its employment. In fact, he has witnessed a case in which the carotid was fatally perforated. The amygdalotomes formerly in use all erred in consequence of the plate for the reception of the tonsil having its large diameter continuous with the axis of the instrument, while the tonsil is developed in the vertical, and therefore contrary direction. M. Deroubaix first contrived an instrument having its plate placed perpendicularly; but finding it difficult to introduce this low enough in the pharynx to embrace all the diseased tonsil, he so changed the disposition that the plate of the instrument is not perpendicular to the handle but oblique, forming with it an open obtuse angle. This easily embraces the whole of the surface to be removed. The operation can be executed with celerity and certainty. It should never be resorted to during the inflammatory stage; for not only is it then very painful and liable to consecutive accidents, but the tissue of the gland is not firm enough to resist the traction. Although tonsillotomy is usually of easy execution, great difficulty is sometimes produced by the terror or indocility of the patient. This is often only to be overcome by prolonged waiting and watching for the opportunity which the patient, by opening his mouth, at last gives of seizing the tonsil with promptitude. Sometimes a patient who has submitted to the removal of one tonsil, obstinately refuses to allow of the second being removed. Such a case is best met by having two tonsillotomes ready. Immediately that the first tonsil has been excised, almost before the patient is aware of it, the second instrument may be applied. When the conformation of the mouth renders the isthmus difficult of access, it is preferable to depress the tongue by means of the amygdalotome itself, than to employ any special instrument for depressing it, which only complicates the operation. M. Deroubaix has never met with hæmorrhage after this operation that could not be controlled by a simple vinegar gargle.

Summary.

Amputation.—Poupart on Successful Case of Amputation at the Hip-joint. (Bull. de l'Acad. de Méd. de Belgique, No. 4.)—Autopsy many years after Amputation at the Hip-joint. (Bull. de la Soc. de Chir., vol. iii. p. 56.)—Debout on Protecting Envelopes for Stumps after Amputation. (Bull. de Thérapeutique, vol. lxv. p. 88; Gaz. des Hôp., No. 94.)

Anus.—Rochard on Formation of Artificial Anus in Imperforation. (Bull. de la Soc. de Chirurgie, vol. iii. pp. 140, &c.)

Astigmatism.—Kugel on Oblique Vision in Astigmatism. (Wien. Wochenschrift, Nos. 27, 28.)

Blepharoptosis.—Deroubaix's Case of Blepharoptosis, treated by Autoplasty. (Presse Méd. Belge, No. 12.)

Burns.—Verneuil on Treatment of Cicatrices. (Bull. de la Soc. de Chir., vol. iii. p. 519.)

Cæsarean Operation.—Winckel's Fifteen Cases of Cæsarean Operation. (Monatschrift für Geburtskunde, July.)

Catheterism.—Mercier on the Employment of Gum-elastic and Angular Bougies. (Gazette Méd., Nos. 23 and 25.)

Coloboma.—Becker on Successful Operation for Congenital Coloboma. (Spitals Zeitung, Nos. 16–18.)

Dislocation.—Doig on Two Cases of Dislocation of the Cervical Vertebræ. (Edinb. Med. Journal, August.)—Ancelet on Dislocation of Sternum. (Gaz. des Hôp., No. 35.)—Demarquay on Double Dislocation of the Jaw, with Anatomical Examination. (Bull. de Thérapeut., vol. lxvi. p. 472; Union Méd., No. 80.)—Parow on the Treatment of Congenital and Old Dislocations of the Femur. (Journal für Kinderkrank. No. 3.)—Poulet on Dislocation of the Elbow inwards. (Gaz. des Hôp., No. 86.)—Moritz's Cases of Dislocation of the Cervical Vertebræ. (Petersb. Med. Lect., No. 5.)

Drainage.—Bottini's Clinical Remarks upon Surgical Dislocations. (Omodei's Annali.)

Ear.—Schwartze's Report on Recent Publications on Diseases of the Ear. (Schmidt's Jahrbuch, vol. cxviii. No. 3.)—Voltolini on Pathological Anatomy in cases of Deafness and Deaf-dumbness. (Virchow's Archiv, vol. xxvii. No. 1.)—Gruber on Pathological Changes in the Eustachian Tube. (Wien. Wochenschrift, No. 26.)

Elephantiasis.—Montgomery on Operative Treatment of Elephantiasis Scroti. (Madras Quarterly Journal, No. 12.)

Emphysema.—Morel-Lavallée on Traumatic Emphysema. (Gazette Méd., Nos. 29, 31, 32.)—Chevance's Case of Emphysema of the Cranium, consequent on Fracture. (Union Méd., No. 98.)

Excision.—Trélat's Report on a Case of Excision of the Elbow. (Bull. de la Soc. de Chirurgie, vol. iii. p. 202.)—Legouest on Excision of the Malleolus. (Ibid., p. 480.)—Smith on Excision of the Head of the Femur after Gun-shot Wound. (American Med. Times, vol. vii. No. 2.)

Eye.—Debout on Ocular Prothesis. (Dublin Quarterly Journal, August.)

Fistula Lachrymalis.—Reybard's New Considerations concerning Fistula Lachrymalis. (Gaz. des Hôp., No. 87.)

Fracture.—Tarjovy on Fracture of Lower End of the Radius. (Bull. de Thérap., vol. lxv. No. 1.)—Richet on Fracture of Acetabulum. (Bull. de la Soc. de Chirurgie, vol. iii. pp. 224–248.)—Sands' Case of Fracture of the Jaw Treated by Intra-dental Splints. (Amer. Med. Times, Aug. 8.)

Gun-shot Wounds.—Vezein on Detection of Balls in Gun-shot Wounds. (Recueil de Méd. Militaire, April. The author passes a dressing forceps down to the ball, and by a torsion movement detaches a fragment of lead.)

Hemeralopia.—Netter on the White Spots observed on the Sclerotica in Hemeralopia. (Gazette Méd., No. 31. The author maintains that the pearly spots declared by M. Bitot, as noticed in our last number, to be pathognomic of hemeralopia, are a mere epiphenomenon occurring only in some cases.)—Desmoretz on Treatment of Hemeralopia. (Recueil de Méd. Mil., April. Judging from the experience of three epidemics, the author is of opinion that no treatment is efficacious. The spontaneous disappearance of the disease may be hastened by seclusion in a darkened chamber.)—Eitner on an Epidemic of Hemeralopia observed on board the *Ancona.* (Deutsche Klinik, No. 25.)

Hernia.—Phillipeaux on Cauterizing the Omentum after Operation for Hernia. (Bull. de la Soc. de Chir., vol. iii. p. 338.)—Metius on the Employment of the Peristaltic Action for the Reduction of Hernia. (Küchenmeister's Zeitschrift, No. 4.)—Chassaignac on the Seat of Stricture in Strangulated Hernia. (Gazette des Hôp., No. 43.)—Gosselin on Cases of Hernia occurring at La Pitié. (Ibid., Nos. 42 and 44.)—Maisonneuve on Reduction of Hernia by Elastic Compression. (Ibid., No. 94.)

Horny Excrescences.—Demarquay's Observations on Horny Excrescences. (Bull. de la Soc. de Chir., vol. iii. p. 552.)

Intestinal Obstruction.—Adelmann on Surgical Treatment of Intestinal Obstructions. (Prag. Viertel. vol. lxxviii.)

Jaw.—Verneuil's Report on the Results of Rizzoli's Operation for Pseudo-arthrosis in Ankylosis of the Jaw. (Gaz. des Hôp., No. 93.)—Boineton a Case of Immovable Jaw, in which Section was performed. (Ibid., No. 90.)

Knee.—Buntzen and Rupprecht on Removal of Foreign Bodies from the Knee. (Schmidt's Jahrb., vol. cxviii., No. 3.)—Borrelli on Treatment of Angular Anchylosis of the Knee. (Gazzett Medica Italiana, No. 30.)

Ligature.—Richard on Elastic Ligature. (Gazette Hebdom., No. 26. The author has made some successful trials with caoutchouc ligatures in the removal of tumours, operations for fistula, &c.)

Lithotrity.—Civiale on the Employment of Lithotrity in Cases of Lesion of the Bladder. (Gaz. des Hôp., No. 67.)—Beyran on Lithotrity in Children. (Ibid., No. 88.)—Ivanchich's Report on Cases of Lithotrity. (Wien. Wochenschrift, No. 26. In this paper the author reports his 133rd case.)

Nævi Materni.—Guersant on the Treatment of Nævi. (Bull. de Therap., vol. lxiv. p. 494.)

Orbit.—A. Graefe on Inflammation of the Orbital Cellular Tissue. (Schmidt, Jahrb., vol. cxix. No. 2.)

Ovariotomy.—Buchanan's Case of Ovariotomy. (Edinb. Med. Journ., Aug.)—Huguier's Fatal Case of Ovariotomy. (Bull. de l'Acad. de Méd., No. 18.)—Grafton's Successful Case of Ovariotomy. (American Medical Times, Aug., No. 5.)—Courty on Ovariotomy as performed in England. (Gazette Hebdomadaire, Nos. 32, 34, and 35.)—Krassowski's Successful Case of Ovariotomy. (Petersburg Med. Zeitschrift, No. 4.)

Patella.—Rizet on the Office of the Patella in relation to its Accidents. (Gaz Méd., No. 34.)

Perineum.—Verneuil on Operation for Laceration of the Perineum. (Bull. de la Soc. de Chir., vol. iii. p. 220.)

Polypus.—Ozanam and Trélat's Cases of Extirpation of Polypus of the Larynx. (Rev. Méd., July, and Bull. de l'Acad. de Méd., No. 14.)—Dolbeau and Verneuil on Polypus of the Nasal Fossæ. (Bull. de la Soc. de Chir., vol. iii. pp. 503, 537.)

Prostate.—Pauli on Hypertrophy of the Prostate. (Virchow's Archiv, vol. xxvii. No. 1.)

Pupil.—Guépin on Formation of Artificial Pupil. (Bull. de Thér., vol. lxiv. p. 468.)

Purulent Infection.—Guérin's Criticism on Flourens' Experiments. (Gaz. Méd., No. 25.)

Retina.—Blessig on Hæmorrhage of the Retina. (Petersburg Med. Zeitschrift, 5.)

Rhinoplasty.—Verneuil on Rhinoplasty. (Bull. de la Soc. de Chirurgie, vol. iii. pp. 62, 108, 497.)

Stricture.—Watson's New Instrument for Relief of Retention in Stricture. (Edin. Med. Journal, July.)—Dolbeau's Urethrotomy in Stricture and Discussion. (Bull. de la Soc. de Chir., vol. iii. pp. 66, 69, 75, 81.)—Perrin's Cases of Internal Urethrotomy. (Gaz. des Hôp., No. 81.)—Maisonneuve's Results of Cases of Urethrotomy. (Ibid., No. 98.)

Syphilis.—Güntz on Syphilitic Fever. (Küchenmeister's Zeitschrift, No. 3. By this term the author indicates the fever which is present on the appearance of secondary symptoms.)—Cullerier's Report on Contagiousness of Secondary Symptoms. (Bull. de la Soc. de Chir., vol. iii. p. 36.)

Tracheotomy.—Buchanan on Tracheotomy in Croup and Diphtheria. (Glasgow Med. Journal, July.)

Transfusion.—Demme on the Employment of Transfusion in Military Surgery. (Schmidt's Jahrbuch., vol. cxviii. No. 3.)—Broca's Report on Transfusion. (Gaz. des Hôp., No. 96.)—Panum's Experimental Investigation of Transfusion. (Virchow's Archiv, vol. xxvii. pp. 240, 295.)

Tumours.—Virchow's Statistics of Tumours. (Virchow's Archiv, vol. xxvii. No. 3.)—Jobert on Tumours of the Scalp. (Gaz. des Hôp., No. 68. Jobert prefers excision to caustic, employing either nitric acid or Vienna paste.)

Ulcer.—Rochard on the Cochin-China Ulcer. (Bull. de la Soc. de Chir., vol. iii. p. 144.)

Uranoplasty.—Pitha on (Jahrb. der Wien. Gesell., No. 2, and Wien. Wochenschrift, Nos. 24, 25.)—Heyfelder on Langenbeck's Operations. (Gaz. des Hôp., No. 98.)

Urinary Passages.—Schmidtlein on Tuberculosis of the Urinary Passages. (Deutsche Klinik, Nos. 22-27.)

Uterus.—Borlee on Extirpation of Fibrous Tumour of the Uterus. (Presse Médicale Belge, No. 33.)

Vagina.—Debout on Operations in Malformations of the Vagina. (Bull. de Thérap., vol. lxv., No. 1.)

Varices.—Dentu on the Treatment of Varices by Perchloride of Iron. (Union Med., Nos. 84, 85.)

Vesico-vaginal Fistula.—Roubaix on the American Operation for Vesico-vaginal Fistula. (Bull. de l'Acad. de Méd. Belgique, No. 2.)—Ulrich and Heppner's Cases of Operation for Vesico-vaginal Fistula. (Jahrb. der Wien. Gesellsch., No. 2; Petersburg Med. Zeitsch., No. 4.)

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D., F.R.C.P.
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I. The Non-Pregnant State.

1. *Clinical Illustrations of Obstructive Dysmenorrhœa, and on a New Uterine Dilator.* By James G. Beaney. (The Medical Record, Melbourne, March, 1863.)
2. *Injection of Iodine in Monocystic Ovarian Disease.* By Scanzoni. (Spitals. Zeitung, July, 1863.)

1. Mr. Beaney relates cases in which more or less complete obliteration of the uterine cervical canal had taken place as the result of inflammation leading to "obstructive dysmenorrhœa." Relief was obtained by artificial dilatation. He also enumerates the following conditions as rendering the expulsion of the menstrual fluid difficult: 1st. Extreme rigidity of the mouth and cervix. 2ndly. Cicatrices of the mouth and canal, or adhesions between the former and the upper part of the vagina. 3rdly. Hypertrophy of the mucous membrane. 4thly. Alterations in the directions of the canal, depending either on growths in it, or on double flexion of the neck. Mr. Beaney gives illustrations of these various conditions, and of the use of a uterine dilator of his construction.

2. From a summary of six cases of presumed monocystic ovarian disease, in which nine injections of pure tincture of iodine, and one injection of a solution of potassa fusa were made, it results that,—in all cases the operation was without effect; the cyst filled again. It is, however, also concluded that the injection is harmless. In every case some of the tincture must remain in the cyst, and it is probable that some always gets into the abdominal cavity, for after every puncture, and the removal of the trocar, there escapes externally more or less of the cystic fluid, whence it may be inferred that some also finds its way between the cyst and the abdominal wall when the cyst-wall is not adherent.

II. Pregnancy.

1. *Extra-uterine Pregnancy.* By Dr. Simpson. (Edinb. Med. Journal, Sept. 1863.)
2. *On the Cervix Uteri in Pregnancy.* By Dr. Matthews Duncan. (Edinb. Med. Jouranl, Sept. 1863.)
3. *Diseases of the Circulation and Respiration, caused by the Mechanical Action of Pregnancy.* By Dr. De Cristoforis (Malachia). (Annali Universali di Med., August, 1863.)

1. Dr. A. Simpson related to the Edinburgh Obstetrical Society, for Professor Simpson, a very interesting case of extra-uterine gestation. The patient was forty-five years of age; she had not been pregnant for twelve years. She was confined to bed with severe pelvic pains. There was a sensitive tumour stretching across the pelvis, between the uterus and rectum. Suddenly this tumour greatly enlarged in the course of twenty-four hours, and produced very distressing and dangerous symptoms. Dr. Simpson made an opening into the mass through the roof of the vagina. Several large, old, and recent coagula were discharged, and at last the foot of a small fœtus projected; a slender, early, entire fœtus, with placenta, was extracted. The patient recovered.

2. Dr. Matthews Duncan gives another illustration of the condition of the

cervix uteri in pregnancy. A woman far advanced in the eighth month died of typhus in the Edinburgh Infirmary. Shortly after death, the fœtus was removed by Cæsarean section. It survived five hours. The cervix measured about an inch in length; it easily admitted the finger; its tissue was greatly hypertrophied and completely softened in every part. The rugæ, especially the anterior and posterior columnæ, were greatly hypertrophied and prominent. The lower margin of the cavity of the cervix could be identified by the presence of a row of Nabothian follicles; the upper margin by the abrupt termination of the arbor vitæ and the expansion into the smooth-walled cavity of the uterus. Dr. Duncan gives extracts from the writings of De Graaf (1677) and Weitbrecht (1750), showing that these authors were aware of the true anatomical condition of the cervix during pregnancy, and that it does not become unfolded to form part of the general uterine cavity, as has been taught in recent times.

3. Dr. De Cristoforis discusses with great fulness the consequences of the mechanical action of the gravid uterus upon the circulation and respiration. He shows that the pressure of the gravid uterus causes on the one hand a repletion, an arterial hyperæmia above, and on the other a venous stasis below. The affections so produced the author considers and illustrates in detail. In discussing the condition described as mechanical superior arterial hyperæmia, he insists upon not confounding this with plethora, inasmuch as the distressing symptoms occur in patients whose blood is actually impoverished in quality. A feature of this condition is the greater development of the superficial veins of the arms and neck. One of the consequences of this hyperæmia is pulmonary distress, slight cough, sanguineous expectoration. He cites cases to show the efficacy of small bleedings in relieving these symptoms, maintaining that bloodletting acts by relieving the circulating system of the excess of blood. He relates a case of tracheo-laryngeal irritation produced by this upper mechanical hyperæmia, which was relieved by the simple emptying of the womb.

With regard to the influence of gestation upon the course of phthisis, he contends that it is disastrous. The pulmonary congestion caused by the upper mechanical hyperæmia renders the lung less pervious, and induces a serous infiltration of the lung-tissue, which united to that of the specific deposition, have for effect the acceleration of the softening of the tubercles. He gives cases in support of his view.

Eclampsia is an affection which the author also assigns to this superior arterial hyperæmia. He rests this hypothesis upon cases drawn from a memoir by Dr. Timermans.

(Some of these were marked by general anasarca, and may therefore be presumed to have been associated with albuminuria.)

The hypertrophy of the right ventricle of the heart, shown by Larcher to attend pregnancy, the author attributes to the pressure of the gravid womb upon the great arterial trunks, aorta, and iliacs. The increased strain thrown upon the heart to overcome this obstacle leads to the increased growth of the organ.

Sudden Deaths during Pregnancy.—Cerebral apoplexy may follow as a consequence of the mechanical superior hyperæmia associated with the hypertrophy of the left ventricle. Pulmonary apoplexy may also result from this condition of the heart acting by impeding the re-flow of the blood from the lung to the left cavities of the heart, caused by the diminution of the capacity of the left ventricle.

The condition called chloro-anæmia by Caseaux, and by others hydræmia, De Cristoforis again traces to pressure. He describes a general œdematous infiltration of the subcutaneous cellular tissue, accompanied by similar œdema of the pulmonary tissue, as a disorder of essentially hydraulic nature. The inferior œdema, and that of the abdominal walls, arises from the pressure upon the iliac veins and their branches; that of the face, arms, and pulmonary tissue is due to the pressure upon the aorta. The imperfect hæmatosis resulting from this mechanical obstruction gives rise to the defective development of globules and the general deterioration of the blood. The author is careful to exclude albuminuria as an explanation of this œdema, since he has found it absent in the cases he seeks to explain.

Anasarca Gravidarum.—Under this name the author describes minutely a condition which he distinguishes from the œdema just referred to. The following case serves for a clinical description:

A countrywoman, aged twenty-four, at the end of her first pregnancy had œdema of lower extremities, and anasarcous tendency. During the eighth month she suffered from cough and difficult respiration. The abdominal parietes were involved in the œdema. The heart-sounds were indistinct; she had headache and vertigo. She was bled several times without sensible relief; took aperient powder and squills, which caused diarrhœa. The respiration became more difficult; the anasarca increased. She died from increasing orthopnœa. The abdomen was opened, and an eighth-month fœtus extracted. Autopsy showed slight sub-arachnoidean serosity; the cerebral substance anæmic, soft; a tumblerful of yellow serum in the pericardium; the lungs œdematous throughout two-thirds of their extent, with signs of recent congestion at the base; the heart was soft, tending to fatty; the parietes of the left ventricle hypertrophied with diminution of its cavity; the liver voluminous, fatty, slightly congested with black blood; the spleen hypertrophied, soft. All the rest was normal, excepting the great œdema of the inferior extremities and of the pudenda.

The author relates six other similar cases, all ending fatally, and in like manner illustrated by post-mortem examination.

The author insists that the concourse of symptoms here depends upon increase of pulmonary œdema, a vitiation of the heart, and the collection of serum in the pericardium. The fatty, soft condition of the heart he traces to the same conditions which induce fatty liver and flaccidity in the spleen—that is, imperfect hæmatosis, long protracted, causing defective nutrition.

The author concludes this very interesting memoir with citations and cases showing the frequency of the complication of heart disease with pregnancy, and the fatal consequences of this affection.

III. Labour.

1. *On Immovable Pelvic Tumours, and their Influence upon Labour.* By Dr. Gustav Braun. (Wien. Med. Wochenschr., xiii. 1863.)
2. *On Pelvic Versions in certain Cases of Contraction of the Pélvis.* By Dr. Hyppolyte Blot. (Arch. Gén. de Méd., July, 1863.)
3. *A New Method of Embryotomy and of Decapitation.* By M. Pajot. (Arch. Gén. de Méd., Aug. 1863.)
4. *On Presentations of the Trunk in Extreme Contractions of the Pelvis.* By Dr. Pajot. (Arch. Gén. de Méd., Aug. 1863.)
5. *Considerations upon Two Cases of Obliquity of the Fœtus during Pregnancy, with Presentation of the Left Shoulder many Days before Labour; Reduction of the Cephalic Extremity by Pressure with a Double Elastic Belt; Labours at Term, natural, Occiput being in the First Position.* By Professor Lazzati, of Milan. (Annali Univ. di Med., Aug. 1863.)
6. *On Artificial Dilatation of the Os and Cervix Uteri by Fluid Pressure from*

above. A Reply to Drs. Keiller, Barnes, and Arnott. By Dr. H. Storer. (Boston Med. and Surg. Journal, July, 1863.)

7. *Report to the Academy of Medicine on Dilating Instruments to provoke premature Labour.* (This is a Report by M. Devilliers on instruments used by MM. Daudé (of Marvejols), Lépine, and Moyne (of Dijon).

1. Dr. G. Braun gives a collection of cases of obstructed labour from immovable pelvic tumours, and adds the relation of two cases observed by himself.

Case 1. Fibroïd on the left wall of the pelvis. A woman had borne a child easily three years before. Braun saw her when thirty-four weeks pregnant with a living child, presenting by breech. There was attached to the left ischium a convex, smooth, hard tumour, the size of an orange, narrowing the pelvic outlet to a transverse diameter of 2¼". Premature labour was induced by vaginal injections continued during three days. There was great difficulty in extracting the arms. The patient had fever, with rigors, three days after labour, and died on the fifth day, under symptoms of œdema of the lungs. *Autopsy:* Endometritis, purulent peritonitis, and infiltration of the retro-peritoneal cellular tissue. The pelvis was narrowed by the hard tumour, which had its basis almost entirely occupying the left wall. The vagina was pushed over to the right by it. It consisted chiefly of fibrous structure.

Case 2. A woman, aged forty, had borne five living children, the last after a severe labour, aided by forceps. She had often aborted. She was now at the end of her sixth pregnancy. Labour had lasted two days without the head coming down. The brim of the pelvis was found contracted to 2½" by a tumour the size of a fist, very hard, quite immovable, of knobby surface, proceeding from the promontory and upper half of the second sacral vertebra, spreading over the right sacro-iliac joint, and making an acute angle with the opposite wall of the sacrum at its lower half. The operator perforated the skull, and extracted by the cephalotribe. The patient recovered.

2. M. Blot discusses the question of turning in contraction of the pelvis, and relates one case in which he resorted to this practice. The proceeding has not yet made any ground amongst French obstetricians. He says, they still, for the most part, have recourse to the forceps. His case, however, is valuable as an illustration:—A woman, aged twenty-four, rachitic, pelvis very distorted, was delivered in 1857, by Professor Dubois, by craniotomy and the cephalotribe. In her second pregnancy, she entered the hospital a few days before term. M. Blot lost no time in inducing labour. The cervix was very slow in dilating owing to the contraction of the brim preventing the descent of the child. Some incisions were made, and dilatation went on quickly. Turning was then effected. The passage of the hand required some force. It was found that the right parietal was considerably depressed at the point where it had been opposed to the sacral promontory. Asphyxiated at first, it was restored. At the age of seven months, the mark of the depression could still be traced, but the child had had no fit and was in good health. The antero-posterior diameter of the pelvis was estimated to slightly exceed three inches.

3. M. Pajot describes a new method of embryotomy and of decapitation as follows:—He uses one branch of the forceps. The crotchet which terminates it (M. Pajot speaks, of course, of the French instrument, of which the handle, made of iron, is so constructed) is perforated, and lets a fine cord, known by the common name of whip (*fouet*) pass through. At the summit of the loop formed and held in the canal of the crotchet is a ball of lead. This crotchet being introduced, the leaden ball tends to fall back towards the uterine neck,

drawing the cord with it. A speculum introduced into the vagina to protect it, the surgeon draws upon the two ends of the cord by a sawing-motion. Less than a minute is enough to complete the section of the neck or trunk. In cases in which the child is bulky, where the scapulæ are caught by the cord, the operation may last five minutes.

4. Dr. Pajot, in a memoir read before the Academy of Medicine, relates five cases of extreme contraction of the pelvis. In the first four, which ended fatally, the child was, in three instances, mature. In the fifth case, the fœtus was in the eighth month; the labour was provoked, turning tried, but did not succeed, although the arm was amputated; craniotomy was resorted to. The following conclusions were drawn:

(1.) If the child is alive, and at term, if it present by the trunk in a contraction below six or seven centimetres, turning by external manipulation having been tried for the purpose of facilitating the application of instruments, and found to be impossible, the Cæsarean section is to be considered.

(2.) The fœtus not being at term, turning being found impossible, the amputation of the arm will certainly facilitate the manipulation for version; moreover, the section of the neck will be very easily made by a new process (described above), and the extraction of the fœtus will not present insurmountable difficulties if the fœtus has not much passed the seventh month.

(3.) If the child is dead even at term, whatever difficulties and dangers may be presented in the delivery by the natural passages, the Cæsarean section will be absolutely rejected. After having applied the new method of embryotomy, attempts should be made to crush successively the different parts of the fœtus which are presented at the brim by repeated cephalotripsy.

5. Professor Lazzati, in an excellent memoir, corrects an error widely prevalent, as to the position of the child in what are improperly called transverse presentations. He shows that, in reality, when the arm or shoulder presents, the fœtus too assumes an oblique position in reference to the uterus. During the last three months of gestation, and especially during the eighth and ninth, the development of the fœtus is such that its occipito-coccygeal length exceeds that of the transverse or antero-posterior diameter of the uterus. The fœtal occipito-coccygeal diameter is from $10\frac{1}{2}$ to 12 inches. The longitudinal diameter of the uterus is from $12\frac{1}{3}$ to 13 inches; whilst that of the greatest transverse uterine diameter measures only from 8 to 10 inches; and the transverse diameter of the inferior segment of the uterus is only 7·5 inches, and the transverse diameter of this region does not exceed 8 inches. This being admitted, and every allowance made for the variations which these measurements may undergo, for the flexibility of the body of the fœtus, and for the yielding of the uterine walls, it will remain as a positive fact, that the transverse and antero-posterior diameters of the uterus are shorter than the longitudinal dimension of the fœtus. Hence, during pregnancy, and in labour, the occipito-coccygeal diameter of the fœtus cannot find itself in relation with these diameters of the uterus. The longitudinal diameter of the fœtus must then correspond more or less closely to the longitudinal diameter of the uterus. In a word, it is unavoidable that one or the other extremity of the occipito-coccygeal diameter—that is, the head or the nates—shall be in relation more or less exact with the inferior segment of the uterus, whilst the opposite extremity of the fœtal ovoid shall occupy more or less completely the fundus. From this reasoning the Professor concludes that we must not retain the terms *transverse* position and *transverse* presentations, but consider them as cases of *considerable obliquity* of the fœtus in the uterine cavity. The head in these cases is placed in one or the other iliac fossa, and on the correspond-

ing part of the inferior segment of the uterus; the nates occupy the fundus, which is usually inclined laterally in the direction opposite to that occupied by the head, If it were otherwise, in turning we should have to seek for the feet in one side of the uterus, where, every practical accoucheur knows, they are not to be found. The following are summaries of Professor Lazzati's cases:

Case 1. A patient, aged thirty, had given birth to her first child, after a natural labour; the second child was turned, on account of shoulder-presentation. Much distressed at the death of her two children, and being again pregnant, and fearing another cross-presentation, she was seen by Professor Lazzati when in the ninth month. The fundus uteri was strongly inclined to the right hypochondrium, where the small angular parts of the child and active movements were felt. Auscultation fixed the position of the head between the umbilicus and the pubes. To the left, and below, corresponding to the iliac fossa, was a considerable prominence of the abdominal walls, formed by the child's head. The back was felt directed forwards. Internal examination revealed only a small, angular body, which retreated as soon as touched; no ballottement; the shoulder was distinctly felt by passing the finger through the cervix. The Professor having ascertained that, by applying his right hand upon the head in the left iliac fossa, and his left hand upon the nates in the right hypochondrium, and pressing in opposite directions, he could bring the head over the pelvic brim, had a belt constructed which should answer the same purpose. This apparatus consisted of a pad to pass inwards on the left iliac region, and another to pass inwards on the right hypochondrium, fitted with springs, attached to a support worn on the back. This perfectly succeeded; the head was thus kept constantly over the pelvic brim. Labour set in a fortnight afterwards, his child being delivered naturally by the head.

Case 2. Considerable obliquity of the fœtus *in utero*. The right shoulder in the left position ten days before the end of gestation, without being preceded by uterine contractions. Application of the belt, continuation of the methodical compression during four days. The head of the fœtus was reduced upon the inferior segment of the uterus on the second day, and was maintained there. The belt was removed after four days and left off for three days. The labour was natural and easy, the head presenting in first position, the child being born alive.

6. In this paper Dr. Storer states his claim to originality in the application of fluid pressure to the cervix uteri for the induction of labour. It is overlooked that Dr. Storer's first publication on the subject is quoted by Dr. Barnes. It is admitted that Mr. Jardine Murray's case of acceleration of labour on account of hæmorrhage from placenta prævia "is the first case of the kind put on record." It is contended that Dr. Barnes' instrument is "a nicety that in practical application possesses little or no advantage over the original form." It is also contended that caoutchouc is a bad material for instruments of this kind; and further, that the dilatation should be effected "*from above*." These objections by themselves imply that Dr. Barnes' instruments and procedures are different from Dr. Storer's, and therefore presumably original. The now numerous cases of the application of Dr. Barnes' instruments and method of proceeding, published by himself and others, sufficiently show that his principle of acting upon the entire length of the cervix by an instrument constructed *ad hoc*, differs essentially in certainty and expedition from those methods in which a distensible bag is passed through the cervix into the cavity of the uterus. Dr. Storer relates but one case, and that not fully, in which premature labour was induced by the application of fluid pressure from above. Greater experience under cases of greater emergency and difficulty would no doubt satisfy him of the inefficiency and untrustworthiness of this procedure. M.

Tarnier and several other practitioners in France have lately contrived and used instruments similar to those of Keiller, Storer, and those first constructed and discarded by Barnes; and their action has been found so imperfect as to lead to their hasty disapproval and the neglect of all instruments of this class by some leading obstetric practitioners of Paris.

7. These instruments are various forms of dilating bags; they are introduced *into the uterus*. The Reporter speaks slightingly of them, and in the discussion upon the Report, M. Depaul expressed himself in the same sense. They cannot be depended upon to dilate *the cervix*, for which structure, in fact, they are not adapted. An objection specified was the enormous size of these bags, some being as large as a fœtal head. Certainly, the dilatation of such bags *inside* the cavity of the uterus cannot be free from danger. But to exert any marked dilating effect upon the cervix in this way the bags must be large. This objection does not apply to Dr. Barnes' instruments, which, being adapted directly to the cervix—the structure to be dilated—need not be large, and are indeed of very moderate capacity.

IV. The Puerperal State.

Practical Observations on the Prevention and Cure of Puerperal Inflammation. By Dr. Robert Johns. (Dublin Quarterly Journal of Medical Science, August, 1863.)

Dr. Johns assigns the following as the causes of puerperal fever: 1. Impaired health during pregnancy. 2. Want of cleanliness and of ventilation; hospital influence. 3. Contagion; epidemics. 4. Distress of mind &c.; 5. Errors in diet, and use of stimulants. 6. Hæmorrhage; introduction of hand for version, and retained placenta; portion of secundines retained, or clots putrefied in the uterus. 7. Drawing the breasts by artificial means too soon after delivery, or repelling the milk too suddenly by cold applications of vinegar, &c. 8. Exposure to cold, too early rising, or going out too soon after delivery. 9. Puerperal convulsions, actual or threatened. 10. Uterine diseases. 11. Inhalation of chloroform during labour.

Dr. Johns insists strongly on the efficacy of early administration of calomel and opium, and bismuth. He says that in no case has he known a fatal issue when there were distinct evidences of the system being affected by mercury. Dr. Johns gives a series of cases, in some of which it appears to be assumed that puerperal fever was averted by putting the patients upon calomel and opium immediately after labour. In other cases, symptoms of a threatening character appeared, notwithstanding the previous administration of calomel and opium. The course of argument seems to be, that whenever a lying-in woman has been subjected during labour to one of the eleven causes of puerperal fever enumerated, it may be anticipated that the disease will occur; women in this position have therefore been subjected to the calomel and opium treatment at once, and if fever did not occur, it was assumed that the treatment prevented it. This reasoning is not absolutely convincing; but the cases are interesting.

V. Summary.

The following papers are, on account of the interest attaching to them, referred to by title, but cannot be analysed, for want of space:—

1. On Puerperal Scarlatina. By Dr. Halahan. (Dublin Quar. Journ. of Med. Science, Aug. 1863.) The author gives the history of twenty-five cases.

2. On Fatty and Piliferous Tumours of the Ovaries. By Dr. A. W. Foot. (Dublin Quar. Journ. of Med. Science, Aug. 1863.)

3. Case of Induction of Premature Labour during the Eighth Month of Gestation, with Successful Result to Mother and Child. By William Low, M.D. (Edinb. Med. Journ., Aug. 1863.) In this case, Dr. Barnes' method was pursued so far as the artificial dilatation of the uterus. Labour was completed by action of uterus in twelve hours.

4. Cases of Cephalhæmatoma; puncture; cure. By Dr. Isnard, and Professor Barrallier, Toulon. (L'Union Médicale, July, 1863.)

5. On Congenital Elongation, and on Hypertrophy of the Neck of the Uterus. By M. Jobert. (L'Union Médicale, July, 1863. In this clinical lecture, M. Jobert distinguishes between the congenital elongation of the uterine neck, and the hypertrophy the result of disease. He advocates cauterization by the hot iron, especially for the first condition.

6. Treatment of Phlegmasia Alba Dolens by the Protection of the Skin from the Action of the Air. By Dr. Robert de Latour. (L'Union Médicale, June, 1863.) The author relates a case in which a cure was effected in a very short time by coating the limb with a layer of collodion prepared with castor oil.

7. Case of Oval Obliquity of the Pelvis of Traumatic Origin. By Dr. Laforgue, of Toulouse. (L'Union Médicale, June, 1863.)

8. On Amenorrhœa and Menorrhagic Fever. By Professor Trousseau. (L'Union Médicale, June, 1863.)

9. On the Treatment of Metrorrhagia by Alcoholic Tincture of Aconite. By Dr. Marotte. (Bullet. de Thér., August, 1862.)

10. Plural Birth and Monstrosity; Union at the Vertices of the Heads. By Dr. Belgrave. (Edin. Med. Journ., September, 1863.)

11. On Chloroform in Puerperal Convulsions. By Dr. McNab. (Edin. Med. Journ., Sept. 1863.) Dr. McNab extols chloroform as pre-eminently the remedy in puerperal convulsions. He relates a case in which the convulsions ceased after the delivery of the child by forceps under chloroform.

BOOKS, &c., RECEIVED FOR REVIEW.

A Manual of Ophthalmoscopic Surgery; being a Practical Treatise on the Use of the Ophthalmoscope in Diseases of the Eye. By Jabez Hogg, Senior Assistant-Surgeon to the Royal Westminster Ophthalmic Hospital, &c. &c. Third Edition. Churchill and Sons. 1863. pp. 296.

Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England. By J. Paget, F.R.S., Surgeon Extraordinary to her Majesty, &c. Revised and Edited by W. Turner, M.B. Lond., Senior Demonstrator of Anatomy in the University of Edinburgh. London. Longman and Co. 1863. pp. 848.

Researches on the Development of the Spinal Cord in Man, Mammalia, and Birds. By L. Lockhart Clarke, F.R.S. (From Philosophical Transactions. Part II. 1862.)

On the Normal Position of the Epiglottis as determined by the Laryngoscope. By G. D. Gibb, M.D. (Reprint from Beale's 'Archives,' No. 13, 1863.)

On the Diseases, Injuries, and Malformations of the Rectum and Anus; with Remarks on Habitual Constipation. By T. J. Ashton. Fourth Edition. London, Churchill and Sons. pp. 411.

Catalogue of the Army Washington Museum. Washington. 1863. pp. 58.

Directions concerning the Duties of Medical Purveyors and Medical Storekeepers, and the Manner of Obtaining and Accounting for Medical and Hospital Supplies for the Army. Washington. 1863.

Den almindelige Therapie. Af Dr. og Prof. O. Bang, Conferentsraad, Commandeur af Danebrog og Vasa, Danebrogsmand m.m. Kjöbenhavn. 1862. 8vo, pp. 56.

Series of Questions addressed by Circular to the Medical Officers in the Federal American Army, from the Surgeon-General's Office at Washington, signed W. A. Hammond, with a view to the future recording of their services in the forthcoming Surgical History of the War.

A Treatise on Hygiène, with Special Reference to the Military Service. By W. A. Hammond, M.D., Surgeon-General U.S. Army. Philadelphia, Lippincot & Co. 1863. pp. 604.

Commentary upon Cases of Malarious Intermittent Fever, with reference to the Moon's Influence upon these Fevers. (Pamphlet reprinted from the 'Medical and Physical Transactions of Bombay.')

Report of Clinical Cases treated in Surgical Wards of the Royal Infirmary of Edinburgh, under the care of Mr. Spence, during the Session 1861-62. By F. Steell, M.D.

Case of Amputation at the Hip-joint. By the same. (Reprinted from the 'Edinburgh Medical Journal,' November, 1863.)

On the Question, Is Oxide of Arsenic, long used in a very small quantity, injurious to Man? By John Davy, M.D., F.R.S. (From the 'Edinburgh Philosophical Journal,' July, 1863.)

Case of "Wasting Palsy," by Dr. Thudichum, with an Investigation of the Nervous Centres. By Dr. J. L. Clarke. (Reprint from the 'Boston Med. and Surg. Journal, July 2, 1863.)

On Artificial Dilatation of the Os and Cervix Uteri by Fluid Pressure from above. By H. R. Storer, M.D. Boston. (Reprint from the same.)

A Manual of Zoology. By M. Milne-Edwards; translated by R. Knox, M.D. Second Edition. Edited by C. C. Blake, F.G.S., &c. London, Renshaw. 1863. pp. 564.

A Dictionary of Chemistry and the Allied Branches of other Sciences. Founded on that of the late Dr. Ure. By H. Watts, F.C.S.; assisted by Contributors. Vol. I. London, Longman & Co. 1863. pp. 1137.

Extracts from the Records of the Boston Society for Medical Improvement. By F. Minot, M.D., Secretary. Vol. V. No. 1. Boston, 1863.

Mentone, in its Medical Aspect; being Letters addressed to a Medical Friend. By J. L. Siordet, M.B. Lond. London, Churchill and Sons. 1863. pp. 111.

On the Pyrophosphate of Iron. By E. N. Chapman, M.D. (Reprint from the 'Boston Medical and Surgical Journal.' Vol. LXVI. No. 1.)

On the Treatment of Diphtheria, with Illustrative Cases. By the same. (Reprint from the same.) Vol. LXVIII. No. 1.

Vascular Bronchocele and Exophthalmos. By J. Warburton Begbie, M.D. Edinburgh, 1863. (Pamphlet, reprint from the 'Edinburgh Medical Journal,' Sept. 1863.)

On the Origin of the Vaccine Inoculation. By E. Jenner, M.D., F.R.S., &c. London, 1801. (Pamphlet, reprinted by E. Elfick, 1863, from an original copy in the possession of J. B. Curgenven, Esq.)

Braithwaite's Retrospect of Medicine, January—June, 1863. pp. 423.

A Guide to the Qualitative and Quantitative Analysis of the Urine, designed especially for the use of Medical Men. By Dr. C. Neubauer and Dr. J. Vogel. Fourth Edition. Translated by W. O. Markham, F.R.C.S., Physician to St. Mary's Hospital. The New Sydenham Society. London, 1863. pp. 439.

Du Strabisme et de la Strabotomie, &c. Par E. Meyer. Paris, Baillière et Fils. 1863. pp. 112.

D'Asthme, sa Nature, ses Complications, &c. Par C. J. Berger, M.D. Paris, Baillière et Fils. 1863. pp. 196.

Estudos sobre o Garrotilho ou Crup. Memoria apresentada á Academia real das Sciencias de Lisboa. Por A. M. Barbosa. Lisboa, 1861. pp. 189.

On the Weight and Dimensions of the Heart in Health and Disease. By Thomas B. Peacock, M.D., F.R.C.P., Physician to St. Thomas's Hospital. (Reprint from the 'Monthly Journal of Medical Science,' 1854.)

Reports, Journals, &c.

The Dublin Quarterly Journal of Medical Science. August, 1863.

Edinburgh Medical Journal. July, Aug., September, 1863.

The Journal of Mental Science. Edited by C. L. Robertson, M.B. July, 1863.

Seventeenth Report of the Commissioners in Lunacy to the Lord Chancellor. 1863.

Third Annual Report of the Belfast District Hospital for the Insane Poor. 1863.

Medical Report of the Royal Lunatic Asylum of Aberdeen. 1863.

The American Journal of the Medical Sciences. July, 1863.

The Medical Record of Australia. March, April, 1863.

The Australian Medical Journal. Melbourne, July, 1863.

The Medical and Surgical Review (Australasian). Melbourne, July, 1863.

The Thirty-fourth Annual Report of the General Hospital and Dispensary for Sick Children. Manchester, 1863.

Canada Lancet. Montreal, July 15th 1863.

Norsk Magazin for Lægevidenskaber Anden Række. XVII. Bind, 4, 5, 6, Hefte. Christiania, 1863.

NOTICE TO READERS.

THE Editor is particularly desirous of having all Reports of Hospitals, Asylum Sanitary Boards, Scientific Societies, &c., forwarded to him; as also Inaugural Lectures, Dissertations for Theses, Medical and Scientific Addresses, &c.

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