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PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *Des Inflammations Spéciales du Tissu Muqueux, et en particulier de la Diphthérite.* Par P. Bretonneau, Médecin-en-chef de l'Hôpital de Tours.—*Paris*, 1826. pp. 540.

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2. *Addition Supplémentaire au Traité de la Diphthérite.* Par P. Bretonneau, &c.—*Paris*, 1827.

Supplement to the Treatise on Diphtheritis.

3. *Memoirs on Diphtheria;* from the Writings of Bretonneau, Guersant, Trousseau, Bouchut, Empis, and Daviot. Selected and translated by Robert Hunter Semple, M.D. With a Bibliographical Appendix by John Chatto, Librarian to the Royal College of Surgeons. (The New Sydenham Society.)—*London*, 1859. pp. 407.

4. *Clinical Lectures on Angina,* delivered in the Hôtel-Dieu by Prof. Trousseau. ('Gazette des Hôpitaux.'—1855.)

5. *Rapports Généraux sur les Maladies qui ont régné en France pendant les années* 1852–1856. ('Mémoires de l'Académie de Médecine.'—*Paris*, 1853–57.)

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8. *Observations on Diphtheritis.* By WILLOUGHBY F. WADE, B.A., M.B., T.C.D., Physician to the General Hospital, Birmingham.—*London*, 1858. pp. 32.

9. *On Diphtheria; its History, Progress, Symptoms, Treatment, and Prevention.* By ERNEST HART, Surgeon to the West London Hospital. (Reprinted from the 'Lancet.')—*London*, 1859. pp. 36.

THERE are a few names in the history of medicine which are inseparably connected with the diseases which their possessors have been the first to discover or to distinguish. It is in this way that the name of the eminent physician who has within the last year passed from among us is most widely known and will be longest remembered; and it is thus that for years past we have remembered the names of Hope, Rayer, Laennec, Corvisart, and many others who have been the first to break ground each in his own special field of pathology. Like honour is due to M. Bretonneau, of Tours, for his admirable investigation of the epidemic disease to which he has attached the name Diphtheritis.

Profoundly impressed with the truth of the aphorism of Laennec, that diseases can only be certainly distinguished by their anatomical characters, M. Bretonneau based his inquiry exclusively on post-mortem investigations, and prosecuted this line of research so far as to arrive at the conclusion with respect to several important diseases previously supposed to have no relation to one another, that they are connected either by identity in their accompanying anatomical appearances, or by so complete a similarity that they may be considered as phases of the same morbid process. An epidemic of malignant sore-throat which occurred at Tours in the year 1818, was the occasion of his earliest observations. His first memoir is mainly devoted to the consideration of the affections which had previously been described in France as epidemic croup, or malignant angina. The second contains in one section a careful description of the epidemic above mentioned, which is followed by a historical summary of previous outbreaks; while two others are devoted to the communicability of the disease by contagion, and to its treatment. In 1825 and 1826 the epidemic again broke out in the neighbourhood of Tours, at La Ferrière and Chenusson, and formed the subject of new researches. These four papers, with various appendices, constitute the treatise on Diphthérite, the first edition of which appeared in 1826.

The word Diphtheritis,* first employed by Bretonneau in this form,

[* We have no wish to enter upon a discussion as to the merits of the two words "diphtheritis" and "diphtheria," but willingly adopt the latter as being the shorter of the two, and in deference to the Registrar-General. For those of our readers who are interested in etymological research, we may state that διφθέρα is the prepared skin of an animal; διφθερίτης, that which is covered with a fur or with a leathern coat; διφθερίας has the same signification as the latter, διφθερίς as the former of these words.—ED.]

and years afterwards modified to its present form, Diphtheria, in deference, as it would appear, to the objections of critics, involves in itself the kernel of his doctrine, the dominant idea of his whole writings,—*the specificity* (to adopt a French word) *of the pellicular exudation.* Diphtheria, according to Bretonneau, is a diseased condition *sui generis* which may have its seat in the mouth, the fauces, the larynx, or on the blistered surface of the skin; and in all of these situations has the same specific characters. Its specificity consists anatomically in the formation of a pellicle of definite structure—pathologically or dynamically in the fact that this pellicle has the power of reproducing itself. Nothing is diphtheria that has not a pellicular exudation; no such exudation is diphtherical which is not capable of acting as a virus or contagium. The pellicular exudation is anatomically identical in all situations, but the disease is to be distinguished not merely by its anatomical characters, but by the additional fact that the exudation is the result or effect of the application of a specific virus to the affected surface, and is in itself capable of similarly affecting other surfaces.

We have thus as succinctly as possible stated our author's doctrine; we will now place before the reader some passages in which it is laid down more at length in his own words. It is a serious inconvenience attending the arrangement of the "Treatise," that as each separate memoir is complete in itself, almost every question is discussed three or four times, and it is therefore necessary to refer to as many passages in order to arrive with certainty at the author's meaning. After observing (p. 20) that the organic alteration of which the pellicular exudation is the product, consists simply of redness of the mucous membrane without swelling or any other change whatever, he proceeds—

"I should not express my whole thought did I not add, that in this pellicular inflammation I see a specific phlegmasia as different from a catarrhal phlogosis as malignant pustule is from zona,—a disease more distinct from scarlatinous angina than scarlatina itself is from small-pox,—a morbid affection *sui generis*, which is no more the extreme degree of catarrh than psoriasis is the extreme degree of erysipelas. In the impossibility of applying to a special inflammation so distinct as this is, any of the unsuitable names which have been given to its several phases, I may be permitted to designate this phlegmasia by the denomination of Diphthérite," &c.

With the view of determining the value of the anatomical fact of pellicular exudation as characteristic of diphtheria, M. Bretonneau made numerous experiments relating to the effects produced by various irritant substances when applied to the mucous surfaces. He found that none of these agents was capable of producing a similar exudation, excepting cantharides.

"The action of oil of cantharides,* when applied to the surface of the tongue and lips, is almost instantaneous. . . . . . . In less than twenty minutes the epidermis shrivels, and becomes raised and detached. It is soon replaced by a concrete pellicle, at first thin and semi-transparent, which speedily becomes more opaque and thicker. Like the diphtheritic exudation, this membrane,

* Ethereal extract of cantharides dissolved in olive oil.—M. Bretonneau.

which is at first slightly adherent, is detached and reproduced with great readiness. Within a period of six or seven days it may be several times renewed."*

From these experiments M. Bretonneau concludes that the pellicle of cantharides is anatomically identical with that of diphtheria, and is consequently compelled to admit, in spite of the dictum of Laennec already quoted, that diseases cannot be distinguished merely by their anatomical characters.

"The facts relating to the cantharidic inflammation do not in the slightest degree weaken the *specificity of diphtheria;* on the contrary, if we consider them in their true light and in their complete development, they prove it experimentally and demonstratively. However close may be the resemblance between the two forms of inflammation, they are distinguished by well-marked characters. The cantharidic inflammation is limited to the surfaces which have been subject to the inflaming action of the vesicant, and soon becomes extinct; which it is in the nature of the diphtheritic inflammation to extend and persist."†

This sentence involves the second part of Bretonneau's notion of diphtheria—the special virulence of the diphtheritic exudation; which, however, is much more fully developed in his recent paper published in the 'Archives Générales de Médecine,' 1855. From this work many passages might be quoted to show that it is the opinion of the author not merely that the disease may be propagated by the application of the secretion from an affected surface to sound parts, after the manner of small-pox, but that, like syphilis, diphtheria is capable of communication from a diseased to a healthy person *by no other channel.*

"Innumerable facts have proved that those who attend patients cannot contract diphtheria, unless the diphtheritic secretion in the liquid or pulverulent state is placed in contact with the mucous membrane, or with the skin on a point denuded of epidermis; and this application must be immediate." (Dr. Semple's Transl. p. 176.)

"The Egyptian disease ‡ is not communicated by volatile invisible emanations, susceptible of being dissolved in the air and of acting at a great distance from their point of origin. It no more possesses this quality than the syphilitic disease. If the liquid which issues from an Egyptian chancre as visibly as that which proceeds from a venereal chancre, has seemed, under certain circumstances, to act like some volatile forms of virus, the mistake has arisen from its not having been studied with sufficient attention. The appearance has been taken for the reality." (Dr. Semple's Transl. p. 184.)

Having thus arrived at a clear understanding of Bretonneau's theory, we may proceed to inquire how far it is rendered necessary by facts. Diphtheria is the *liaison* by which our author connects several affections which in the prevailing nosology of this country are separated from each other by wide intervals.

* Traité de la Diphthérite, p. 356. This passage is omitted in the translation.

† Ibid., p. 367.

‡ This term is used by Bretonneau on the supposition of the identity of diphtheria with the disease described by Aretæus as Αἰγύπτια ἕλκεα. Aretæus, De Causis et Signis ac. Morb. (Ed. Kühn) p. 19.

"To prove that croup is but the extreme degree of malignant angina; that malignant or gangrenous angina is not gangrenous; that there is no relation between sphacelus—between a mortification, however superficial it may be, and the alterations which this disease leaves behind it" (Dr. Semple's Transl. p. 6),

this is what Bretonneau professes to have accomplished: so that according to him diphtheria includes and unites croup and angina, but excludes and separates from the latter disease gangrenous sore-throat or gangrene of the fauces. We shall endeavour to make it plain to the reader that the first two of these diseases can only be regarded as identical, if we assign the word croup a meaning which does not belong to it, and that it is impossible to describe malignant angina in any terms which shall not include sloughing of the sub-mucous tissue as an important though not essential element.

"Croup is but the extreme degree of malignant angina."—Such a doctrine accords so ill with the classical conception of croup which established itself in the mind of each of us in his first course of the practice of medicine, or on his first reading of Dr. Watson's graphic chapter on cynanche trachealis, that we are compelled to assume as the only possible explanation that our author uses the term in an entirely different sense.

Dr. Francis Home, father of the late professor of medicine in Edinburgh, in a shilling pamphlet published there in the year 1765, first introduced into medical literature the word croup, which had been previously in popular use to designate a sporadic disease frequently fatal by suffocation. Although the works of previous authors from Hippocrates downwards* contain descriptions in which we cannot fail to recognise it, yet it is clear that to Home belongs the honour of for the first time defining its characters with sufficient precision to distinguish it from other germane affections. He first pointed out that the existence or formation of a false membrane in the larynx is essential to the disease, and is the source of danger; and that all its invariable symptoms are immediately dependent on this condition. Among these he drew special attention to the stridulous quality of the voice, which he compared to the crowing of a young cock, and considered to be "the true diagnostic sign of croup." He also described its insidious invasion and progress, its alternating accessions and remissions, and dwelt upon the frequent absence of all alarming symptoms, except those directly consequent on narrowing of the glottis. He regarded it as an inflammatory disease, capable of being successfully treated only by bleeding, leeches, and purgatives.†

Home's description, which was derived from the careful observation of twelve cases, in ten of which post-mortem examinations were made, is the foundation on which the modern doctrine of croup was built. The conclusions of Home were not only accepted by the physicians of Great Britain, but by Rosen in Sweden, and Michaelis in Germany. They seem, however, soon to have become obscured by the prevalent

* See the remarkable passage in the Prognosticum. Hippocrates (Ed. Littré), t. ii. p. 175.

† Inquiry into the Nature, Cause, and Cure of the Croup (Edinb. 1765), p. 32.

dogmatism of the time, or overlaid by a mass of literature consisting of partial records of observations in which the Hippocratic method of relating all the facts observed was abandoned, and those only were described which seemed to favour the views of the writer. Much of this obscurity was cleared by the appearance of the works of Cheyne in this country,* and of Vieusseux,† Jurine,‡ and Albers§ abroad, and particularly by the report drawn up by Royer-Collard.||

All of these were the rich fruits of the celebrated *Concours* instituted by the first Napoleon in consequence of the sudden death of the infant son of Louis Bonaparte and Queen Hortense. The article "Croup," in the 'Dictionnaire des Sciences Médicales' (1812–22), may be considered as a *résumé* of all that was original in the essays presented for competition. In this essay the ideas of Home are but reproduced and enlarged; the details of the outline which the genius of the Scotch physician enabled him to sketch from so limited materials, are filled in by the author, M. Royer-Collard, from the inconceivably vast records of observation placed at his disposal as secretary of the commission. It is the leading idea of these writers that croup is an acute inflammation of the mucous membrane of the air passages, distinguished from others by the rapidity of its progress, by the existence of concrete exudation in the larynx, and by the fact that it principally attacks children under ten years of age. They regard cold and moisture as its main causes, and support this inference by all that is known as to the seasons during which the disease is most apt to occur, and the climates in which it is most prevalent; and they hold that it is its habit to select for its invasion single individuals in large populations, without communicating itself to the rest—in other words, that it is apt to be sporadic, not epidemic.

Thus we see that Dr. Home's definition of croup had received the sanction of the highest authorities in France a short time before the publication of Bretonneau's first paper. Notwithstanding this, he so entirely undervalues the labours of our countryman as to express his astonishment that

> "A work containing only a small number of isolated and inconsistent (*disparates*) facts, had effaced the traces of the ancient traditions, and had exercised so great an influence for half a century on the opinion of practitioners." (Dr. Semple's Transl. p. 4.)

Strange, indeed, if it were true; but we have already seen that Home's facts were not inconsistent; and that they were not isolated is evident from the general endorsement which has been accorded to them by a crowd of careful and honest observers. There is such a disease as Home described, and after him Cullen and Cheyne—a disease which is sthenic in character, sporadic as regards its origin, and is best treated by antiphlogistics. Bretonneau's laryngeal diphtheria,

* Essays on the Diseases of Children (Essay I.) Edinb. 1801.
† Mém. sur le Croup. Geneva, 1812.
‡ Mém. sur l'Angine de Poitrine. Paris, 1815.
§ Commentatio de Tracheitide Infant. Leipzig, 1816.
|| Rapport adressé à S. E. le Ministre de l'Intérieur, sur les Ouvrages envoyés au Concours sur le Croup. Paris, 1812.

miscalled croup, although it closely resembles Home's disease anatomically, differs completely in its dynamical characters; it is contagious and epidemic; its exudation is an active virus, which produces in the subject of it a marked dyscrasia.

> "What real identity," says Dr. Cheyne,* "can there be between two diseases, the one caused by cold, the other by contagion; the one always purely inflammatory, the other generically typhoid; the one requiring a decidedly antiphlogistic treatment, the other local stimulants of the most powerful kind, tonic medicines and cordials, bark and wine, according to the procedure of our forefathers?"

Having thus disposed of croup by removing it entirely beyond the limits of the discussion, we have only to deal with malignant angina, which term we understand to include all those truculent varieties of sore-throat which, in various places and at various times, have prevailed epidemically, especially among children. Is Bretonneau's diphtheria, the main features of which we have traced under his guidance, a faithful representation of this disease? The question is one of considerable extent, and could not be answered within the limits of a review. We can only endeavour to point out some of the more prominent difficulties which present themselves. Successive, and even synchronous, epidemics of sore-throat exhibit very marked differences in their characteristic symptoms and dangers, and have been frequently regarded as different diseases. Bretonneau established their identity on the firm basis of an historical comparison of the descriptions of former times with his own more exact observations; and we conceive this to be one of his most important achievements. The identity of all forms of epidemic sore-throat being admitted, it still remains, however, to be shown what, in the midst of so much variety, constitute its invariable characters. These Bretonneau has endeavoured to generalize in his conception of diphtheria; we shall see, as we proceed, that in one or two respects that generalization is defective.

Malignant angina has at all times exhibited a marked tendency to become epidemic; it has presented itself in this form in all the countries of Europe,—at the end of the sixteenth century, in Spain; in the seventeenth, in Italy; and in the eighteenth, after a long interval, simultaneously in England, France, Sweden, Germany, and the United States. Its outbreaks are usually limited in extent, sometimes not spreading beyond a single building, a single village, or quarter of a large town. The experience of ancient and modern times agrees in teaching us that neither in the origin nor during the progress of an epidemic is the invasion of the disease determined by personal communication. These facts are not only not explained, but absolutely excluded, by the theory of a fixed diphtheritic virus. If malignant angina is communicable in no other way than syphilis, why does it spread in any other way? If its material cause be concrete, and cannot under any circumstances become volatile, how can you account for its diffusion at all?—a diffusion which, we learn from our own expe-

* Cyclop. of Pract. Med., vol. i. p. 494.

rience during the last three years in this country, no less than the history of former epidemics, may be so complete as to affect every family residing within the limits of the epidemic area. And even if this objection were overcome, and it were admitted that this may be accounted for by mere inoculation, how does it happen that its prevalence is so limited, both as regards time and space, that one village or hamlet only is invaded, while others similarly situated, and in constant communication with the first, remain exempt; that it breaks out unexpectedly, remains for a few months, and disappears as if exhausted? These considerations are sufficient to show that the virus theory only needs to be confronted with facts, and we at once see that it is untenable. The agency of a concrete contagium cannot possibly explain the epidemic prevalence of any disease, and it is particularly inapplicable to malignant angina.

As our space will not allow us to inquire, even in a cursory manner, how far the general characters of epidemic sore-throat—those of them which are common to all of its varieties—are fairly represented by diphtheria, we shall confine ourselves to one or two points which seem to be of special importance, namely, the constitutional state which accompanies diphtheria, and the consequent anatomical conditions of the mucous membrane. We shall find no difficulty as regards either of these in ascertaining our author's opinion. It is true that in the 'Traité de la Diphthérite,' he tells us but very little of the constitutional symptoms, the reason being that at the date of its publication he had not observed them, and for many years afterwards did not attribute to them any importance.

> "At the onset of diphtheria, the organic functions and those which belong to the life of relation are so little disturbed, that in general children who are already dangerously affected by malignant angina, retain their habitual appetite and continue their play. . . . . . The disease only becomes mortal when the membranous layers which line the interior of the air-passages form, by their accumulation or their adherence, a mechanical obstacle to respiration. . . . . . If a topical treatment modifies the diphtheritic inflammation, the return to health follows immediately on the cessation of the local disease."*

But in the recent paper in the 'Archives' already quoted, M. Bretonneau is compelled to modify his former views. In the epidemics which have occurred during the last few years in Paris, the disease has assumed so insidious a form, and hastened to its fatal termination in a manner so unaccountable according to the theory above expressed, that he is led to admit a new element of danger—a toxæmia, as he calls it, to which the peculiar adynamia of diphtheria is to be attributed; but he is still determined, at any cost, to keep true to the theory, that diphtheria is essentially a local disease. Still maintaining that the constitutional phenomena of diphtheria are secondary, and therefore incapable of existing independently of the local changes, he supports his position by an assumption of facts, and endeavours to make it appear that whenever diphtheria assumes a suddenly fatal form, when-

* Addition Supplémentaire au Traité de la Diphth., pp. 25–27.

ever the constitutional appear to precede the local changes, the explanation is to be found, not in the antecedence of a morbid diathesis, but in the secret development of diphtheria in the nostrils! If we inquire what are the facts on which this assumption is founded, we find nothing more substantial than the occasional occurrence of cases in which coryza and glandular swelling have preceded for some days the more serious symptoms. Although it cannot fail to excite our surprise that so capable and careful an observer should content himself with reasoning which appears so frivolous, it must be admitted that a disease so variable in its character as malignant angina is extremely embarrassing to the nosologist. There can be no doubt that in the epidemics on the banks of the Loire, the characters of the disease were as Bretonneau described them, but in Paris during the last half-dozen years, and in the departments bordering the English Channel, especially at Boulogne, it has certainly assumed a different aspect, and been attended with new dangers. Trousseau—the great friend of Bretonneau, of whom he speaks in the paper before us with the strongest expressions of affection, and to whose advocacy his doctrines owe much of their acceptance,—Trousseau has unreservedly admitted this change of type of diphtheria; and as he is almost as much identified with Bretonneau's doctrines as if they were his own, his testimony may be received, so far as it bears against them, as unexceptionable.

"There is a form of diphtheria to which for seven or eight years past innumerable victims have succumbed," . . . . which "differs so completely from all others in the general aspect of its symptoms, that one would be tempted to establish a line of demarcation; but in directing our attention to its mode of invasion and etiology, we have no difficulty in recognising conformity, and even identity, the difference being that the diphtheritic disease assumes a character of exceptional gravity, and kills at once by the constitutional affection, without the participation of the larynx. Usually the sore-throat seems to be the first symptom, ... but sometimes it is preceded by a coryza of great severity, as if the pituitary membrane had been attacked before the fauces."

Along with this "there is a swelling of the lymphatic ganglia of the neck, which is sometimes so enormous as to extend beyond the jaw," and is altogether out of proportion to the intensity of the faucial affection.

"Join to this acute pain in the head, extremely intense fever (excessive frequency of the pulse), and you will have the signs of the onset of the worst forms of diphtheria. Some hours after, you will observe false membrane on the uvula and velum, ... the discharge from the nose becomes fetid, and if you open the nares with an ear speculum, false membranes are observed on the septum and turbinated bones. . . . . The patient does not sleep, and is in a state of extreme agitation; the breathing is stertorous and snoring. . . . . . After thirty-six or forty-eight hours the features assume a livid pallor, delirium follows, and the unfortunate patient dies with all the appearances of anæmia, and in a state of somnolent tranquillity, which strongly contrasts with the agitation that distinguishes the agony of croup. It is impossible to describe the horrible prostration, the powerless exhaustion, the frequent faintings, in one of which the thread of life is oftened severed."*

* Gazette des Hôpitaux, 1855, p. 397.

If it were necessary to proceed further in order to prove that M. Bretonneau, in taking as universal the type of epidemic sore-throat observed by him at Tours, was mistaken so far as relates to the absence of constitutional symptoms, we should only have to refer to M. Isambert's account of the epidemic in Paris in 1856, to M. Perrochaud's report on that of Boulogne, or to the recent experience of our own country.

We have next to inquire how far the anatomical conditions of the mucous membrane of the fauces assigned to diphtheria are common to all the forms of malignant angina. The absence of all possible relation between diphtheria and gangrene of the fauces is one of the points most prominently insisted upon by Bretonneau, who considers it characteristic of the affection that the mucous membrane remains entire and unaltered throughout. Malignant angina, he observes, is unaccompanied with any sloughing, and a contrary opinion could only arise from deceptive appearances, for "in none of the cases at Tours, even when malignant angina had assumed the most repulsive aspect, could anything be discovered which resembled a gangrenous lesion." The testimony of history, so far as it can be considered of any value in reference to anatomical questions, the author finds to be entirely in his favour.

"The results of the analysis of historical testimony do not differ in any respect from those which my own direct observations furnish me. . . . . As one has no difficulty in recognising in the most incorrect drawing a familiar object, so one is able to seize without difficulty, after having first studied them from nature, the main features of diphtheria in the descriptions which have come down to us, at whatever epoch, and under whatever name its ravages have been detailed." (Dr. Semple's Transl. p. 27.)

So far as we may conclude from the historical summary contained in the second memoir of the epidemics of the sixteenth and seventeenth centuries, it would appear that these agreed in their characters with diphtheria. This may be also admitted as regards some of the epidemics of the last century. In others, however, the appearances of ulceration and sloughing were so minutely described, and seemed to be so constantly present, that it is extremely difficult, if not impossible, to admit that the observers were mistaken. However this may be, we have much better evidence for the settlement of the question in the results of modern times, derived from researches conducted with a special view to its solution. In some of the more recent French epidemics to which reference has been already made, gangrene has occurred not as a mere accident, but as the expected termination of all the most malignant cases. In 1841, M. Becquerel published an accurate description of one of these epidemics, in which gangrenous sore-throat prevailed at the same time with cases presenting the typical characters of diphtheria. The two forms of disease were indistinguishable as regards their origin and mode of attack, the local affection not being preceded by fever or any other constitutional symptoms. The appearances of the fauces were in all cases in the first instance purely diphtheritic. In those which in their progress assumed the septic aspect, the pellicular exudation became friable, and soon separated from the mucous surface; this when first seen was usually entire, but exhibited the appearance and colour of a limited eschar, which was finally thrown off, leaving a deep excavation. In several instances this resulted in sudden and fatal hæmorrhage. The constitutional state preceding death was that which usually accompanies gangrene. The Hippocratic countenance, the sensible diminution of temperature, the rapid and almost imperceptible pulse, the frequent vomiting, and involuntary relaxed stools, together with the restless agitation of the patient, constitute an aggregate of symptoms characteristic of the septic intoxication. These cases were as a rule fatal, and often terminated in this manner in spite of manifest improvement in the local condition.

If the fact of the existence of gangrene had been assumed merely from the appearance of the fauces during life, we might have imagined that a mistake in observation had been made; but this we think is inadmissible in presence of the careful dissections on record. It was found that in fifteen cases examined after death there was gangrene affecting the tonsils exclusively in nine, the pharynx and pillars of the velum in the remaining six. In the tonsil the gangrene was either central or near the surface. In either case the resulting cavity was irregular in form, contained a sanious fetid liquid, and was surrounded by softening of the sub-mucous tissue, which was more or less converted into greenish-grey detritus. It appeared that the disintegration always commenced beneath the mucous membrance, which at first, merely swollen and rugous, gradually assumed a gangrenous appearance and colour, and was converted into an eschar; this, unless the process was anticipated by death, finally separated, leaving the cavity exposed.

The history of Becquerel's epidemic illustrates in a remarkable manner the fact, that although the septic type of malignant angina differs as much from the purely membranous in its symptoms as in its anatomy, yet it occurs under the same epidemic influence, and, as it were, interchangeably with it. The epidemics which have prevailed since 1841 in Paris and the northern departments of France, have in general conformed more closely to the former than to the latter. Such was the well-known epidemic at Boulogne in 1855, which in a mean mortality of 1000 annually, proved fatal in fourteen months to 366 persons, 341 of whom were children under ten; and such were several others of equal fatality, though not of so long duration, which are recorded in the Reports of the Academy of Medicine.

In the Paris epidemics of 1855 and 1856, M. Isambert distinguishes both forms of diphtheritic angina; the one tending to prove fatal by extension to the larynx, the other, which he designates as angina diphtheritica maligna, having totally different characters. In this disease "the membranous exudation, soon after its appearance, softens, and assumes a dirty grey or blackish colour; the uncovered mucous surface is livid, the adenitic swelling is enormous, and affects not only the glands themselves, but the cellular tissue, the skin often sloughing from excessive tension." Death is preceded by a gradually increasing prostration, not accompanied by any more marked nervous symptoms than those described by M. Becquerel.

If we extend our inquiry to the epidemics of malignant sore-throat which have during the last few years prevailed in this country, we shall find that they differ as widely from Bretonneau's model as those we have been considering. Our readers are well acquainted with the so-called sore-throat, with ulcers, described by Dr. Huxham as having been prevalent somewhat more than a century ago in Liskeard, St. Austle, and other towns in Cornwall. It is not a little remarkable that the same neighbourhood has been subject during the last three years to a similar affection, which seems to resemble closely that described by M. Becquerel, in the fact that membranous exudation of unusual thickness and tenacity is associated with softening and disintegration of the sub-mucous tissue. This epidemic has been well though shortly described by Mr. Thompson, of Launceston, in the 'British Medical Journal,' and we have had ourselves the opportunity of studying it recently. In the milder cases the only local changes observed were redness and swelling of the tonsils, with superficial ulceration; in others the affected parts were covered with exudation, on the separation of which the mucous surface remained entire. In a third class of cases both elements were combined, tough, leathery exudation covering the tonsils, velum, and uvula to a variable extent, and co-existing sometimes with central softening of the tonsil, its surface remaining intact, sometimes with complete destruction of the gland and of the corresponding pillar of the velum. The situation and extent of the loss of substance were various; it may be generally stated, however, that it was confined to the tonsils, velum, and faucial arches, the destruction of which parts was in one instance so complete as to leave a permanent communication between the buccal and naso-pharyngeal cavities, the whole of the right pillar of the velum having disappeared, excepting its margin, which stretched across, downwards and backwards, from the uvula towards the epiglottis. In those instances in which the softening was superficial, the mucous membrane was usually found, on the separation of the exudation, to be converted into an ash-coloured eschar of limited extent, and bordered with deep violet; this subsequently separating and leaving a slowly healing ulcer. Corresponding with these varieties in the anatomical characters of the disease, various modes of fatal termination were observed. In by far the greater number of cases, death was consequent on extension of the exudation to the larynx, and was preceded by the symptoms of croup. In the rest of the cases, the fatal termination was either caused by hæmorrhage from the fauces, by unexpected syncope after apparent recovery, or by the aggregate of symptoms which we have already referred to as described by M. Becquerel.

It can scarcely fail to strike the reader that the affection under consideration would be just as correctly designated by the term "sore-throat, with ulcers," employed by Huxham and Fothergill, as by that of diphtheria; a fact which appears the more remarkable when we consider that the very towns in which Huxham's disease most prevailed in 1748–50, have been most severely visited during the last few years. Are these two epidemics, separated by an interval of more than a century, of the same nature? A careful comparison of their symptoms assures us that they are, and that Bretonneau, in disclaiming all relationship between his diphtheria and the "sore-throat, with ulcers," was mistaken. Respecting the old epidemic, we have two sources of information which require to be combined in order to arrive at a correct knowledge of its nature. The attention of Huxham was mainly directed to the condition of the fauces, and he very imperfectly appreciated the tendency of the disease to extend to the air-passages. Yet we have distinct indications that his cases must frequently have terminated by laryngeal symptoms. "Not only," says he, "were the nostrils, fauces, &c., affected, but the windpipe itself was sometimes much corroded, and pieces of its internal membrane were spit up." And as regards the mode of death, either "the patients lingered for a considerable time and then died tabid," or "it fell more suddenly and violently on the lungs, and killed in a peripneumonic manner."*

What is wanting in Huxham's description is supplied by the remarkable paper of Dr. Starr, of Liskeard, published in the 'Philosophical Transactions.' We have here, besides other details of the epidemic, a complete observation of a case in which the pellicular exudation commencing in the fauces, extended to the larynx. There is scarcely a feature of Bretonneau's diphtheria unrecorded; nothing is omitted as regards the physical properties of the exudation, its adherence to the subjacent surface, its repeated detachment and reproduction. The usual symptoms of extension to the larynx appeared the eighth day after the first illness of the child, their nature being proved by the expectoration of a tubular cast of the trachea and larger bronchi, which Dr. Starr imagined to be the shed mucous membrane, but the appearance of which he has perpetuated in a good engraving.†

The characters of the Cornwall epidemics are not those which have been most common in this country. Usually the fatal result has been preceded neither by the symptoms of croup nor by those of septic poisoning. The patient falls into a condition of tranquil drowsiness, which continues for some hours before death. That this state of stupor is more apparent than real is proved by the observation, that on being roused from it he is capable of performing actions which from their nature imply the integrity of all the nervous functions.

At this point our space compels us to bid farewell to M. Bretonneau. It will be clear to our readers that the treatise on Diphthérite is a nutshell in which a valuable kernel of truth lies concealed, although somewhat difficult to extract. That kernel we conceive to consist in the two facts, that all the various forms of epidemic sore-throat which have prevailed at different times and in different places are identical,

* Huxham. A Dissertation on the Malignant Ulcerous Sore-throat. London, 1757. pp. 24, 25.

† An Account of the Morbus Strangulatorius Epidemic at Liskeard, in 1749. Phil. Trans. 1750.

and that the common character by which this identity may be recognised is the existence of the pellicular exudation. We have endeavoured to show, that so far from inoculation being the only mode of diffusion of diphtheria, there is no sufficient ground for believing in the existence of a concrete diphtheritic virus—that so far from laryngeal obstruction being the only cause of death, it does not even appear to be the most frequent; that in some epidemics ulceration and sloughing are not merely occasional consequences, but essential elements of the local affection; and that there exist two types of diphtheria, which may be properly distinguished as septic and membranous, the one of which in its most marked expression is characterized by the tendency to rapid extension and repeated reproduction of the exudation, by the integrity of the subjacent mucous membrane, and the absence of all constitutional symptoms; the other, locally, by the softening and disintegration of the sub-mucous tissue, constitutionally, by all the symptoms of the septic intoxication. These two forms are capable of being combined in all possible proportions. So far as the facts which have been collected enable us to judge, there is no recorded instance of a fatal epidemic of angina in which membranous exudation has not been observed; but there has been every possible variation as regards its extent and the degree of its development. Epidemics have occurred in which even in rapidly fatal cases the whole area covered has not exceeded that of a sixpence; while in others, it has not only extended from the fauces to the nares, larynx, and trachea, but has affected the genital mucous membrane and the skin.

The "Treatise on Diphthérite" (including under that title in addition to the original work, the author's paper which appeared three years ago in the 'Archives Générales de Médecine,' on the means to be taken for the prevention of the disease), constitutes the greater part of the volume recently published by the New Sydenham Society. Of the remaining memoirs, each contains much valuable material; at the same time it would perhaps be possible in the midst of such an *embarras de richesses* as exists in the literature of diphtheria, to make a better selection. Thus, for example, we should have perferred Trousseau's lectures delivered in 1855 to his articles in the 'Dictionnaire de Médecine' published in 1835; as unquestionably the conclusions of an author on a scientific subject can be best judged of by his most recent writings.

M. Guersant's article on the same subject has influenced the progress of investigation in so important a manner, that it could not have been dispensed with in a collection of papers on diphtheria. The author uses the word croup only in the anatomical sense, that is, as equivalent to laryngeal diphtheria, and does not refer at all to the disease to which, as we have endeavoured to show, it is alone applicable—the croup of Home, Cullen, and Cheyne. A similar importance attaches to the third of the selections from the writings of M. Trousseau, the article on cutaneous diphtheria, published in 1830. His observations prove incontestably, "that the diphtheritic affections of the skin are of a nature identical with those which have their seat in the mucous membrane of the larynx and fauces." The paper contains an account of one of those epidemics, of repeated occurrence in France, in which the cutaneous affection has been so frequent as to become the prominent characteristic of the disease. The application of a blister to a patient affected with malignant angina, was certain to be followed by pellicular exudation on the vesicated surface; and in the localities and families in which such cases existed, even the most trifling excoriations assumed the diphtheritic aspect—the same causes which produced the faucial disease in one individual, producing the cutaneous in another. Nor is the external manifestation of the diphtheritic poison in any respect less formidable than the faucial. In many of M. Trousseau's cases the symptoms of typhoid adynamia were present in an aggravated form; they often terminated fatally, or if not, were followed by a tedious convalescence. Similar facts were observed by M. Daviot in the epidemic which he describes. "Diphtheria might be seen simultaneously in several members of the same family, attacking in one the pharyngeal mucous membrane, in another the skin, in another the respiratory passages, in a fourth all these organs at once or in succession." He fails to discover in the facts which have come under his observation any ground for inferring that cutaneous diphtheria results from the application to the denuded derma of diphtheritic matter from the throat, or from other parts of the surface previously affected. Although the contagionists find in the phenomena of cutaneous diphtheria strong ground for the support of their theory of inoculation, several facts might be mentioned which seem to point in the opposite direction. Not only does the cutaneous exudation present itself in persons not previously affected with pharyngeal diphtheria, but it very frequently selects remote situations, and such as would appear inaccessible to inoculation —as for example, the folds of the groins in children, and the spaces between the toes. It not unfrequently happens that diphtheria, especially when it occurs consequently on measles and scarlatina, is complicated with an eruption of bullæ of rupia simplex in various situations. These often become the seat of cutaneous exudation. Instead of the formation of a brownish thick crust, it is observed, as the bulla becomes flaccid from the absorption of its contents, that a firm concretion can be felt beneath the still entire epidermis; this is found on the removal of the covering to exhibit all the characters of the diphtheritic exudation. A single well-observed fact of this kind is sufficient to cast a doubt on the theory of inoculation.

There is no part of the volume of the Sydenham Society which will better repay study than the researches of M. Empis, founded upon an epidemic of diphtheria observed at the Hôpital Necker in 1848. The author treats his subject under the heads of Pathological Anatomy, General Pathology, Diagnosis, Etiology, Prognosis, and Treatment. He is the only observer whose writings we have before us who has investigated the pathological anatomy of the disease with the aid of the microscope. After noticing the statement of Vogel, that the Oïdium of muguet is to be found in the pellicle of diphtheria, a statement which one finds repeated from time to time by other authors, and which is to be attributed to the confusion of all pseudo-membranous exudations under the term diphtheria, he compares the diphtheritic exudation with other similar products—the buffy coat of the blood, the false membrane of pleurisy, the concrete exudation of blistered surfaces, and that which occurs in the angina of scarlatina; and admits that as regards the two latter it is impossible to draw any distinction founded on microscopic investigation. His results, however, in this respect are rendered of little value by the absence of drawings or even of careful descriptions of appearances.

M. Empis throws some new light on the genesis of diphtheritic exudation. He shows that the commencement of the pellicle is in reality an act of coagulation; that the mucous membrane exudes in the first instance a liquid plasma, in which coagulation takes place by a precipitation of fibrine independently of any agency of the living tissue. This is most plainly seen in the air-passages, particularly in the larynx and trachea, in which the tubular cast is rarely ever adherent, and is usually much smaller than the cavity which it occupies, its external surface being therefore separated by a considerable interval from the mucous membrane. That coagulation is not determined by the mucous membrane is still more strikingly exhibited in the fact to which M. Empis bears testimony, that "at the end of a few hours after the operation of tracheotomy, whatever care might be taken to clear the canula, the instrument was seen to be lined with a layer of whitish concretions, the thickness of which continually increased. These concretions were evidently only the result of the coagulation of the liquid by which the sides of the canula were constantly covered." M. Empis's views of the general pathology of diphtheria are more philosophical than those of Bretonneau. He clearly recognises as a character of diphtheria "the property which it has of being generalized in the economy, like the diseases *totius substantiæ*," a property, as he well observes, which may be best appreciated when the disease is studied epidemically. He concludes that diphtheria, "like all the disseminated phlegmasiæ, presupposes a general morbid condition or special diathesis, which engenders and interweaves inflammations identical in their nature, but which differ in their seat, their extent, their intensity, their duration;" and he considers these local manifestations as the expression of a special element of disease intimately combined with inflammation in its development, but different from it in its essence. "The existence of such a diathesis is revealed by the general symptoms. The prodromata are of longer duration than in the pure phlegmasiæ;" the patients pass from the state of health to that of disease through an intermediate condition, which from the first foreshadows the specific nature of the impending affection, and which is marked by "*malaise*, anorexia, slight fever, dysphagia, and glandular swelling."

In the paper of M. Daviot we shall confine ourselves to two questions which appear distinctive of his researches—the communicability of diphtheria by personal intercourse, and the treatment of the disease by antiphlogistics.

"Pharyngeal diphtheria," says the author, "is purely and simply an epidemic disease. Like other diseases which assume this character, it only manifests itself in those localities and individuals which have the most affinity for it. Springing from an alteration in the constituent elements of the atmosphere—an alteration unknown in its essence, but appreciable in its effects—it is propagated through the medium of that fluid. . . . . A great number of persons were struck by the epidemic a few days after arriving in the infected places, and without having communicated with any patient." (Dr. Semple's Transl. p. 362.)

M. Daviot denies, as regards the epidemic at Autun, that it generally happened that all or the greater number of the members of a family were attacked at once, and states that it was quite as common that only a certain number of persons living under one roof were affected, and that the successive attacks took place at considerable intervals, "usually at each recrudescence of the epidemic, that is to say, whenever the epidemic agent having attained a renewed activity, exercised its preference for the same idiosyncrasies."* This quite accords with our own experience. We have known several instances in which all the members of a family have been attacked during the course of an epidemic, but at intervals of such length as to preclude the supposition that the successive attacks resulted from personal communication. Such results M. Daviot thinks can only be accounted for by "Similarity of organization and predisposition in individuals placed under the same hygienic circumstances, and therefore subject to the same morbific influences. . . . Will any one contend that the contagious principle could have six months, a year, or even more, of incubation before its development? Such an explanation is contrary to all probability, and does not require to be refuted."

M. Daviot did not meet with a single instance of the communication of diphtheria by personal intercourse. None of the nurses or others engaged in cauterizing the throats of affected children contracted the disease. He finally concludes that pharyngeal diphtheria is not in itself contagious, and that it only appears to be so when associated with eruptive fever.

M. Daviot's paper is of special value in relation to the question of treatment. While on the one hand the completeness of his description assures us as to the identity of his cases with those we meet with on this side of the Channel, he adopts an opposite treatment. In this country there is so general an unanimity in favour of the employment from the first of a stimulant and analeptic treatment, that we are somewhat surprised to meet with a physician who, from the observation of 550 cases, of which 57 were fatal, recommends venesection and local bleeding "coup sur coup," after the manner of Bouillaud. Venesection it appears was only employed when the patient was strong and plethoric, the general reaction great, the skin hot, the pulse full and accelerated, in which case it was sometimes repeated, and was always followed by the best results. Local bleeding was used more extensively "even in patients whose constitution seemed weak, and in whom there was little or no reaction." The plan adopted was to apply leeches, in small numbers and repeatedly, at intervals of six

* Dr. Semple's Translation, p. 358.

hours, thus keeping up a flow of blood until the general reaction had entirely ceased. "The signal services which have been rendered to me by this *modus faciendi*, compel me, as a duty, to announce the results of my experience, and to recommend the plan to the profession." These facts illustrate the general truth that in every epidemic there are a number of cases in which the constitutional state on which the tendency to a fatal termination depends is not developed at all, and consequently any treatment may be employed with a similar result, a favourable change taking place before the end of the first week. They further show that even in cases of diphtheria, when inflammatory symptoms are present, local bleeding may be employed with great relief to the patient: but Daviot himself is compelled to admit that in the only cases which presented symptoms of danger, the use of such remedies is to extinguish the only hope of life which remains. Accordingly, he adds :—

"When at the commencement I recognised the membranous angina, which I will term adynamic, as manifested by the absence of reaction and by the rapid progress of the disease, the brownish colour of the membranous exudation, the gangrenous smell of the mouth, the smallness of the pulse, the discomposure of the features, and general collapse, &c. &c., I avoided all evacuations of blood, in order to have recourse to a stimulant method of treatment." (Dr. Semple's Transl. p. 368.)

M. Daviot's essay is followed by a series of well-recorded cases, the omission of which in the translation we regret.

Before leaving the Sydenham Society's volume, we must say a word in commendation of the manner in which the translator has accomplished his laborious task. In the translation of a scientific work, clearness and accuracy of rendering are almost the only qualities which are indispensable. We have carefully compared the English version with the original, and have great pleasure in bearing testimony to the general merits of the translation; if we point out one or two passages in which we think that the meaning of the author has been incompletely represented, we do so with a view to the correction of the oversight in a future edition, and not by any means in the spirit of censure.

"Aucun tissu peut-être n'est passible d'un seul *mode inflammatoire*." (p. 1.)	"No tissue, perhaps, exhibits a single *inflammatory lesion*." (p. 1.)
"On n'eut pas tardé à découvrir que l'angine maligne *ne* consistait *pas* dans une nécrose du tissu muqueux." (p. 7.)	"They would not have failed to discover that malignant angina *consists only* in a gangrene of the mucous tissue." (p. 4.)
"Il ne sera pas inutile de reproduire dans un *tableau tracé d'après nature* les traits, &c." (p. 48.)	"It will be convenient to represent its characteristic features in a *tabular form drawn up from nature*." (p. 24.)
"*On ne peut méconnaître* la diphtérite thrachéale dans l'affection orthopnoïque dont parle Baillou, &c." (p. 61.)	*We cannot mistake* tracheal diphthérite *for* the orthopnœic affection of which Baillou speaks." (p. 29.)
"Nul doute que *ce ne fut bien* notre maladie." (p. 77.)	"There is no doubt that this *was not really* our disease." (p. 36.)

Mr. Wade's valuable pamphlet lays claim to novelty as being the first publication in which the importance of albuminuria as a complication of diphtheria was recognised. Its scope is limited to the symptomatology of the disease. It professes, in fact, to be the first chapter of a more extended work, comprising the whole subject. After an introductory sketch of diphtheria as it presented itself at Birmingham, which leaves the impression on the mind of the reader that the author has not only possessed and used large opportunities of observation, but that he has formed a clear and well-defined conception of the disease as a whole, he proceeds to an extended discussion of its relations with scarlatina. He concludes that diphtheritic scarlatina (that is, we suppose, scarlatina accompanied with diphtheritic exudation) cannot be distinguished by the appearance or physical properties of the pellicle from true diphtheria; and secondly, that there is a pathological affinity between the two diseases. With both of these propositions we entirely concur; we have already seen that in cases of pure diphtheria there is the greatest possible variation as to the extent, consistence, colour, and adherence of the pellicle. That there is affinity between the two diseases is so evident as scarcely to need illustration; they are both epidemic pyrexiæ, which are accompanied with morbid changes, differing, it is true, in their nature, but agreeing as to the organs affected; but if Mr. Wade intends, as we suspect he does, to break down the specific distinction of the two diseases, we are at issue with him. Without referring to facts on the other side of the Channel, abundant evidence has now been collected in this country, that scarlatina is as distinct from diphtheria as from measles or small-pox. Scarlatina and diphtheria have been associated together in England in a most remarkable manner; epidemics have occurred in which, although scarlatina has been seen without diphtheria, it has been rare indeed to observe diphtheria independently of its more familiar associate. In these localities it has invariably happened that practitioners, judging from their own experience exclusively, have concluded that they had to do, not with a new disease, but with a modification of an old one. Facts are now, however, on record, which render such conclusions wholly inadmissible. Diphtheria has recently prevailed epidemically in some localities, although scarlatina was not only absent at the time of its prevalence, but had been unknown for years. In others, in which scarlatina has prevailed with great fatality contemporaneously with diphtheria, it has been rarely complicated with it, and not in the most fatal cases. Finally, scarlatina in a mild form, not accompanied with any throat affection, has co-existed with the fatal prevalence of diphtheria.

Respecting his discovery of the relation of albuminous urine to diphtheria, Mr. Wade observes:—"I had never met with any hint, either from authors on this subject, or from my professional friends, which indicated that they had discovered or suspected the existence of this most momentous complication." Along with albumen the author usually finds in the urine tube-casts and renal epithelium, the former being either of the kind designated as "small waxy casts," or "epithelial casts." We can find no observations tending to show to what extent the progress of the disease is affected by the condition of the kidneys which these appearances indicate, excepting the mention of a single instance in which the discovery of albuminuria was accompanied by increase of stupor, restlessness, and jactitation; nor has the author informed us at what period of the disease he has first detected albuminuria.

It is a most remarkable fact that, notwithstanding the extended clinical inquiries of which diphtheria has been the subject in France, this symptom was overlooked for so many years, and that the disease needed to cross the Channel in order that this element in its nature should be discovered. That the renal complication is a grave one there can be no doubt, but further research is yet necessary to enable us to determine its prognostic value. We have seen cases in which albuminuria has manifested itself throughout the whole course of the disease, in which not only has the termination been favourable, but no other alarming symptom has been present. On the other hand, fatal cases have occurred in which it has been absent. Further observation will doubtless soon throw light on this the most important moot point in the pathology of diphtheria.

The remarkable after-symptoms of diphtheria which we have had so many opportunities of observing in England, had in like manner, until the last few years, attracted very little attention. Dr. Faure, in his essay on this subject, terminates a series of interesting cases by the following vivid picture:—

"The patient had had diphtheria; he is cured, and every trace of false membrane has disappeared. Some time after, without known cause, the skin loses colour and acquires an almost livid pallor. Sharp pains attack the joints; the limbs lose their strength; by and bye the patient falls into an almost inconceivable weakness." (Op. cit. p. 66.)

All those functions which depend on muscular action are impaired.

"The legs are no longer capable of sustaining the weight of the body, the arms lose their strength, and can no longer obey the will; the movements of the body become staggering and uncertain, and seem no longer directed to a determined purpose. . . . The velum is completely paralysed, and hangs down like a withered, lifeless, curtain, and hinders speech and swallowing. All the muscles of the jaw, neck, and breast are more or less paralysed, consequently upon which deglutition and breathing are rendered difficult." (p. 66.)

Along with this there is impairment of vision, unequal dilatation of the pupils, and strabismus.

"The cutaneous sensibility is markedly diminished; in the limbs sometimes it is annulled, sometimes abnormal sensations are experienced, particularly that of formication. . . . There is no general reaction, fever is rare, the skin usually more or less moist. . . From time to time the intelligence flashes through the stolid expression, which becomes more and more marked in the countenance, or a smile plays among the features. Finally, the prostration reaches its highest degree, and death follows, either after a faint, or as the last expression of exhaustion, the tranquil extinction of life." (p. 66.)

One word on the designation, diphtheria. Mr. Wade severely blames Dr. Farr for assuming the right of "imposing on the medical profession a term which he has coined and circulated, without the courtesy of previously submitting it for their approval or rejection." Probably Mr. Wade is already aware that the form, diphtheria, was not Dr. Farr's invention; but without reference to this, the protest is certainly too categorical. Every one has a right to invent and circulate a new name, and is certainly not to be blamed because it is accepted and comes into general use. Etymologically, we think that diphtheritis is much more defensible than diphtheria, being analogous, as M. Bretonneau observes, to *pleurites*, *phrenitis*, *ascitis*, and other similar words, which were originally used by the Greek physicians adjectively,* to denote in each case the diseases of, or belonging to, the side, the diaphragm, or the belly, respectively, without any reference to the idea of inflammation. But, on the other hand, we also agree with those who are of opinion that, in the common mind of the profession, the termination *itis* is apt to lead to the false notion that the disease is of an inflammatory type, and that therefore it is practically a good thing that the change has been effected. We regret that a word with an English ending, such as diphthery, has not been adopted, which we could use without the fear of being called in question either for false quantity or bad Greek.

Review II.

Verslag over den Staat der Gestichten voor Krankzinnigen, en toelichtende Opmerkingen nopens de daarbij gevoegde Statistieke Tabellen betrekkelijk hunne Bevolking over de Jaren 1854, 1855, *en* 1856. Aan Zijne Excellentie den Heer Minister van Binnenlandsche Zaken. Ingediend door de Inspecteurs dier Gestichten.—*'s Gravenhage,* 1858.

Report on the Institutions for the Insane, with Explanatory Remarks respecting the Statistical Tables appended, in reference to their Occupants, during the years 1854, 1855, *and* 1856. Presented by the Inspectors to his Excellency the Minister of the Interior. With numerous Tables.—*The Hague.* Royal 8vo, pp. 146.

The signatures to the above-named very able and valuable Report are those of the civil inspector, C. J. Feith, and of the medical, the distinguished physiologist and psychologist, J. L. C. Schroeder van der Kolk. To the zeal and energy of the latter the present admirable state of the institutions for the insane in Holland is mainly owing, and it is from his pen, we believe, that the Report now before us almost exclusively emanates. We may therefore expect to find much valuable information in its pages.

There are at present in Holland eleven authorized medical institutions for the insane, viz., those of 'sHertogenbosch (better known to the English reader under the name of Bois-le-Duc), Zutphen, 's Gravenhage (The Hague), Dordrecht, Delft, Amsterdam (for Jews), Meerenberg, Utrecht, Franeker, Deventer, and Maestricht. To this list the

* ἡ φρενῖτις νόσος, ὁ ἀσκίτης ὕδρωψ.

institution at Rotterdam, though not yet formally recognised, is for the present added.

The first chapter in the Report, on details relating to the buildings and furniture, need not detain us. Separate chapels for the use of Protestants and Roman Catholics are incidentally mentioned as existing in connexion with some of the asylums.

The second chapter is devoted to the subject of medical attendance. The writers observe—

"The number of institutions possessing one or more medical men appointed exclusively to attend the asylums, namely, those of Zutphen, Dordrecht, Meerenberg, Utrecht, Franeker, and Deventer, has not been increased since the publication of our former report. The most urgent necessity for such a provision exists undoubtedly in the institution at 's Hertogenbosch, in which, at the end of 1856, not less than 243 patients were under treatment. No proof is required to show that a physician who is at the same time engaged in ordinary civil practice, and consequently can spend only a few hours daily in the institution, even if he combined the warmest zeal with the greatest abilities, must be unable to bestow on so many patients the care they so much require, to the nature of which, having in our former report (pp. 24–27) explained our views, we need not now return. In our opinion it is very much to be regretted that the exertions which have frequently been made to put that institution in possession of a physician devoted exclusively to its service, should so far have proved unavailing, principally for want of means to pay such an officer, whose remuneration should of course be on a more liberal scale than would be required to compensate the more or less fugitive attendance of a few hours daily. We consider this want in an institution where so many patients are received to be a very great defect. A similar want is felt also in some other establishments." (p. 18.)

We quote the foregoing at length, as the views contained in it are important and of general application. Passing over mere local details, in which of course the Report abounds, we shall now allude only to such points as may strike us as being of more general interest.

It appears that—

"While Asiatic cholera appeared in 1854 and 1855 in different places where institutions existed, eleven of the latter remained quite free from the disease. Only in the asylum at Utrecht did it break out in 1854, when five women of the lowest class died of the disease.

"Other diseases occurring in different localities have not been communicated to the institutions. Thus the asylum at the Hague remained in 1854 and 1855 free from the small-pox, although the latter was very prevalent in the immediate neighbourhood of the institution; and that at Meerenberg, in 1855, enjoyed immunity from an epidemic of measles, which at the time existed in the surrounding districts. In the asylum at Utrecht a threatening epidemic of small-pox, with which the superintendent of the women of the higher class was attacked, was timely arrested by a general revaccination, to which 160 individuals were subjected. In like manner the institution at Deventer remained in 1856 free from the measles, then prevalent there; at least, only one woman who had been for more than four years under treatment for violent periodical mania was attacked with the disease.

"In other respects the state of health of the patients in the different institutions, although of course variable, was, according to the reports we have received, on the whole favourable. Only the following exceptions have been communicated to us:—At Rotterdam, in 1856, several patients suffered from very bad bed-sores and gangrene of the subcutaneous connective tissue, which was often very destructive. Scabies, too, spread among the unclean women, but was successfully treated with inunction and baths. At Dordrecht, in 1856, a more than ordinary mortality prevailed among the men of the lowest class, chiefly in consequence of diarrhœa. In the opinion of the physician, the situation of their sitting room, to which latter the sun has not sufficient access, and which is not sufficiently ventilated, may have contributed to this result. Some improvement has been effected with respect to ventilation. The unusually great mortality which occurred at Meerenberg in 1855, is ascribed chiefly to the exhausted condition of many poor patients at the time of their admission. At Franeker, there were at intervals many cases of intermittent fever in each of the years 1854–1856, as well as in the preceding two years, particularly in the latter half of the year." (p. 22.)

A table, constructed to exhibit at one view the relative mortality from different causes during five years at nearly all the institutions, affords the following result; the per-centage proportion of the number of deaths from each particular disease to the whole number of deaths during the five years is also shown: Apoplexy, $186 \cdot 979 = 0 \cdot 19$; marasmus, $342 \cdot 979 = 0 \cdot 349$; diarrhœa, $47 \cdot 979 = 0 \cdot 048$; consumption, $162 \cdot 979 = 0 \cdot 165$; other diseases, $242 \cdot 979 = 0 \cdot 247$.

The several institutions vary very much as to the frequency of the occurrence of different diseases. Consumption is rarely met with in the asylum at Utrecht.

The author proceeds to examine the reports sent in from the several asylums, with a view to the deduction of practical inferences.

In the treatment of the insane, the first place must be awarded to removal to an asylum as quickly as possible after the occurrence of the malady. The truth of this proposition is so generally admitted that we need not follow the author through his enumeration of the reasons why the change should be so beneficial. In some cases, the psychical, moral, and dietetic influences thus brought into play are sufficient to effect a cure without the aid of medicine. The author observes that this is particularly true of insanity from drunkenness, but that unfortunately, on returning to society, patients affected from that cause almost all revert to their old evil propensity, and, as relapse cases, re-people the asylums. On the other hand, instances have occurred where repeated admonitions, and the conviction that after their dismissal total abstinence is indispensable for their welfare, have been productive of permanently good results. Cases of melancholy, also, afford examples of the efficacy of judicious management apart from the use of directly therapeutic means. Instances are brought forward exhibiting the good effects of regular instruction in neglected imbecile youths. A case is adduced where music had the effect of rousing a young man who had continued for three years in a state of the most hopeless stupidity after mania, and of leading ultimately to his complete recovery. Nutritious diet will be an important means of cure, particularly where weakness, exhaustion, a bad state of the fluids, and perverted sanguification have laid the foundation for the depression of the mental powers.

"A half-witted person, who was for two years in the institution, had all the appearance of a perfect idiot. His head was very large, misshapen, and bald in several places, covered with sores and scabs; his eyes were small, oblique, and squinting; his features without expression; his ears were thick and bent forward; his skin was covered with ulcers; his gait was unsteady and reeling; he was dirty, mischievous, good-for-nothing, troublesome, tormenting, stealing, devouring everything; in short, he was one of the most repulsive inmates of the refractory department of the house. Considered incurable, his treatment consisted solely in rewards and punishment. Imperceptibly his health improved, the ulcers disappeared, his hair began to grow; he was less intolerably filthy, more useful, and better adapted for work. After some time he was transferred to the quiet department, he was well clothed, and set to work on the land; he became more intelligent, and was at last dismissed quite recovered (Dordrecht), certainly affording a remarkable proof how slow a physician ought to be in declaring a case incurable. Occupation, with the consequent abstraction of ideas, and the promotion of the general health, contribute powerfully to recovery, and sometimes lead to restoration where there was reason to despair of it.

"A man had fallen, in consequence of apoplexy, into a state of perfect frenzy, which before his admission into the institution had lapsed into dementia; he spent several months in this state, which was combined with indescribable melancholy, when, at the instigation of the attendant of the ward in which he had been treated since his admission, he began to occupy himself with housework, and subsequently with jobs connected with his trade (that of a carpenter). From that time his improvement steadily progressed, and after a stay of ten months in the institution he was fit to be dismissed, unilateral paralysis of the face being the only remnant of the disease under which he had laboured (Zutphen). Such an example is the more important, as dementia supervening on apoplexy is usually considered to be almost always incurable." (pp. 30, 31.)

Sometimes the accidental occurrence of another disease produces such a change in the system as to cure the patient of insanity. An instance is given from the report of the Deventer Institution, where measles had this effect; and a remarkable case is quoted of intermittent fever of the tertian type, in which the patient, who was quite insane at other times, became perfectly collected during the height of each febrile accession. When the attacks ceased to recur, he relapsed into his former state of uninterrupted insanity, but the attendant physician having learned from what had occurred that to excite the vascular system was the indication for treatment, had soon the satisfaction of seeing him able to resume his business of tailor, and shortly afterwards of being able to dismiss him as cured (Zutphen).

As to the employment of different medicines, the advantage to be derived from them is of course modified by circumstances. Thus, in the institution at Meerenberg, the receptacle of the poor of Amsterdam, tonic and exciting remedies (preparations of arnica, and bark) are found most generally useful; while among the less exhausted class admitted into the asylum at Utrecht, depressing medicines are more commonly indicated. Of lowering agents, calculated at the same time to moderate vascular action, and to relieve over-excitement of the brain, the author unhesitatingly awards the first place to tartar emetic. It is often most advantageously given in combination with direct sedatives, as henbane and opium.

In cases where chronic effusion or induration is to be apprehended, iodide of potassium, with, if necessary, infusion of digitalis, will be found a powerful agent. The iodide has proved very serviceable in chronic abdominal indurations with melancholy. In some cases calomel has been advantageously employed as an antiphlogistic and resolvent. Digitalis was often very efficient in allaying exalted action of the brain (particularly where there was great vivacity without strong vascular action). It seems to act more directly on the nervous, and only secondarily on the vascular, system, in which it differs from tartar emetic. Hence it is not always indicated where the latter is useful. Digitalis frequently acted only symptomatically, so long as the sympathetic cerebral congestion and its remote causes, or the greater or less inflammation of the membranes of the brain, were not combated (Utrecht). Digitaline has also been found useful, particularly where it was necessary to give the medicine in small doses.

The effect of opium is very various, being sometimes beneficial and sometimes the reverse. The use of this drug appears to be indicated chiefly where there is great præcordial anxiety, sleeplessness, and melancholy. In two women, habitual constipation quickly yielded to the administration of half a grain of the watery extract morning and evening (Meerenberg). In such cases, the medicine appears to act by removing the spasmodic constriction of the left colon, which is always met with in that species of constipation.

"Much depends on the greater or less activity of the cerebral vessels. In large doses opium restores the equality of the circulation, the cold of the hands and feet, and their blue colour, disappear. The depressed state of the vascular system is removed, the radial pulse, from being very small and contracted, becomes larger and fuller, the cutaneous excretion is restored, the passive congestion of the head is diminished or disappears, the spasmodic contractions of the left colon are relaxed, rendering the bowels more regular, and preventing the reflex action of the colon on the spinal cord and brain, which excites such very distressing and depressing sensations; a general excitement takes place in the system, and the patient becomes more lively, begins to look at things in a brighter aspect, and becomes bolder and more energetic.

"It is therefore particularly in melancholy, with a very passive state of the vascular system and with inequality of the circulation, with more or less sleeplessness and præcordial anxiety, that opium is in its proper place. In long-continued idiopathic irritation of the brain supervening on meningitis, consequently in more excitable persons labouring under chronic insanity, the medicine will do more harm than good. Where it is indicated, it sometimes effects with surprising rapidity a complete revolution in the condition of the patient. In a case of dull melancholy, with tendency to suicide, where tartar emetic, cupping to the neck and temples, and leeches to the anus, had been employed without success, so rapid a change followed the use of two grains of watery extract of opium four times a day, that on the second day the patient, having had a profuse night perspiration, with a merry countenance and air of earnestness, assured the physician that he was once more better, and that his indisposition had disappeared. His whole being was changed; he acknowledged his former delusion, took part in everything that was going on, was quiet, and subsequently left the institution quite recovered." (Dordrecht.) p. 36.

Lupulin, too, is useful, particularly in cases of sexual excitement. It appears to be less efficacious in producing sleep than decoction of hops.

Extract of belladonna seems to act more especially on the sympathetic nerve, particularly on the part supplying the intestines, and to allay its excessive irritability. Hence, by removing the spasmodic contractions of the left colon, it becomes an useful adjunct to aperient medicines.

Camphor, in doses of two or three grains four times a day, is also a soothing medicine, but its use is contraindicated where there is irritation of the bowels, constipation, a strong pulse, or idiopathic affections of the brain.

> "In some cases where, from opposition on the part of the patient, it was impossible to administer any medicine, the inhalation of chloroform repeated several times daily, but without producing loss of consciousness, seemed in the long run very much to allay the excited state of the nervous system. Internally given, too, in doses of from twelve to sixteen drops in a mixture, three or four times a day, it was frequently very useful in acute hysterical mania. Its internal action was tolerably like, though more strongly sedative than that of sulphuric æther." (Utrecht.) p. 39.

The use of laxatives is of much importance in the treatment of the insane, both on account of the frequent occurrence of constipation in that class of patients, and also as a means of deriving from the head. The aperients to be chosen are such as act on the large intestine, and those most used in the Dutch institutions are the watery extract of aloes and the cortex rhamni frangulæ. The author finds it better to give the aloes in very small doses, repeated four or five times a day, rather than in one dose at night, and recommends combining it with a minute quantity of tartar emetic or extract of belladonna. In cases where the tendency to distension of the colon is very great, a grain, or somewhat more, of sulphate of copper daily, in combination or not with extract of aloes and belladonna, is remarkably useful. This drug appears, by its tonic property, to counteract the dilatation of the colon; by its action on the stomach the appetite is increased, the digestion is improved, and the previously cachectic patients acquire a more healthy look. Extract of nux vomica, too, is an useful adjunct to aloes. The cortex rhamni frangulæ has the property of producing solid motions without pain or griping, and is consequently greatly preferable to senna leaves. Where there is difficulty in getting the patient to take medicine voluntarily, small doses of croton oil, given in plums or tarts, have answered the purpose. By such means we obtain an alvine evacuation, but do not check the tendency to constipation. The author justly adds, that strong drastic purgatives do more harm than good, they produce too much irritation of the intestinal canal.

In cases where tonics, or even stimulants, were indicated, the preparations of iron have been found very useful. Considerable benefit has been derived also from the administration of sulphate of copper to the amount of not more than one grain daily, in divided doses. Still more debilitated patients require Peruvian bark, as a substitute for which, especially in practice among the poor, the author recommends an infusion of the flowers, or a decoction of the root, of arnica. Arnica, he adds, excels Peruvian bark in its exciting property, though it must perhaps yield to it as a direct strengthener. The flowers have more of the former, the root more of the latter, quality.

At the close of this more especially medical chapter, the author relates some remarkable cases. The first is one of anuria. In a female, fits of hysterical mania alternated with anuria of a peculiar nature; for the anuria, which sometimes lasted an entire week, without a drop of urine being voided, was accompanied with the evacuation through the mouth of several pounds of an alkaline water, possessing a strongly ammoniacal odour. The patient asserted that this fluid was vomited, but as it was not unfrequently voided after a copious meal, without containing a trace of food, the attendant physician suspected that it should be attributed to an excessive flow of saliva, probably combined with eructations. Diuretics were employed without success, till at last the disease appeared to yield to the inunction over the loins of ung. scillæ (prepared from the bulb of squill, macerated with caustic potash, and made with lard into an ointment). This was followed by a copious excretion of urine, and a cessation of the attacks of hysterical mania (Zutphen).

In a melancholic patient with dystomania (dipsomania), a peculiar board-like hardness of the abdominal walls, the result of constant tension of the muscles, caused by persistent irritation of the internal parts, was observed. Post-mortem examinations of former cases of the same description had revealed a state of chronic hyperæmia of the mucous membrane of the stomach and duodenum, following which indication, nitrate of silver was prescribed. Only four grains had been given when a decided improvement took place, not only in the state of the general health, but of the patient's mind, and in a few weeks he was able to leave the institution.

In a case of constant excitability in a man, requiring the almost uninterrupted employment of restraint, a cure was obtained, after the failure of other means, by the alternate use of bichloride of mercury and iodide of potassium, the attendant physician having discovered an eruption on the skin, evidently the result of secondary syphilis, to which, too, the insanity was attributable. Cases of the immediate cure of insanity by the reduction of prolapsus uteri are also given.

Instances are brought forward of the injury often done in incipient insanity by general bleeding. Local abstraction of blood, on the contrary, is frequently highly useful, especially where there is active congestion of the brain. Where leeches are employed, the author recommends that after they have fallen off, the use of elastic cups shall be substituted for that of soaking and burdensome warm poultices.

The proportion of epileptics to the whole number of patients received into the several institutions, appears to be increasing: thus, in the ten years 1844—1853, it was a little more than six; while in the three years 1854—1856 it was upwards of seven per cent., showing an increase of 11·6 in the thousand. The author adds some very important and practical observations in reference to epilepsy, but his views on this subject will come more fully before us in our consideration of his great work on the medulla oblongata.

An abstract is given of the results of the post-mortem appearances observed in the bodies of those patients deceased in the several institutions during the period over which the Report extends, from whose friends permission had been obtained to institute necroscopic examination.

Cerebral Cavity.—On conducting such investigations it is in the first place necessary to consider whether the disease had been idiopathic, that is, whether it was produced originally or secondarily by any change of organization in the brain, as is the case in acute mania and its consequent dementia; or whether the affection was only sympathetic, having its primary seat in some remote part, particularly in the abdomen.

It is seldom that an opportunity occurs of making a post-mortem examination of the first stage of idiopathic mania. The organic changes then consist chiefly in slight effusion under the arachnoid, or after drawing off the pia mater, in a change of the colour of the convolutions, if these have been washed with water, from white to a light rose-colour. In the subsequent course of the disease, we find attachment of the pia mater to the cortical substance, in mania, constantly under the os frontis; in idiopathic melancholy, at the vertex, sometimes extending over the anterior lobes. In a still more advanced stage the great accumulation of blood in the vessels usually diminishes, the quantity of effused serum becomes more considerable, atheromata and commencing ossification manifest themselves in the vessels, and the pia mater, formerly adherent to the cortical substance, is easily separated, particularly in a far-advanced stage of dementia; the grey matter becomes paler, the brain is often very soft or else increased in firmness; in paralytic cases we find that the morbid change has extended into the ventricles; the pia mater is firmly adherent to the corpora striata, or these parts themselves are very much softened; the dura mater is strongly attached to the skull, and the latter is usually, in consequence of irritation of the outer lamina of the dura mater (the proper periosteum internum), thickened and hardened. In idiotism there is almost always congenital deficiency of development of the brain, particularly of the subfrontal convolutions. In a drinker, the ventricles were uncommonly dilated (Meerenberg). In a paralytic, whose speech had been much impaired, and in whom the whole brain was anæmic, the corpora olivaria were compressed by a hyperæmic vascular network and a gelatinous exudation (in consequence of meningitis), which not only covered the surface of the brain, but also surrounded the root of the right vagus. The left lung was tolerably healthy, with few adhesions; while the right was very hyperæmic, and attached with fibrous organized bands to the diaphragm. The pleuræ costalis and pulmonalis were attached to one another by means of a number of flocculent non-organized bands, which were easily torn with the finger, while the right lung swam in turbid (purulent ?) serum (Dordrecht). Unfortunately the origin of the vagus, where probably local disorganization existed, was not examined. In another case of idiopathic mania, where the patient could not speak at all, the corpora olivaria were strikingly small (Utrecht). In an idiot who spoke with great difficulty, they were unequal and oblique (Zutphen). In a paralytic maniac, who latterly could scarcely utter a word, and in whom deglutition also was impeded, degeneration of the corpora olivaria was met with (Utrecht). In an epileptic patient, pus was met with in one corpus olivare (Meerenberg). In another, the medulla oblongata was found hard and atrophied (Franeker). In a man who had laboured under hallucinations, the ventricles were highly distended, with thickening of the walls, in consequence of internal meningitis (Zutphen). Osteoporosis of the entire left petrous bone was found in the case of a woman who had suffered from hallucinations of hearing of the same side, and in whom no particular morbid change of the brain was discovered (Zutphen).

A woman, aged forty, previously healthy, except that she had a tendency to constipation, without any assignable cause began to manifest an unusual degree of appetite, with great anxiety and impulse to suicide. Admitted into the Utrecht institution, she became calmer, but now laboured under religious melancholy. She grew weaker, the pulse intermitted, her legs swelled; gastric fever set in, with a yellow, bilious colour of the skin, headache, and increasing distress; she began to cough without presenting any physical signs of thoracic disease, and sank after having been eighteen days under treatment. Microscopic examination showed that all the vessels in the brain and spinal cord, even to the finest capillaries, were studded internally with extremely fine granules of dark pigment. The lungs and heart were healthy, though the blood was not coagulated. From wearing stays the liver and stomach were quite displaced to far below the navel. The menses had long been absent.

Thorax.—Diseases of the chest are often met with among insane and epileptic patients; sometimes large cavities exist without their presence being betrayed by any symptom. Frequently, consumption runs its course without any trace of cough; pneumonia without pain or uneasiness, which is evidently due to altered action in the vagus and medulla oblongata. Thus, in an insane person who had never manifested any symptom of thoracic disease, a cavity as large as a hen's egg, with dense and hepatized walls, was found in the left lung; the medulla oblongata was very much hardened (Dordrecht). The coexistence of pneumonia with epilepsy seems greatly to aggravate the attacks of the latter.

Abdomen.—It may be stated as a general rule, that prolongation and dilatation of the transverse colon, or indeed also of the sigmoid flexure, occur in a greater or less degree, with strictures beneath the dilatations, in melancholic individuals, although these remote abdominal causes may also give rise to mania. In one instance, the colon was found to measure three times its normal length. The transverse colon has frequently descended to the os pubis. In one case, it passed over the liver and rose to the third rib. In two instances, this part of the colon was adherent to the symphysis pubis.

The proportion of domestic servants to patients in the several institutions in Holland varied, in 1856, from 1 to 5·4 in Amsterdam, to 1 to 12·5 in Rotterdam. The asylum in the latter city is intended almost exclusively for the poorer classes. The average proportion was 1 to 8·1. The total number of patients in all the institutions at the end of 1856 was 1956. The proportion these bore to the whole population of the kingdom was, on the 1st January, 1854, as 0·51 to 1000; on the 31st December, 1856, it had increased to nearly 0·60.

The authors express their conviction that the opportunity of attendance on divine worship is an indispensable requisite in a well-regulated institution for the insane, and accordingly, at the end of 1856, regular services were held in eleven out of the twelve asylums in Holland. They add, that in their next report they will be able to state that similar arrangements have been made at Deventer, where the obstacles to the introduction of Divine worship were of longest continuance. In 1856, two-fifths of all the patients were judged fit to attend the services, and proofs are brought forward of the influence of the privilege in promoting recovery.

The report on the instruction of the insane is favourable, so far as it goes. The authors promise further information on this subject in their next report. They strongly recommend the suitable occupation of such patients as may be at all fitted for employment. As means of amusement, billiards, the cultivation of flowers and animals (pigeons, rabbits), stereoscopes, and books are employed. The *Illustrated London News* is among the publications taken at the Utrecht Institution. The authors are of opinion that the *non-restraint system*, though good in principle, may be carried too far, and that in urgent cases and during fits of great excitement, moderate bodily restraint should be made use of.

Some chapters follow on the diĕt, clothing, and bedding of the patients, the warming and lighting of the institutions, &c., and this very able Report (from which we have endeavoured to extract the principal points of more strictly medical interest, reserving a fuller consideration of the moral treatment of the insane until the next occasion, when more fully developed results will probably be before us) concludes with a number of elaborate statistical tables, containing a large amount of valuable information as to the condition of the insane in the kingdom of Holland.

Review III.

1. *On the Development of the Teeth, and on the Nature and Import of Nasmyth's "Persistent Capsule."* By Thomas H. Huxley, F.R.S. ('Quarterly Journal of Microscopical Science,' No. 3, pp. 149, 164.)

2. *On the Structure and Mode of Formation of the Dental Tissues according to the Principle of "Molecular Coalescence."* By George Rainey, M.R.S.C., and Lecturer and Demonstrator of Surgical and Microscopical Anatomy at St. Thomas's Hospital. ('Quarterly Journal of Microscopical Science,' No. 28, pp. 212, 225.)

3. *A System of Dental Surgery.* By JOHN TOMES, F.R.S., Dentist to the Dental Hospital of London, and to the Middlesex Hospital. With 208 Illustrations.—*London*, 1859. 16mo, pp. 583.

4. *Transactions of the Odontological Society.* Vol. I. *London*, 1859.

IN no department of the profession has more rapid progress been made within the last quarter of a century, than in that of dental surgery. Dentistry has recently undergone a total reorganization, and the Legislature has recognised the importance of the subject by authorizing the College of Surgeons to establish a Board of Examiners on Dental Surgery, and empowering that Corporation to grant dental diplomas. It may not be out of place briefly to pass in review the successive steps which have mainly contributed to this result, the particulars of which are not likely to be familiar to many of our readers.

About sixteen years ago several dental practitioners, recognising the necessity of greater freedom of intercourse between members of their branch of the profession than then existed, attempted to establish a society on a basis similar to that of the other medical societies of the metropolis, viz., for the purpose of reading papers, and the discussion of professional and scientific subjects. The objects of the proposed society, however, were not to be limited to the above purposes: the necessity of some standard of qualification for members of this branch of the profession was generally admitted, and it was contemplated therefore to make such society subservient to the attainment of a dental qualification. But though, owing to the want of unanimity so necessary for the accomplishment of the above objects, and the difficulty of organizing such a society, the attempt was unsuccessful for the time, it must be looked upon as foreshadowing what has been subsequently accomplished.

Perhaps no circumstance can more clearly testify to the increasing importance of the subject, than the addition within the last few years of a dental surgeon to the medical staff of most of the London hospitals. Twenty years ago, there were not more than two of the hospitals at which the necessity of a dental surgeon was recognised; at the present day, so fully is this want admitted, that there is no hospital, and scarcely a dispensary, without such an officer, while lecturers on dental surgery have been appointed in the majority of the medical schools.

The necessity of a recognised qualification becoming yearly more manifest, a memorial upon the subject, signed by several eminent dentists, was, in December, 1855, addressed to the President and Council of the College of Surgeons, requesting that a special examination in the department of Dental Surgery might be instituted.

In the autumn of the year 1856, it became apparent to dentists that some organization amongst themselves was necessary. It would be tedious to give the particulars of the long controversy which ensued; suffice it to state that it resulted in the formation of "The Odontological Society" and "The College of Dentists." Each body professed an identity of purpose, viz., the elevation of the dental practitioner, and each agreed that education and examination offered the proper method of attaining the desired end, but a difference of opinion arose as to the body from which the diploma of qualification should emanate. The Odontological Society proposed a dental diploma from the College of Surgeons, in accordance with the views of the memorialists upon the subject; while the College of Dentists contended for the establishment of an independent institution, which should grant the requisite diploma. Towards the close of the year 1857, the two societies, feeling that their antagonism was prejudicial to their general and common interests, each appointed delegates to arrange the preliminaries upon which an union could be brought about. The terms of amalgamation being settled, and agreed to by their respective councils, it only remained to submit them to the general body of the members of each society. They were first submitted to the members of the College of Dentists at a meeting held on January 8th, 1858, when the terms agreed to by the delegates were rejected. Having now no alternative but to pursue the course they had commenced for the attainment of their object, the Council of the Odontological Society, at a special general meeting held on March 19th, 1858, for that purpose, issued a "Report" to the members of the steps they had taken, and the correspondence they had carried on with the Council of the College of Surgeons upon the subject of a dental diploma from that Corporation.

After the reading of the report, the Society, approving the course taken by the Council, unanimously resolved to establish a school for teaching those branches of science appertaining peculiarly to the practice of dental surgery which are not taught in the existing medical schools; the members empowered the Council to open such school as soon as circumstances would permit. Pursuant to this resolution, the Council requested the co-operation of several members of the College of Dentists in the formation of a Committee for the establishment of a dental hospital as a preliminary to a school. This hospital, called the "The Dental Hospital of London," situated at No. 32, Soho-square, the premises formerly occupied by the Linnæan Society, was opened towards the close of the year 1858, and is now in a flourishing condition; a school in connexion therewith, called "The London School of Dental Surgery," has been established in conformity with the requirements of the Dental Charter. A school has also been organized by the College of Dentists, called "The Metropolitan School of Dental Science."

At the time the memorial was addressed to the President and Council of the College of Surgeons, it appeared that it was not in the power of the latter body to institute a special diploma in dental surgery; a clause was therefore introduced into the New Medical Act which passed through Parliament in the Session of 1858, conferring the requisite power upon the College of Surgeons, and authorizing that Corporation to undertake the regulation of the education of future dentists; in virtue of this power, embodied in a special charter, the dental diploma has been established.

A journal devoted exclusively to subjects connected with dental surgery, and affording a medium of communication between members of this branch of the profession, must be looked upon as a prominent feature of the present era. The first number of 'The British Journal of Dental Science,' a monthly periodical, appeared in July 1856. This was followed by 'The Quarterly Journal of Dental Science,' which in January, 1859, was also changed into a monthly issue, under the name of 'The Dental Review.' Both journals are of a scientific character, and well conducted. The former is supposed to express the sentiments of the Odontological Society, the latter is considered the organ of the College of Dentists. A volume of Transactions has been published by the Odontological Society, containing several papers of interest, which bear ample testimony to the position which the Society has already attained, and may be taken as an augury of its future beneficial influence. Having premised these brief remarks on the politics of dentistry, we now turn to its more important scientific features.

The publication by the late Mr. Alexander Nasmyth of the views of the Swedish and German physiologists on the structure and development of the teeth, in 1839, followed by Mr. Owen's admirable work, 'Odontography,' may be considered as the commencement of the modern era of dental surgery. Mr. Tomes' elaborate course of lectures on the subject, however, delivered at the Middlesex Hospital, first published in 'The Medical Gazette,' and subsequently in a volume, entitled 'a Course of Lectures on Dental Physiology and Surgery,' laid the foundation of scientific practice, by establishing a line of treatment of the diseases of the teeth based upon physiological and pathological knowledge; and indeed to Mr. Tomes is due the credit of having written the first account of the structure of the teeth in this country, furnished in a paper describing his researches, and read before the Royal Society, June 21st, 1838, entitled 'On the Structure of the Teeth, the Vascularity of those Organs, and their Relation to Bone.'

The researches of the Swedish, German, and English physiologists on the structure and development of the teeth are given in the fifteenth number of this periodical. The authors therein quoted are, Fraenkel, Raschkow, Müller, Retzius, Tomes, Owen, Goodsir, and Nasmyth. They generally agree in the correctness of the conversion theory—viz., that the elements of the pulp are transformed into dentine, that the enamel is formed from the enamel organ or enamel pulp, and the cementum from the capsule. The subject has subsequently been investigated by Mr. Huxley, who has arrived at conclusions different from those of the above-named authors, and we now proceed to give an outline of his views.

For the purposes of examination, Mr. Huxley takes the skate, the mackerel, the frog, the calf, and man, as accessible specimens of each of the great divisions of animals possessing teeth. In dealing, first, with the question, what are the structures concerned in the development of the teeth? Mr. Huxley states that there are two modes of their development—viz., that in which the pulp is never free, but from the first is included within the capsule, as in the mackerel and frog; and that in which the pulp projects freely at one period above the surface of the mucous membrane, becoming subsequently included within a capsule formed by the involution of the latter, as in the human subject. The skate offers a sort of intermediate stage.

He then gives an analysis of the developmental conditions in the mackerel, the frog, and the skate, the inference of which is, that in both the mackerel and the frog the papilla is wholly a process of the derm (or that which in a mucous membrane corresponds to it) outwards, while the sac is a process inwards of the same structure, and that the homogeneous substance, with its embedded nuclei between the two, corresponds with the epidermis or epithelium; he states that in the skate the follicle is an involution of the derm, the papilla is a process of it, and the epithelium between the two becomes metamorphosed sometimes into a peculiar stellate tissue.

In tracing the conditions in man, Mr. Huxley states that a space filled with a fluid always exists between the inner surface of the capsule and the outer surface of the pulp—*the two are perfectly free from all adherence to one another*—and in addition to the above fluid is a more or less abundant whitish matter, which sometimes adheres to the one and sometimes to the other (Goodsir). A structureless membrane, the membrana preformativa of Raschkow (the basement membrane of Bowman), may be traced over the whole surface of the pulp, and in perfect continuity on the walls of the capsule—in fact, into its basement membrane. He then gives a description of the whitish substance between the basement membrane of the pulp and that of the capsule, and infers that from its structure and its relation to the above membranes, it is nothing more than the altered epithelium of those organs. It is the so-called "*enamel organ*" of authors.

Mr. Huxley then pays a tribute to the late Mr. Alexander Nasmyth, for having, as he thinks, published the first accurate and detailed account of this so-called enamel organ in his work 'Researches on the Development, Structure, and Diseases of the Teeth,' 1849. After quoting the description, he concludes that in man, as in the skate, the mackerel, and the frog, the tooth pulp is a dermic process bounded by its basement membrane; that the capsule is an involution of the derm, bounded by its basement membrane, and that the epithelium of these organs lies between them, having in this case received the name of "*enamel organ*" from the supposition that the enamel was developed by the calcification of its elements. Mr. Huxley then points out that the "actinenchymatous" tissue (Raschkow) of the calf occupies a wide interval between the basement membrane of the capsule and the alveolar wall, and does not correspond with the stellate tissue of man, as has been assumed by all writers; but that the true homologue of the "enamel organ" in man, is in the calf a thin layer of elongated cylindrical epithelium cells, between the wall of the capsule and the surface of the pulp.

Mr. Huxley next inquires into the relation of the dentine, enamel, and cement to the pulp, the capsule, and enamel organ, and states that neither the capsule nor the "enamel organ" take any direct share in the development of the dental tissues, all three of which—viz., enamel, dentine, and cement—are formed beneath the membrana preformativa or basement membrane of the pulp. The prominent point of evidence adduced in proof of this assertion is the demonstration of a voluminous transparent membrane about 1–2500th to 1–1600th of an inch thick, which by the addition of strong acetic acid may be raised from the surface of the pulp in a human fœtus at the seventh month, beneath which, Mr. Huxley states, the ends of the enamel fibres may be very distinctly seen. Under a high power, the surface of the upper part of the ossified cap appears reticulated, the meshes being about 1–5000th of an inch in diameter, and the membrane exhibits innumerable little ridges upon its outer surface about 1–5000th of an inch in diameter. At its lower edge this membrane passes into the membrana preformativa—in fact it is the altered membrana preformativa itself. A membrane corresponding with that described in the human subject is also found in members of each of the other groups of vertebrata which possess teeth, presenting, Mr. Huxley thinks, correlative evidence of the correctness of his views. Mr. Nasmyth is again alluded to as having described this membrane under the name of the "persistent capsular investment;" but Mr. Huxley adds, that he, like all who have succeeded him, misled by the supposed mode of development of the enamel from the enamel organ, imagined that as the "persistent capsule" was outside the enamel, it could be nothing else than the membrane of the dental capsule. Mr. Huxley then concludes that since this "Nasmyth's membrane" is identical, on the one hand, with the persistent capsule which lies external to both enamel and cement, and, upon the other hand, with the preformative membrane of Raschkow, or otherwise with the basement membrane of the pulp, it is clear that all the tissues of the tooth are formed beneath the basement membrane of the pulp—in other words, they are all true dermic structures, none epidermic.

Mr. Huxley asks, thirdly, what is the relation of the histological elements which enter into the composition of the soft parts to the dentine enamel and cement, which are formed from or within them? In entering upon this problem, Mr. Huxley denies the correctness of the conversion theory—viz., that the histological elements of the pulp become calcified and converted into dentine; and believes that the dentinal substance is deposited within the pulp beneath the membrana preformativa. This he proposes to call the "Deposition Theory." In refutation of the former (the conversion theory) Mr. Huxley adduces the absence of nuclei in young dentine, which he failed to discover, even when, by the use of strong acids, the calcareous matter was dissolved out; though those which exist in the pulp became much more distinct and even coarse in their outlines, the bodies which have been described as nuclei being, he says, simply lacunæ. Again, by the addition of a solution of iodine to the pulp, its nuclei are demonstrated by becoming brown, but the dentine under this agent remains pale. Mr. Huxley is more satisfied with this negative evidence, as in young bone it is easy to demonstrate the nuclei in the lacunæ by the aid of acids. He thinks the enamel affords still less evidence of conversion from a cellular structure; between it and anything which can be called a nucleated cell, it has on the outside Nasmyth's membrane, on the inner the layer of dentine, which in man is formed before it; and he points to the dermal teeth or plates on the upper surface of the head of the skate having as distinct a layer of enamel as those of the mouth, though in this case there is neither rudimentary capsule nor "enamel organ." In a morphological point, Mr. Huxley thinks the cement is homologous with the enamel; in proof of which, allusion is made to a section of a human tooth, the upper portion of the cement of which exhibits in places a very distinct striation resembling its perfect enamel. Again, the cement on the fang of a molar of a young calf which had not cut the gum consists of an external Nasmyth's membrane, internal to which three-fourths of the thickness of the layer are formed by parallel fibres 1–5000th of an inch in diameter, quite structureless, and completely resembling enamel fibres; these were softened and rendered pale by the action of caustic ammonia. On the root of the fang of the molar in front of this, which had cut the gum some time, and had come into use, the cement had the ordinary structure. In these teeth, the capsule, though closely connected with the outer surface of the fang, could be readily stripped from it, and then exhibited a layer of epithelium upon its inner surface, showing clearly that the cement was not derived from its ossification. Mr. Huxley concludes :—

1. The teeth are true dermic structures, formed by the deposit of calcareous matter beneath the basement membrane of a dermic papilla, or that which correspouds with one.

2. Neither the capsule nor the "enamel organ," which consists of the epithelium of both the papilla and the capsule, contribute *directly* in any way to the development of the dental tissues, though they may *indirectly*.

3. The histological elements of the pulp take no direct part (except, perhaps, eventually in the cement) in the development of the dental tissues, becoming either absorbed or being pressed in by the gradual increase of the latter; and the dentine is formed, not by ossification of the histological elements of the pulp, but by deposition in it.

Mr. Rainey has put forth another theory, viz., that the mode of calcification of the dental tissues is identical with that by which bone and shell are formed, viz., the principle of "Molecular coalescence," and that the dentinal tubules have no distinct parietes, but are merely spaces bounded directly by the dentine fibres and the partially coalesced dentine globules. He published his views 'On the Formation of the Skeletons of Animals, and other Hard Structures formed in Connexion with Living Tissues,' in the fortieth number of this Journal. A subsequent communication was made by him to the 'Journal of Microscopical Science for July, 1859,' detailing his researches in special reference to the structure and development of the teeth. In tracing dental development Mr. Rainey states that, in a cusp in which calcification has made but little progress, a membranous border is distinguishable, extending from the lower margin of the shell of tooth substance; which, having the same relation to the calcified portion of a young cusp that the membranous edge of a flat bone has to the ossified part, he proposes to call the "membranous matrix of the cusp." It is situated between the enamel and dentine pulps, its surfaces being respectively in contact with the corpuscles of each, and its free margin lodged in the groove formed by their union, to which groove it is united by exceedingly fine connective tissue.

After the deposition of calcareous particles on it, this structure divides into two layers; one the membranous matrix of the dentine, the other, the membranous matrix of the enamel. It appears to be made up of very delicate flattened corpuscles of different shapes and sizes, but generally longer in the vertical than in the transverse direction of the cusp.

In speaking of the formation of dentine, Mr. Rainey says its first indication is the appearance of very minute and more or less scattered bright particles, like very fine particles of dust, on the inferior surface of the membranous matrix, a short distance from its lower border. These globules become arranged in lines, or globular masses, and by their ultimate coalescence the lines become fibres or rods of dentine. This process, says Mr. Rainey, is exactly the same as that which takes place in the calcification of the claw of the lobster. The spaces between the rods, or between the partially coalesced globules or granules, are the so-called dentinal tubules. In proof of the absence of parietes to the tubes, Mr. Rainey argues that, as the above named dusty-looking material—incipient dentine—always intervenes between the partially formed tubes and the dental pulp, all the fluid which is contained in their interior must have first passed through mere interstices or spaces; hence if at this, the most important epoch of a tooth's formation, mere spaces have sufficed for the conveyance and supply of interstitial fluid to its substance, he does not see why parietes should be afterwards added to those spaces, as by such an addition a complex form of structure would be superadded to a simple one, after the tooth tissues had ceased to perform any obvious organic function; thus this substitution of tubes with parietes for mere spaces, would come too late to serve any obvious purpose. Mr. Rainey thinks the shape of the spaces favours his views; he says that the rods, or fibres, are of a quadrilateral form, and assume all directions, from the pulp cavity to the external surface between a vertical and horizontal axis; and that the spaces are formed at the conflux of every four, passing more or less deeply between each adjoining pair, and depending upon the degree of coalescence of contiguous rods. Now, if a section be made through such an assemblage of fibres and passages as above described, Mr. Rainey says the cut rods will present sections of various forms, some will be nearly square, others diamond-shaped, and a third set linear, depending upon their several directions; and the spaces will partake of the same form, being at first more or less circular, and then becoming angular or arrow-shaped; and where the rods are imperfectly formed, being made up partly of globular portions, the passages running between them will partake of the same form. Now if these rods had inclosed tubes of the form represented by some authors, Mr. Rainey maintains that their sections must have presented first circular areas, and then ovals, becoming gradually more excentric until they ended in forming straight lines. Mr. Rainey is not disposed to recognise the appearance of rings with a dark point in the centre in transverse sections of dentine, as evidence of the existence of tubes. In refutation of such proof he adduces the structure of the siliceous cuticle of the common cane, which at the conflux of every three of the hexagonal blocks of silica of which it is composed, presents the same forms and appearances as have been described in the dentine, viz., the annular, arrow-shaped, and linear forms.

Mr. Rainey states that the particles of the enamel are known from those of the dentine by their parallel linear arrangement in contradistinction to the rows of globules and spherical masses of the latter tissue. These particles join, form continuous wavy lines, and coalesce into the ordinary form of enamel, in which all appearance of the antecedent stages becomes more or less obliterated. The fibres of newly-formed enamel soon show a disposition to break up into irregularly quadrilateral forms, but Mr. Rainey says he has never met with the regular hexagons described by some authors: he thinks that the explanation he has given of the mode of formation of dentine and enamel will explain the laminated character of these tissues, its distinctness depending upon the completeness or incompleteness of the coalescence of the dentine and enamel particles; the interglobular spaces about the extremities of the laminæ, and the contour lines or markings, and the absence of any appreciable spaces in some parts of all teeth, are, he thinks, also explicable on the same principle.

Mr. Rainey regards the osteo-dentine, or "crusta petrosa," as formed on a membranous matrix, directly continuous with and similar in structure to that of the dentine; and the primary particles are only to be distinguished from dentine particles by their subsequent arrangement. They coalesce in the same manner, but in place of taking a rectilinear arrangement they have somewhat of an arborescent form, the small spicular branches of which anastomose and inclose areoleæ of a more or less circular form. These may be regarded as Haversian canals, lacunæ or canaliculi, and in the event of their containing vessels, must of course be considered as the former. This tissue being considered as bone, Mr. Rainey has called the vessels and epithelial corpuscles in contact with its matrix "the bone pulp," and thus the analogy between bone and dentine is preserved; the pulp cavity of a tooth corresponding to a true Haversian canal, the spaces between the dentine rods to the lacunæ, and the extensions of these spaces between uncoalesced portions of dentine to the canaliculi of common bone. The enamel presents similar analogies, but less obvious and striking.

In tracing the development of the dental tissues, Mr. Rainey says, "It is obvious that both chemical and physical effects have been produced, the formation of new compounds is a proof of the one, and the definite forms which have been taken up by their aggregated molecules are a proof of the other;" he believes that these effects are due neither to the sole influence of vitality, nor to the exclusive operation of physical forces, but that they are produced directly by physical and mechanical agency under the control of a general vital principle.

We now come to what we cannot but consider the most important and interesting work connected with the present subject. It is entitled 'A System of Dental Surgery,' by John Tomes, Esq., F.R.S., and consists of 583 pages, with 208 illustrative diagrams. Its character and merits will be duly estimated from the epitome of its contents which we now proceed to give.

The first great section of the work is devoted to the subject of "teething," which comprises above a third of the whole. Under this term the author describes the condition of the teeth and jaws at the time of birth, and traces the successive changes onwards, until the temporary teeth have arrived at maturity. To accomplish this object it became necessary to form a collection of young skulls, in which the different stages might be observed, no such series existing; the labour and ingenuity bestowed upon the subject reflect the highest credit upon this distinguished physiologist.

The author first notes the fact that the development of the hard tissues of a tooth is preceded by the formation of soft tissues, or tooth pulp, of equal size and form to the future tooth; also that both the forms and dimensions of the crowns of the teeth are unalterably fixed long before the jaws are sufficiently enlarged to admit of their ultimate and normal arrangement. The different degrees of development of the crypts in the maxillæ of a full-grown fœtus, and their relations to each other, are then given; also those of the articular process, the alveolar edge, the angle, the inferior margin, and the coronoid process of the lower jaw, and the zygomatic process of the upper jaw. The degree of calcification of the different teeth at the period of birth is also stated. The author next proceeds to describe the condition at two months, showing that a greater change has taken place in the jawbones than in the teeth, and that this is more observable in the lower than in the upper jaw. The relative and respective growth of each process at three, six, and eight months is then most minutely described, as well as the periods in which development in the teeth during these ages is most active. In instituting a comparison of the relative changes which mark the growth, Mr. Tomes points out the necessity of adopting fixed points which are the least subject to alterations of position from which to make measurements. For this purpose he selects the foramen mentale, as its position may be assumed to undergo little or no change after birth. On the inner surface of the lower jaw, the tubercles for the attachment of the genio-hyo-glossus and genio-hyoideus are selected. The upper of the two pairs of processes are at all ages nearly on the same level as the mental foramina, a slight advantage in height being commonly in favour of the latter. An increase of an eighth of an inch from the symphysis to the mental foramen is shown between the fœtal and nine months' jaws in favour of the latter, which increase will be found to correspond with the greater thickness in the antero-posterior direction, near the symphysis of the nine months' jaw; proving that the growth of the anterior parts of the lower jaw is produced by addition of bone to the anterior surface, rather than by any material increase by the development of bone in the fibro-cellular tissue which up to this period unites the two halves; development in the latter position appears to have its period of activity limited to intra-uterine life. After birth the process of growth in this direction is all but suspended until the period arrives for the osseous union of the two halves of jaw, when the action is resumed, the fibro-cellular tissue is replaced by bone, and all further increase at this point is then at an end. A fuller description is given of the jaws of a nine months child in consequence of the specimen having attained that stage of development which immediately precedes the eruption of the teeth. The author dwells upon the fact of the growth of bone being the result of additions to its external surface in contradistinction to interstitial growth. He considers that the conditions of the alveoli, coincident with the progressive development of the teeth, have not attracted the amount of attention which the subject deserves. He has noticed and described very minutely the partial absorption of the walls of the sockets preparatory to the passage of the teeth through the gums, and although this is an important and necessary action for the liberation of the crown of the tooth from the socket, the author states that as far as he knows it has escaped observation altogether. After noticing the completion of the development of the enamel on the crowns of the central incisors, characterized by its smooth and polished surface, which succeeds to the dull, opaque, and almost chalk-like character of the incomplete tissue, and after describing the exact position and degree of development of the other teeth at nine months, the author pursues the investigation with the same minuteness at the ages of twelve, eighteen, twenty-one, twenty-eight, forty, and forty-one months; and the changes in the maxillæ, and development of the teeth at those respective ages, are fully delineated.

The explanation of the changes in the angle of the jaw at forty months deserves especial notice. The mental foramen being maintained as the fixed point of measurement, it is demonstrated that the increased depth of the jaw has been obtained by additions to the alveolar edge of the bone. If equal additions had been made to the lower border, the relations between the body and rami would have been maintained, but with the rapid growth of the alveolar margin, the angle formed by the two divisions of the jaws has become changed; the rami have been elongated, but their growth has been gradual, and not synchronous with the alveolar border, which has been subject to sudden accelerations. This condition however is compensated by the increased depth of the alveoli, and by the protrusion of the several teeth taking place at different periods, in different parts of the jaws; if the whole of the temporary teeth were cut at the same time, and the growth of the alveoli were equal throughout the whole line, the elongation of the rami must assume a sudden activity, otherwise the front part of the mouth could not be closed. By the uninterrupted but comparatively slow elongation of the rami, and the rapid but successive growth of the front and back parts of the jaws, a relation of parts results by which the whole series of teeth are allowed to be brought into contact simultaneously. The more acute angle of the jaw of the forty-months-subject as compared with younger subjects is intended to be confined to the alveolar margin; but the angle formed by the junction of the lower border of the body of the jaw, and that bounding the posterior portions of the rami, will be found to be more obtuse, hence preserving at these points a greater similarity to the younger jaws, and the condition is maintained so long as the jaw continues to increase in length. At four years and a half, the author assumes primary dentition to be completed.

In discussing the subject of abnormal condition of the temporary teeth, *Irregularity of position and number*, with lateral union, are treated of. Under the head, *eruption*, the conditions of the soft parts are considered; and an illustration is given of a vertical section through a decalcified lower jaw of a nine months fœtus, passing through one of the developing teeth, and exposing the different tissues in regular order, from the epithelium to the periosteum of the lower border of the jaw.

Attention is then paid to *Dentition as a cause of local and constitutional disturbance*, followed by a section on the *Relations of the temporary to the developing permanent teeth at the period when the former are fully formed.* The indications of the approaching change of the first for the second set are pointed out; a series of illustrations is given on the subject of the irregularity in the position of the permanent during the existence of the temporary teeth, and the cause of such malposition explained. In speaking of the effects of disease in the temporary teeth on their successors, the author alludes to inflammation being caused by a temporary tooth spreading in a strumous subject involving a large portion of the jaw, and resulting in necrosis: the teeth, whether permanent or temporary, implanted in the sequestrum are usually lost. Cases are cited in which portions of the jaw, including the temporary and the crypts of the permanent teeth were lost; new bone eventually took the place of that which had been removed, and the jaw again became perfect: in several cases permanent teeth most unexpectedly made their appearance, suggesting the idea that the teeth as well as the bone had been reproduced. Mr. Tomes refuses to recognise the development of a second series of permanent teeth, and suggests an explanation of the phenomena. He first refers to the fact of dead being in all cases detached from living bone, by absorption of the layer of living tissue which connects the two: the apertures of the crypts are by the same process greatly enlarged, the connexion between the walls of the crypt and the sac of the developing tooth is in the normal state but a slight one, and this in the character of cases referred to may be rendered still more slight by the action of disease. Now, in the presence of the foregoing conditions, it is not improbable that the pulps of the permanent teeth remain attached to the soft parts, while the crypts included in the sequestrum were removed; and if such were the case, the developing teeth might again be surrounded by newly-formed bone. The practical inference from this theory is, that in those cases where necrosis of the jaw occurs during the presence of the temporary teeth, the sequestrum should be allowed to remain until it is perfectly detached both from the contiguous bone and soft parts, and that its removal should be effected with the least possible injury to the latter; so that the permanent teeth, if not destroyed by the disease, may be placed under the most favourable circumstances for their future growth and evolution.

In the section treating of the *Shedding of the temporary teeth* considerable attention is bestowed upon the absorption of their fangs: the notion of absorption being caused by the pressure of the growing permanent teeth is only noticed to be discarded, and several conditions relative to absorption are alluded to, as having hitherto escaped observation. In absorption of teeth the same characteristic surface is seen as that shown by bone when undergoing a similar action—viz., a surface full of deep indentations, as though they had been made by a sharp piercing instrument with a semicircular extremity. The surface acted upon has an irregular festooned outline, and closely applied to this surface a cellular mass is found, which is but slightly adherent. The miscroscope demonstrates the structure of this peculiar organ. The surface is made up of peculiar multiform cells, each one being composed of several smaller cells. The relation of the more superficial cells to the wasting surface of the dental tissues is peculiarly interesting. The surface of the papilla is closely applied to the wasting surface of the tooth, and the individual indentations correspond to and are occupied by these large cells; below the surface the papilla is made up of ordinary nucleated cells and free nuclei, similar to those contained in the superficial compound cells, while at and near the base the tissue assumes the characteristics of developing fibrous tissue. If a tooth which has lost its fang be carefully removed, we shall find remaining in its place the growing papilla, corresponding exactly in size and form to the surface from which it has been separated, and this separation may often be effected with so little injury that no blood appears upon its surface after the operation, although the organ is highly vascular and readily torn. Laforgue and Bourdet recognised the presence of the absorbent organ, but supposed it to exhale a fluid capable of dissolving the roots of the temporary tooth. In further refutation of the absorption of the fangs of the temporary teeth being caused by the permanent, Mr. Tomes mentions the fact of the occasional complete absorption of the fangs of the permanent teeth themselves; also that of the temporary teeth occasionally maintaining their place to the exclusion of the permanent ones, which are then kept within the substance of the jaw, or appear in some unusual position. Absorption once commenced may not only be suspended, but development may take its place; the excavated surface of the dentine, cementum, and enamel may be covered with cementum following all the irregularities of the former tissues, and closely united to them. This may itself again be absorbed; the process of absorption may be again suspended, and development be renewed; in fact, wasting and reparation may alternate until, by the preponderance of the former, the tooth is shed.

In speaking of *The eruption of the permanent teeth*, the author states that absorption of the coronal portion of the crypt takes place to admit of the passage of the tooth. The comparatively large size of the crown as compared with the neck or root necessitates a breadth of socket during the period both of development and of evolution far greater than is required for the implantation of the fully emerged organ; hence a tooth at this stage of its progress can be readily moved from side to side by moderate pressure, and very slight mechanical obstruction will turn it either into or out of its normal position. The crowns while within the jaws are placed in an uneven line, and this irregularity would become permanent if all were to make their appearance through the gums at the same time. The evolution of the permanent teeth individually and the coincident conditions are then described in the chronological order in which they usually appear.

The *Development of the alveolar processes* in connexion with second dentition is then treated of, and their synchronous growth with the teeth demonstrated, after which the *Growth of the maxilla during second dentition* is examined, and a series of measurements given; the result of which is to show that, taking the spinæ mentales as the points of measurement, the depth of the jaws below the upper spinæ is doubled between the ages of nine months and seven years, and that after the seventh year but little change takes place at this point in respect to depth, while the parts above the spinæ mentales, during the same period, increase from $\frac{4}{12}$ths to the $\frac{10}{12}$ths of an inch, the maximum being attained by the fourteenth year. The mode of growth is also referred to—viz., by subperiosteal development upon the pre-existing bone.

Under the head of *Irregularity of the permanent teeth*, the various positions which the teeth assume, and the mechanical means resorted to for their reduction, are very fully treated of. The causes of these abnormal conditions are traced out, and the consequences shown of the prolonged infantile obliquity of the ramus of the lower jaw into the adult period, and the premature rectangularity of that process, characteristic of more advanced life; also, those of excessive and deficient alveolar development. The absorption of bone, caused by moderate pressure constantly maintained, is demonstrated by the effect on the sockets of the teeth of the mechanical means for reducing irregularity; and the tendency of teeth whose position has been altered by pressure to return to their former positions on the removal of the mechanical restraint for keeping them in the newly-acquired position, is referred to absorption proceeding more rapidly than development of bone. In studying the causes which produce the mal-positions in which the whole of the anterior teeth are involved, the author insists on the necessity of the investigation being commenced prior to the eruption of the permanent teeth. Mr. Tomes denies that irregularity of the permanent teeth is caused by the removal of the temporary organs, and maintains that the contraction of the jaw is not a consequence of such practice. On the other hand, he thinks that the persistence of the first may, and frequently does, interfere with the outward progress of the second set. The abnormal position which each individual tooth may assume, and its treatment, are then given; cases are quoted, showing that apparently insurmountable obstacles may be overcome by ingenuity and perseverance. In the enumeration of the singular positions in which teeth are occasionally found, some are more curious than instructive; for example, an instance is mentioned of the third molar in the lower jaw being situated between the coronoid process and the condyle, the crown reaching nearly to the level of the sigmoid notch; other cases are mentioned in which the same tooth is directed with more or less obliquity inwards, others again in which the position is horizontal instead of perpendicular. Irregularity from transposition of permanent teeth generally affects the canines, which may either be placed between the lateral and central incisors or between the bicuspids. The position of the canine of the upper jaw is during the period of development so much above the adjoining teeth, that any irregularity in the growth of the neighbouring parts of the alveolar ridge, or of its contents, may throw it either in front of the lateral or behind the first bicuspid tooth.

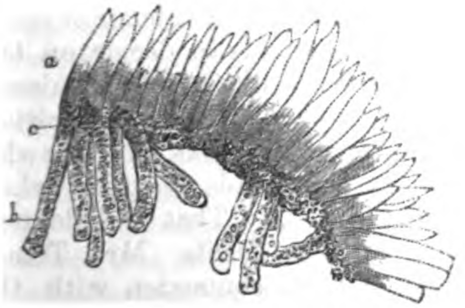
Under the head of *Irregularity in the number of the permanent teeth*, it is remarked that excess is more common than deficiency. The supernumerary teeth may be situated in either jaw, though more frequently in the upper than in the lower, and in every part of the alveolar arch; the neighbourhood of the incisors, however, is their ordinary position. In form, they may either resemble those of special members of the normal series, or may assume a somewhat irregular conical shape, sufficiently characteristic to be at once recognised as supernumerary teeth. To the former class, the author suggests the term supplemental, in contradistinction to that of supernumerary. The characters of the latter are then described. In treating of *Irregularity in the forms of the permanent teeth*, instances are given of the excessive or diminutive size occasionally met with; others, again, in which the special form of a member of the series may be altogether lost, so much so that the mass representing it would scarcely be suspected to be a tooth. A case is mentioned in which the second molar of the lower jaw was represented by an irregularly flattened mass composed of enamel, dentine, and cementum, thrown together without any definite arrangement; and another of an upper tooth in which a cavity is formed external to the pulp cavity which communicates with the surface of the tooth, and which is lined with a thin layer of somewhat imperfectly developed enamel. Where one of the series is imperfect, the author considers that such imperfection must be attributable to a local cause, such as the prolonged existence of a gum-boil in connexion with a temporary tooth; or the encroachment of a neighbouring tooth upon the formative pulp may lead to the formation of a dwarfed and misshapen tooth.

Interrupted development of the dental tissues is stated to arise from a constitutional, and not a local, cause, and to be characteristic of the condition of system at the period the imperfect portions were formed. The character of both enamel and dentine under this condition are explained as revealed by the microscope. In such cases, the temporary teeth have nothing to do with the character of the permanent. Allusion is made to a condition described by the author under the head *Dilaceration*, in his "Lectures on Dental Physiology," the result of displacement of the calcified portion of a tooth from the tissues which were instrumental in its production, the development being continued after the normal position of the calcified part had been lost. In such cases, the dental tubes are greatly bent or disturbed in their course at the point of injury. The relations of the enamel, the dentine, and of the cementum are, also, interfered with at a corresponding point.

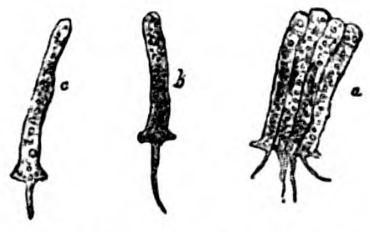
We pass over the author's remarks on the number and size of the roots, to consider the union or gemination of contiguous teeth, the next subject treated of. A distinction is drawn between cases of congenital union which must have been effected through the medium of their respective pulps prior to the development of the teeth themselves, and those in which teeth placed in close apposition become united by the large development of cementum (exostosis) consequent upon disease.

Under the head of irregularity of the permanent teeth, the last subject treated of is *Irregularity in the period of their eruption.* The teeth most subject to it are stated, and an inquiry is made into the condition of the teeth themselves at the period of eruption, also into the nature of the process of eruption in these exceptional cases. This is the last subject treated of under the head of *Teething*, which concludes one division of the work. As will appear, the consecutive changes in the teeth and jaws, which in the healthy subject keep pace with the general growth of the body, have to some extent been traced, and the results which are entailed when the development of those parts is interfered with pointed out.

The second division of the work begins with an account of the dental tissues. Commencing with the enamel, the author combats Mr. Huxley's views with regard to its development, and proves to demonstration the conversion of the columns of the enamel pulp into the fibres of the perfect tissue; the first of the accompanying illustrations is intended to exhibit the columns of the enamel pulp connected with the



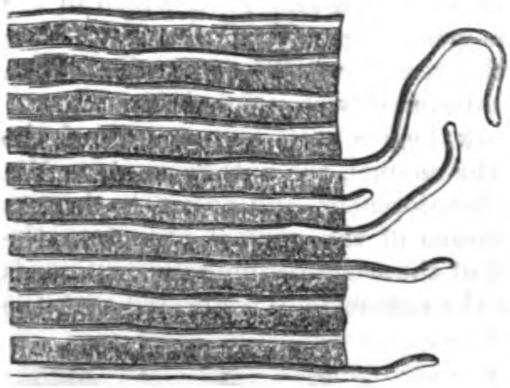
The columns of the enamel pulp *b*, connected at *c* with the decalcified enamel fibres at *a*.



a, bundle of columns of the enamel pulp, adherent to each other by the ends which approach the enamel; *b* and *c*, detached columns of the enamel pulp terminated by delicate processes, with circumferential dilatations above the point from which the process starts.

decalcified enamel fibres; and the second shows that many of the columns are terminated by delicate processes, which must at the time of separation have been withdrawn from the interior of the partially calcified fibres. The author believes that each column is composed of a delicate sheath, in which are enclosed one or more nuclei, the interspaces being occupied by transparent granular matter: he points also, as corroborative evidence of the correctness of the conversion theory, to the fact of the columns of the enamel pulp being similar in size to the developing enamel fibres, also to the position and direction of the one having that which will be assumed by the other. Mr. Tomes asks, why it is that we have cohesion at the point of junction of the columns and fibres, when the columns above and the fibres below so readily separate from each other, and from the coherent part? (the so-called membrane). He suggests a chemical solution of the question, and thinks that this part may have undergone a chemical change preparatory and necessary to calcification, and that it is thereby rendered cohesive; he considers, however, that inasmuch as the demonstration of the so-called membrane is only possible by the means of reagents, its very existence must be considered equivocal. A full description is then given of the development of the enamel.

The principal point of interest in reference to the dentine, is the permanent occupation of the tubes by a soft fibril, which, after passing from the pulp into the tube, follows its ramifications. The mode of procedure by which Mr. Tomes made this discovery is then explained; and two illustrations given, one, which we insert, showing the fibrils extending from the edge of the dentine; the other showing the fibrils in connexion with the pulp, after the dentine has been removed; also the fibrils passing from the pulp into the tubes of the dentine, and emerging on the surface of the latter tissue. These fibres the author has traced into the enamel; he expresses a doubt whether they are tubular or solid, and is unable to decide on the manner in which they terminate in the pulp. Their recognition however, he thinks, explains why under certain circumstances, dentine is susceptible of pain, while under other conditions the sensitiveness is lost. That the dentine owes its sensation to the presence of the fibrils, Mr. Tomes thinks cannot be doubted, seeing that if their connexion with the pulp be cut off by the destruction of the latter, all sensation is at once lost. The greater degree of sensitiveness observable in the dentine below the enamel—that is at the point of ultimate distribution of the tubes, and consequently of the fibrils—may, the author thinks, be fully accounted for on the supposition that the latter are organs of sensation, and subject to the same laws as nerves of sensation, the



A section from the crown of the tooth of an adult, made in a plane with the direction of the dentinal tubes, and afterwards decalcified and then torn in a line transverse to the direction of the tubes. The fibrils are shown extending from the torn edge.

highest sensibility of which is confined to their terminal branches; it is supposed, however, that fluids may and do pass through or by the side of the fibrils, and that when these become calcified near the surface of the dentine, the hardening materials must have been derived from the pulp. Thus the fibrils are subservient not only to sensation, but are also the channels by which nutrition is carried on in the dentine. It is suggested that the absorption or diminution of the pulp, in order to give space for the developing dentine, may correspond with that process by which the tissues of a temporary tooth disappear before the growing papilla. An illustration is given of the dentinal cells adherent to the developing dentine, and the continuity of the calcified and uncalcified tissues clearly shown. Another drawing shows these cells detached, and it is demonstrated that under certain conditions of caries the formative cells the outlines of which had been perfectly obliterated by calcification, are, during the progress of disease, again rendered apparent. The author appears to favour Mr. Rainey's views on the subject of calcification.

The subject of caries is very amply discussed, together with all the practical questions of operative dentistry which it involves. This section gives us a satisfactory impression with regard to the advances in the scientific and manual branches of dental science, but it need not now detain us.

We pass on to the next subject which occupies our author's pen, and to which he devotes eighteen pages,—viz., *Dental Exostosis*. The disease is defined as "an addition of tissue normal in character, but abnormal in amount to a pre-existing tissue of the same structural character." In exostosis the cementum is described as thickened either locally or generally, the dentine in no case participating in the enlargement. Occasion is here taken to give a description of the structure of this tissue, and its analogy to bone is pointed out. The author states that a distinction must be drawn between those cases in which the affection is consequent upon pre-existing disease in the teeth followed by marked irritation of the alveolar membrane, and those in which it is developed independently of any other disease. Two cases are mentioned in which epilepsy was consequent upon diseased teeth, the most prominent feature being exostosis of the roots. It is concluded that extraction of the diseased tooth is the only remedy which can be depended upon. *Necrosis*, which is next examined, is defined as a loss of the vitality of a part or the whole of a tooth. The disease involves the death, but not necessarily the decomposition of the dead part, the tissues of which become dissolved, but are seldom softened. The dentine may lose its vitality in consequence of the pulp having been destroyed, and yet the cementum may retain its connexion with the periosteum. The connexion affords the means by which the tooth may retain its place for an indefinite period. Where additions are made to the cementum in this affection, as is often the case, there is a difference in its character and that of the original cementum. The affection may be confined in the first instance to the cementum, the dentine and pulp retaining their normal relations. Any attempt to restore vitality being useless, extraction is the only remedy. Under the head *Absorption of the roots of permanent teeth*, instances are mentioned, firstly, in which the whole or part of the root of a sound permanent tooth is absorbed without reference to the growth of an adjoining tooth, and secondly, those cases where a portion of a permanent tooth is absorbed to make way for the eruption of a neighbouring tooth. Twenty-two pages are devoted to *Diseases of the pulp* and their treatment; the former are divided into irritation, and acute and chronic inflammation. Irritation, if long continued, has a tendency to produce calcification of the pulp. Acute inflammation of the pulp, which is generally caused by exposure, and irritation from foreign bodies, ends in the death of the pulp. Chronic inflammation of the pulp differs from acute in the less active character of the symptoms, and in the results to which it leads; the pain is seldom long continued or very intense, and comes on generally at irregular intervals. The inflammation is limited to the exposed part, whereas in the acute form the whole substance of the pulp is affected. The exposed portion is changed in character, and becomes an organ of secretion, purulent or serous fluid being poured out from its surface. In this abnormal condition, pain is not a necessary consequence; polypus of the pulp may ensue, which is not necessarily very sensitive, or the pulp may disappear gradually without pain.

The subject of the next chapter is divided into general inflammation of the alveolar membrane affecting the sockets of the majority of the teeth equally, and dependent for its origin upon a constitutional condition, and local inflammation involving the sockets of one or two teeth; the latter is again subdivided into acute and chronic. In general inflammation a disposition is provoked to grind the teeth forcibly together, the pressure of the teeth into their sockets gives temporary relief—the teeth are very sensitive to pressure, and feel lengthened and loosened, and cannot be used in mastication without pain. The disease extends from the inner to the outer covering of the sockets and gums. In severe cases suppurative action is established, which may lead to absorption of the alveoli and loss of the teeth, or be succeeded by necrosis of the alveolar margin. The treatment must be addressed to the improvement of the general health, aided with local remedies, which are described. Acute inflammation is a consequence of pre-existing disease. The symptoms are slight uneasiness and tension; pressure of the fang into the jaw gives relief, but the uneasiness returns on the pressure being withdrawn—a dull heavy pain follows, and the tooth feels to be longer than its fellows. Alveolar abscess follows. The treatment consists in local bleeding, or extraction of the tooth, and when suppuration has taken place, in the liberation of the pus by the lancet. Chronic inflammation of the dental periosteum may be caused by a collection of tartar, or a ligature about the neck of a tooth, or pressure in an oblique direction by an antagonistic tooth; the obvious treatment is the removal of the exciting cause.

In the remainder of the work the author successively discusses "Absorption of the alveoli," "Hypertrophy of the alveolar portions of the jaw," "Necrosis of the alveolar processes," and a number of other subjects possessing much practical interest. We have no doubt that these parts, together with the admirable illustrations which accompany them, will prove as attractive, as they are instructive, to those who have recourse to Mr. Tomes's work. The name of this writer would, from his antecedents alone, suffice to raise great expectations of any work coming from his pen, and such expectations will not be disappointed by a perusal of the volume before us. His labours and previous writings on the subject which he has so vigorously and successfully prosecuted, justly entitle him to the highest rank as a physiologist, and his last publication will add another to his already well-deserved honours. The early portion of the work is characterized by minutiæ of observation, the results of which are detailed with great ability and clearness. The commencement of the second part treats as fully of the structure and development of the teeth as would be consistent with the character of a manual, while opportunity is taken to communicate the anatomical and physiological discoveries made by the author since the publication of his former work. The remainder of the volume is eminently practical, constituting a text-book to which reference on every point of practice may readily be made, thus supplying a want which has been long felt.

The whole work is most carefully got up in every respect, and it is impossible to speak too highly of the admirable manner in which Mr. Bagg, the artist, has performed his share in the work.

Review IV.

Guy's Hospital Reports. Edited by Samuel Wilks, M.D., and Alfred Poland. Third Series. Vol. V.—*London,* 1859. pp. 391.

In the present volume of the 'Guy's Hospital Reports' we find, as usual, and, as would seem almost unavoidable, considering the field from which they are gathered, much that is valuable and instructive. We are presented with twenty-two different papers, and the volume contains numerous well-executed plates and woodcuts, which are pretty equally distributed among the surgeons and the physicians, while the number of communications by the latter very considerably preponderate over those contributed by the former.

We proceed to give a brief summary of the individual papers.

I. *Contraction of Œsophagus from corrosive Poison.—Gastrotomy.* By J. Cooper Forster.—The gradual contraction of the œsophagus in a boy, aged four years and four months, following the accidental deglutition of a caustic bleaching fluid, seventeen weeks before admission under Dr. Addison's care, left no hopes of recovery except by surgical aid. Accordingly, twelve days after his entry into Guy's, the operation was performed. An incision was made at the outer edge of the rectus muscle in the left hypochondrium, commencing at the cartilages and opposite the space between the seventh and eighth ribs. After some little delay, the greater curvature of the stomach was reached and divided, and the edges stitched carefully to the abdominal parietes by an uninterrupted suture. Every quarter of an hour for the first two hours half an ounce of milk and egg and egg and wine were introduced into the stomach by the wound alternately. Subsequently the intervals of feeding were prolonged. The child did very well till the fourth day after the operation, when, as appeared at the autopsy, owing to some of the sutures giving way, a portion of the contents of the stomach escaped into the abdominal cavity, peritonitis immediately ensued, to which the patient succumbed as to shock.

We think, with the author of the paper, that though the operation in this case was not ultimately successful, the fatal issue was comparatively accidental, and the circumstances attending it sufficiently encouraging to justify a repetition of the procedure in similar cases.

II. *The Injuries and Diseases of the Nervous System.* By THOMAS BRYANT.—The present is the first of a series of papers in which the author proposes to make his readers thoroughly able to understand the principles which have guided the surgeons of a large metropolitan hospital in their practice, and to show that this practice is based upon scientific and pathological inquiries. Mr. Bryant bases the observations now before us upon the experience of the last four years, including 425 cases, 242 of which were cured, 95 relieved, and 88 proved fatal. We take the subjects in the order adopted by the author.

1. *Scalp-wounds*, 116, all cured; only 2 were followed by erysipelas, and in 1 there was troublesome bleeding due to hæmophilia. The treatment was throughout of the simplest kind.

2. *Concussion.*—This is considered in the simple and the complicated form. In the former, of which there were 56 cases (all cured), 8 with a scalp-wound, there was only a temporary interruption or suspension of the cerebral functions. The author dwells upon the fact that vomiting in these cases generally shows itself upon the first appearance of reaction, and regards it as the result of a more active circulation through the cerebral centres. He observes that, in some cases, after a partial return of consciousness, there is a relapse into the insensible condition, which he denominates relapsing unconsciousness, and which the surgeon must not regard as the result of a serious encephalic lesion, because it is not dangerous nor indicative of any definite complication. Convulsions, severe headache, inflammatory symptoms, and squinting, are also observed in simple concussion during the period of reaction; illustrative cases are given of each variety. Of the complicated variety there were 26 cases, two of which were fatal; in the first, a girl, aged four, who was knocked down by a packet of hops falling upon her, death ensued 60 hours after, during which time there was no return of consciousness. There was no fracture, but the brain was found bruised all over, especially towards the anterior lobes and upon its upper surface, and at its base the anterior and middle lobes were likewise ecchymosed. There was also extravasation of blood at these parts; the ventricular fluid was pinkish, and the parts around ecchy-

mosed. The second case was that of a man, who in a state of drunkenness was thrown against a kerbstone. Partial recovery from the immediate effects took place, but epileptiform seizures followed. On the eighth day after the injury he was attacked with delirium tremens, from which in its turn he appeared to recover after ten days' treatment; but he again became comatose for seven days, from which state he recovered three hours before his death. The autopsy showed a layer of blood over the whole surface of the brain: on the right side there was a thick clot, which "had evidently been effused for some days." There was red softening of a portion of the left hemisphere, and upon the anterior lobe of the right side; the latter "evidently the result of *contre coup.*" No injury of the vessels could be detected, so that the idea of apoplexy having taken place was negatived. In illustration of the fact of apoplexy as a result of diseased vessels being the cause of death in some cases of concussion, Mr. Bryant brings forward a case, which, from not being included in his tabular summary, we conclude to have occurred previous to the quinquennial period from which they are taken.

3. *Fractures of the Cranium,* 73 cases; of which 49 were fatal, and 24 cured. Many of them are given in detail, and are of considerable interest. We extract some of the conclusions drawn by the author:—

"1. That fractures of the skull, although attended with depressed bone, if uncomplicated with any severe concussion of the brain, and therefore with any cerebral injury, and if the dura mater remains uninjured, as a rule terminate successfully. 2. That simple fractures of the skull associated with depression, if unattended with marked symptoms of compression, are to be left alone. 3. That compound fractures of the skull, attended with depression and comminution of the bones, are to be treated by the removal of the loose portions; and if symptoms of compression exist, and the bones cannot be removed without the use of the trephine, that instrument is to be employed, although with great care, as it would appear that severe injuries to the cranium may recover as long as the cerebral hemispheres are uninjured."

The danger, in fact, depends upon the extent of the injury done to the intracephalic contents; the prognosis is always unfavourable when the dura mater or the brain-structure have suffered. The external wound and the fracture of the skull may have healed, and yet the fatal issue result from the secondary inflammation set up in the brain or membranes. Of fractures of the basis of the skull Mr. Bryant has 30 examples, of which the large number of 12 recovered.

"In three cases the fracture extended through the orbit, as indicated by subconjunctival ecchymosis. In eight examples there was hæmorrhage from the ears; in all this was followed by a discharge of serum, and in seven cases associated with paralysis of the facial nerve upon the same side. In two instances there was bleeding from the nose. In one case there was a serous discharge from the ear, accompanied with paralysis of the facial, and in one following hæmorrhage from the ear, but unaccompanied with paralysis."

The value of these lesions as diagnostic aids was proved by the postmortems of the fatal cases, to which, however, we must refer our readers. The remaining sections of this valuable paper are devoted to the injuries and diseases of the spine. We must confine ourselves to the following numerical statement. The total number of cases was 150, which were distributed as follows:—

Concussion of the spine	22, all cured.
Dislocation „	6, all fatal.
Fracture and dislocation	3, relieved; 15, fatal.
Lateral curvature	5, „
Diseased spine, cervical	15, „
„ dorsal and lumbar .	19, „
Spinal abscess, cervical	1, „
„ psoas	22, relieved; 3, died.
„ lumbar	18, „ 2 „
„ gluteal	1, „ 1 „
Spinal paralysis „	11, „ 6 „

III. *Cases of Ruptured Uterus during Parturition.* By J. Braxton Hicks, M.D., Lond., F.A.S.—Dr. Hicks classes this accident under three heads. 1. Laceration of the cervix, with or without a portion of the vagina. 2. Laceration of the fundus and cervix. 3. Lacerations of sufficient size to allow of the escape of the uterine contents. The first variety may not prove at all formidable; but, as in a case given, may be fatal by hæmorrhage. The second class is much more serious, but even here recovery may ensue; while, on the other hand, death may result where no rupture has been suspected. The third class is necessarily the most serious. In 43,880 cases, obtained partly from private sources, and partly from 'Guy's Hospital Reports,' Dr. Hicks finds that there were 9 cases of undoubted rupture, all of which died; 7 doubtful cases, of which 6 were fatal; but he gives us no information as to the frequency of the rupture of the cervix; nor does he advert to those cases in which, after escape of the child from the uterus, the system adapts itself to the new condition, the rent heals, and the fœtus is converted into a lithopædion, or discharged in various ways.

IV. *Cases of Morbus Addisonii, Melanœmia, Anœmia Idiopathica, Leucocythemia Splenica et Lymphatica.* By Samuel Wilks, M.D.—This paper gives a critique of Dr. Addison's views with regard to suprarenal diseases and its connexion with the remarkable deposit of pigment in the rete mucosum which accompanies it. Dr. Wilks considers the objections which some writers have brought against Dr. Addison's doctrines, and shows that the latter are perfectly consistent and have not been disproved. He republishes a case of morbus Addisonii, and adds some new ones illustrative of simple anæmia which proved fatal without organic disease. The remarks on melanæmia are quoted from Frerich's work on the liver, in illustration of which Dr. Wilks brings forward two cases that were observed and published by Mr. Bright, in his 'Medical Reports.'

V. *Sanguineous Meningeal Effusion (Apoplexy), spontaneous and from injury.* By Samuel Wilks, M.D.—This is a collection of eight cases of effusion of blood within the cranium, given to illustrate the occasional difficulty of determining the diagnosis between apoplectic effusion, the effects of poison or the results of injury. The cases will not bear further condensation.

VI. *Cases of Poisoning.*—Three by sulphuric acid, one by soap lees, one by alcohol, and one by Burnett's solution. The first case was that of a woman who, by mistake, swallowed a wine-glassful of Burnett's solution (chloride of zinc), causing death by perforation of the stomach in fourteen weeks. The remarkable feature of the case is the absence of all pain at the stomach from the commencement to the termination. During the first three days only, there was a burning sensation in the throat and chest; the only other symptoms were persistent vomiting and gradually increasing marasmus.

In the second case, a child, æt. one and a half year, died twelve hours after swallowing a mouthful of soap lees. The pharynx and œsophagus were slightly swollen and of a yellowish brown hue, the stomach itself being quite unaffected.

The third case was accidental poisoning of a man by drinking a dessert-spoonful of oil of vitriol. Death on the fourth day, the effects being produced in the œsophagus and stomach. The latter was softened throughout, but not perforated; its contents were not acid, nor was any poison discoverable.

In the fourth case, death resulted in eleven days in a woman, æt. fifty-five, from drinking a wine-glassful of dilute sulphuric acid on an empty stomach. Blood was vomited and passed by stool at first, followed by a kind of dysenteric diarrhœa, but there was no pain throughout. The prominent parts of the fauces and œsophagus were inflamed and invested by a brown granular lymph. The whole mucous membrane of the stomach was sloughed off. Excepting slight ulceration in the duodenum and some fibrinous patches in the lower part of the ileum, the small intestine was healthy. The whole mucous membrane of the large intestine was brownish grey, and its surface rough, from the exudation of a granular lymph.

The fifth case was also one from the accidental administration of sulphuric acid to an infant of seventeen months. Death resulted in twenty-one hours from œdema of the submucous tissue surrounding the top of the larynx. The acid had also reached the stomach, the greater part of which presented the usual charred appearance.

The last two cases illustrate the effects of sulphuric acid and nitric acid, taken accidentally, on the mouth only. Both patients recovered under suitable treatment.

VII. *Description of some new Wax Models, illustrating several Cases of Elephantiasis, Diseases of the Nails, &c.*—Among these descriptions, we notice one of a case of lepra tuberculosa occurring in a man who had never been out of Great Britain. We are able to corroborate the fact of elephantiasis occurring in persons not affected with any tropical taint, as we have ourselves met with two cases, a female and a man, who suffered from the disease in the legs. Mr. Cooper Forster also publishes an account of the effect produced upon the finger and thumb nails of the students and demonstrators engaged upon the dissection of bodies which had been injected with a saturated solution of common white arsenic. We abridge the following account given by one of the sufferers:—An inflammatory action was caused beneath the finger nails, these parts and the tips becoming very painful, and pus forming under the nails, except those of the thumbs. The employment of india-rubber shields protected the fingers from further irritation, but the nails, from the decomposition of the pus, assumed a dirty appearance, and were almost entirely separated from the subjacent parts. The nails subsequently arched over the ends of the fingers without adhering to them. Portions of the nails were submitted to analysis by Dr. Taylor, and very distinct traces of arsenic found. The chemical examination was made three months after the attack. The symptoms first described occurred in the person of Mr. Cann; others suffered similarly, but not so severely.

VIII. *Malposition of the Abdominal Viscera, in relation to the Causes and Diagnosis of Disease.* By S. O. Habershon, M.D.—In addition to a brief description of a case (illustrated by a plate) in which all the thoracic and abdominal viscera were completely transposed, this paper contains some remarks on the changes produced in the site of the abdominal by pathological conditions, tending "to show the uncertainty of resting *solely* on their normal site as our guide in diagnosis."

IX. *Gunshot Wound in the Loins. Fracture of the lamina of the vertebra; perforation of the descending colon; fecal abscess; large white kidneys; albuminuria. Death nearly four years after the injury.*—The main point in this case is the fact, that the patient's death was due chiefly to the disorganization of the kidneys set up a year after reception of the injury, the latter having given rise to a fecal fistula in the left lumbar region.

X. *Cases of Pyæmia.* By S. O. Habershon, M.D.—These are two cases, the one of pemphigus, the other of pneumonia, in which death ensued under symptoms resembling those of pyæmia; but there is no satisfactory evidence, post-mortem or other, to prove the correctness of that view.

XI. *Cases illustrative of the Treatment of Rheumatic Fever.* By G. Whitley, M.D.—The author compares twenty-three cases of rheumatic fever of the acute fibrous kind treated by the physicians of Guy's Hospital.

"In fifteen of these cases salts of potash were given, either at first or after the failure of other means. When cardiac complications existed, these remedies were combined with calomel, antimony, and opium, and with blisters; while in some of the simple cases of joint affection they were administered quite alone. In no case did they fail to effect a cure, which, even in severe cases, was sometimes attained rapidly. The latter remark applies, indeed, to some recent acute cases treated with lemon-juice, but in many instances this remedy unfortunately proves ineffectual."

Dr. Whitley also quotes the experience of Dr. Hermann Weber as to the use of large doses of bicarbonate of soda in this disease. In five cases, six to eight drachms, given in the twenty-four hours, caused a speedy alleviation of symptoms; the general conclusion, however, is, that it yields in efficacy to the preparations of potash.

XII. *Lesions of the Nervous System producing Diabetes.* By F. W. PAVY, M.D.—In the last volume of the 'Guy's Hospital Reports,' of which we gave an analysis in our number for January, 1859 (p. 171), Dr. Pavy expressed opinions regarding glycogenesis, which differed from those he had previously upheld. He there maintained that the sugar found in the blood of the right side of the heart of a dead animal was a post-mortem effect, because in a large number of analyses of the blood taken from the right heart of living animals only the merest trace of sugar could be detected. The present paper contains the results of numerous experiments and vivisections, performed with a view to still further elucidating the question. The author considers them to be completely corroborative of his views last enunciated. The following are the lesions of the nervous system, artificially produced, which he has found productive of saccharine urine:—Division of the lower part of the medulla oblongata; destruction of the medulla oblongata and decapitation, followed in each case by artificial respiration; division of the ascending branches of the superior thoracic ganglion on both sides. When these were successively divided, the diabetes was much increased after the division of the nerve on the second side. Deligation of the carotid and vertebral arteries gave opposing results; saccharine urine resulting in some cases, and not in others. When the contents of the vertebral canals alone were divided, no diabetes resulted; whereas, when after deligation of the vertebrals and division of the contents of the vertebral canals no diabetes followed, this was at once produced on the subsequent deligation of the carotids. Again, ligature of the carotids with destruction of the parts in front and behind the transverse processes of the atlas near the vertebral foramen caused saccharine urine. Removal of the carotid ganglion of the sympathetic, singly or on both sides, equally induced diabetes; division of the thoracic sympathetic induced contradictory results, while diabetes followed removal of the superior cervical ganglia. So that Dr. Pavy gives us satisfactory evidence of saccharine urine being brought about, though probably only temporarily, by lesions of certain parts of the sympathetic.

XIII. *Contributions to the Practical Surgery of New Growths or Tumours.* Series III., *Cysts.* By JOHN BIRKETT.—Mr. Birkett devotes this paper to the cysts of the cutaneous covering, and introduces his subject by saying that, "with the exception of the cysts developed around foreign bodies and entozoa, all of those which are developed in the skin depend upon a morbid process taking place in the glandular structures." Unless some destructive action have occurred in the cysts, the latter always show the anatomical elements of the gland, and by careful examination the trace of the original orifice may be discovered. Mr. Birkett, in illustration of his view, brings forward a considerable number of cases, first, of cysts in relation with the glands which secrete sebaceous matter forming in various parts of the body, as the cheek, the back, the scalp, the mamilla, and elsewhere; and, secondly, of cysts in relation with the glands which secrete mucus, as in the lips, under the tongue (ranula), or in the vagina.

XIV. *Cases of Aneurism of the Cerebral Vessels.* By W. GULL, M.D.—The author is of opinion that intracranial aneurism is more frequent than it was formerly supposed to be, owing to the greater care with which autopsies have been made during the last ten years. Recent coagula may conceal the sac after rupture has occurred, and it will not be found unless very carefully looked for; as it is often small, thin, and transparent, except at the point of rupture.

"Further, also, when death has taken place from changes around the aneurism, as by pressure or softening, the sac itself may present such appearances that, unless a minute dissection be made of it, its true nature may not be discovered. Whenever young persons die with symptoms of ingravescent apoplexy, and after death large effusion of blood is found, especially if the effusion be over the surface of the brain in the meshes of the pia-mater, the presence of an aneurism is probable."

Dr. Gull analyses 25 cases of intracranial aneurism, collected from various sources, and arranged according to the seat of the lesion, and adds 6 more cases that have fallen under his own observation. The general conclusion at which he arrives is, that although we may "from the circumstances sometimes suspect the presence of aneurism within the cranium, we have, at best, no symptoms upon which to ground more than a probable diagnosis."

We may observe, in confirmation of the remark of Dr. Gull, that cerebral aneurism prevails earlier in life than ordinary apoplexy; that we find the average age of the 31 cases upon which he bases his remarks, to be $21\frac{6}{31}$ years.

XV. *On Retroversion of the Aortic Valves, from Disease in the Sinuses of Valsalva.* By WILLIAM GULL, M.D.—This paper is written to show that atheromatous rigidity of the sinuses of Valsalva may induce retroversion of the valves of the aorta by diminishing the distension of the sinuses during the recoil of the blood, and thus depriving the valves of the necessary support.

XVI. *On the Destructive Changes in the Lung from Diseases in the Mediastinum, invading or compressing the Pneumogastric Nerves and Pulmonary Plexus.* By WILLIAM GULL, M.D.—Three cases are related in this communication to show that whatever may be the result of section of the pneumogastric nerves in the neck, when the nerves are injured by disease at the root of the lung, gangrenous pneumonia results; because, not only are there large branches from the sympathetic at that part, injury of which would have an influence upon the morbid processes, but because the branches going to either lung may be individually implicated, and consequently the corresponding lung be directly deprived of its nervous supply. The first case is one of aneurism of the arch of the aorta pressing upon the left vagus and upper part of the pulmonary plexus, followed by sloughing pneumonia of the left lung; the second, consolidation and incipient gangrene of the right lung, following cancer of the œsophagus, involving the right vagus and branches of the pulmonary plexus, behind the right bronchus; the third, implication of the right vagus and branches of the pulmonary plexus in a fibrous growth in the mediastinum surrounding the right bronchus, inducing consolidation and gangrene of the corresponding lung.

XVII. *On Factitious Urticaria.* By WILLIAM GULL, M.D.—In some persons the irritability of the skin is such that slight friction induces appearances resembling those of urticaria, which Dr. Gull attributes to the muscular fibre cells of the skin; hence, where these are most numerous the phenomenon most readily presents itself. The author adduces an instance in which it was unusually marked.

XVIII. *Contributions to Dental Pathology.* By J. JAMES A. SALTER, M.B., F.L.S., F.G.S.—The author presents us with five well-illustrated cases in which permanent teeth were impacted in the substance of the maxillary bones of adults in whom the temporary teeth were found present and firm. He adds an interesting instance of a capsule of a tooth expanded into a serous cyst occupying the superior maxilla, but of which there was no indication in the external form of the bone.

XIX. *Cases of Injured Arteries.* By ALFRED POLAND and CONSTANTINE HOLMAN, M.D.—The first of these cases is one of traumatic aneurism of the ulnar artery in the palm, treated ineffectually by ligature of the artery to and from the sac, but cured by ligature of the radial. The second is one of ligature of the ulnar artery to arrest hæmorrhage from a wound in the radial, which had been treated to no purpose by ligature at both ends of the latter. In the third case, after operation for necrosis of the head of the fibula, aneurism of the anterior tibial formed, which was ligatured ineffectually at the seat of injury, but was cured by ligature of the femoral. A fourth case of a different nature is appended: after old dislocation of both hip-joints into the sciatic notch from strumous disease, the head of the right femur separated spontaneously, and was removed from an abscess below Poupart's ligament.

XX. *On some Points in the Diagnosis and Treatment of Heart Disease.* By G. H. BARLOW, M.D. XXI. *Cases illustrative of the*

Etiology of Enlargement of the Heart. By G. H. BARLOW, M.D.—In the first of these communications the author dwells upon the phenomena characterizing aortic and mitral valvular diseases respectively. In the second, Dr. Barlow gives the particulars of 18 cases of cardiac hypertrophy, which he refers to the second and third of the following classes, to which he reduces the conditions that contribute to the production of this morbid condition.

1. Obstruction from changes in the orifices of the heart, or in the course of the circulation.
2. Obstruction arising from changes in the quantity or physical properties of the blood.
3. Deficiency of strength in the parietes of the heart itself.

XXII. *On some Failures of Marsh's Process for the Detection of Arsenic.* By WILLIAM ODLING, M.B., F.R.S.—The object of this paper is to prove that Marsh's process cannot be relied upon to detect minute quantities of arsenic in the presence of organic matter.

REVIEW V.

The Emotions and the Will. By ALEXANDER BAIN, A.M., Examiner in Logic and Moral Philosophy in the University of London.—*London,* 1859. 8vo, pp. 646.

AFTER the controversy between the Neptunists and the Vulcanists had been long carried on without definite results, there came a reaction against all speculative geology. Reasoning without adequate data having led to nothing, inquirers went into the opposite extreme, and for a length of time confining themselves wholly to collecting data, relinquished reasoning. The Geological Society of London was formed with the express object of accumulating evidence; for many years hypotheses were forbidden at its meetings; and only of late have attempts to organize the mass of observations into consistent theory been tolerated.

This reaction and subsequent re-reaction well illustrate the recent history of English thought in general. The time was when our countrymen occupied themselves, certainly to as great an extent as any other people, with all those high questions which present themselves to the human intellect; and, indeed, a glance at the various systems of philosophy that are or have been current on the Continent, suffices to show how much other nations owe to the speculation of our ancestors. For a generation or two, however, these more abstract subjects have fallen into neglect, and, among those who plume themselves on being "practical," even into contempt. Partly, perhaps, a natural accompaniment of our rapid material growth, this intellectual phase has been in great measure due to the exhaustion of argument, and the necessity for better data. Not so much with a conscious recognition of the end to be subserved as from an unconscious subordination to that rhythm traceable in social changes as in other things, an era of theorizing without observing, has been followed by an era of observing without theorizing. During this long-continued devotion to concrete science, an immense quantity of raw material for abstract science has been accumulated; and now there is obviously commencing a period in which this accumulated raw material will be organized into consistent theory. On all sides—equally in the inorganic sciences, in the science of life, and in the science of society—may we note the tendency to pass from the superficial and empirical to the more profound and rational.

In Psychology this change is conspicuous. The facts brought to light by anatomists and physiologists during the last fifty years are at length being used towards the interpretation of this highest class of biological phenomena; and already there is promise of a great advance. The work of Mr. Alexander Bain, of which the second volume has been recently issued, may be regarded as especially characteristic of the transition. It gives us in orderly arrangement the great mass of evidence supplied by modern science towards the building up of a coherent system of mental philosophy. It is not in itself a system of mental philosophy, properly so called; but a classified collection of materials for such a system, presented with that method and insight which scientific discipline generates, and accompanied with occasional passages of an analytical character. It is indeed that which it in the main professes to be—a natural history of the mind. Were we to say that the researches of the naturalist who collects and dissects and describes species, bear the same relation to the researches of the comparative anatomist tracing out the laws of organization, which Mr. Bain's labours bear to the labours of the abstract psychologist, we should be going somewhat too far; for Mr. Bain's work is not wholly descriptive. Still, however, such an analogy conveys the best general conception of what he has done; and serves most clearly to indicate its needfulness. For as, before there can be made anything like true generalizations respecting the classification of organisms and the laws of organization, there must be an extensive accumulation of the facts presented in numerous organic bodies; so, without a tolerably complete delineation of mental phenomena of all orders, there can scarcely arise any adequate theory of the mind. Until recently, mental science has been pursued much as physical science was pursued by the ancients: not by drawing conclusions from observation and experiment, but by drawing them from certain arbitrary *à priori* assumptions. This course, long since abandoned in the one case with immense advantage, is gradually being abandoned in the other; and the treatment of Psychology as a division of natural history, shows that the abandonment will soon be complete.

Estimated as a means to higher results, Mr. Bain's work is of great value. Of its kind it is the most scientific in conception, the most catholic in spirit, and the most complete in execution. Not only does it delineate the various classes of mental phenomena as seen under that stronger light thrown on them by modern science; but it includes in the picture much which previous writers had omitted—partly from prejudice, partly from ignorance. We refer more especially to the participation of the various bodily organs in the mental changes, and the addition to these mental changes of those many secondary ones which the actions of the bodily organs generate. Mr. Bain has, we believe, been the first adequately to appreciate the importance of this element in our states of consciousness; and it is one of his chief merits that he shows how constant and large an element it is. Further, the relations of voluntary and involuntary action are elucidated in a way that was not possible to writers unacquainted with the modern doctrine of reflex action. Add to which, that some of the analytical passages that here and there occur contain important ideas.

Valuable, however, as is Mr. Bain's work, we regard it as essentially transitional. It fulfils the office of presenting in a digested form the results of a period of observation; adds to these results much well-delineated fact collected together by himself; arranges new and old materials with that more scientific method which the discipline of our times has fostered; and so prepares the way for better generalizations. But almost of necessity its classifications and conclusions are provisional. In the growth of each science, we may observe, that not only is correct observation needful for the formation of true theory; but that true theory is needful as a preliminary to correct observation. Of course we do not intend this assertion to be taken literally; but as a strong expression of the fact that the two must advance hand in hand. The first crude theory or rough classification, based on very limited knowledge of the phenomena, is requisite as a means of reducing the phenomena to some kind of order; and as supplying a conception with which fresh phenomena may be compared, and their agreement or disagreement noted. Incongruities being by and bye made manifest by wider examination of cases, there eventually comes such modification of the theory as brings it into a nearer correspondence with the evidence. This reacts to the further advance of observation. More extensive and complete observation brings additional corrections of theory. And so on till the truth is reached. In mental science, the systematic and extensive collection of facts having but recently commenced, it is not to be expected that the results can be at once rightly formulated. All that may be looked for are approximate generalizations which will presently serve for the better directing of inquiry. And hence, even were it not now possible to say in what way it does so, we might be tolerably certain that the work of Mr. Bain bears the stamp of the inchoate state of Psychology.

We think, however, that it will not be difficult to find in what respects its organization is provisional; and at the same time to show what must be the nature of a more complete organization. We propose here to attempt this: illustrating our positions from his recently issued second volume.

Is it possible to make a true classification without the aid of analysis? or must not there be an analytical basis to every true classification? Can the right relations of things be determined by reference to their more obvious characteristics? or does it not commonly happen that certain hidden characteristics, on which the more obvious ones depend, are the truly significant ones? This is the preliminary question which a glance at Mr. Bain's scheme of the emotions suggests.

Though not avowedly, yet by implication, Mr. Bain assumes that a true conception of the nature, the order, and the mutual relations of the emotions, may be arrived at by contemplating their conspicuous objective and subjective characters, as displayed in the adult. After pointing out that we lack those means of classification which serve in the case of the sensations, he says—

"In these circumstances we must turn our attention to *the manner of diffusion* of the different passions and emotions, in order to obtain a basis of classification analogous to the arrangement of the sensations. If what we have already advanced on that subject be at all well founded, this is the genuine turning point of the method to be chosen, for the same mode of diffusion will always be accompanied by the same mental experience, and each of the two aspects would identify, and would be evidence of, the other. There is, therefore, nothing so thoroughly characteristic of any state of feeling as the nature of the diffusive wave that embodies it, or the various organs specially roused into action by it, together with the manner of the action. The only drawback is our comparative ignorance, and our inability to discern the precise character of the diffusive currents in every case; a radical imperfection in the science of mind as constituted at present.

"Our own consciousness, formerly reckoned the only medium of knowledge to the mental philosopher, must therefore be still referred to as a principal means of discriminating the varieties of human feeling. We have the power of noting agreement and difference among our conscious states, and on this we can raise a structure of classification. We recognise such generalities as pleasure, pain, love, anger, through the property of mental or intellectual discrimination that accompanies in our mind the fact of an emotion. A certain degree of precision is attainable by this mode of mental comparison and analysis; the farther we can carry such precision the better; but that is no reason why it should stand alone to the neglect of the corporeal embodiments through which one mind reveals itself to others. The companionship of inward feeling with bodily manifestation is a fact of the human constitution, and deserves to be studied as such; and it would be difficult to find a place more appropriate than a treatise on the mind for setting forth the conjunctions and sequences traceable in this department of nature. I shall make no scruple in conjoining with the description of the mental phenomena the physical appearances, in so far as I am able to ascertain them.

"There is still one other quarter to be referred to in settling a complete arrangement of the emotions, namely, the varieties of human conduct, and the machinery created in subservience to our common susceptibilities. For example, the vast superstructure of fine art has its foundations in human feeling, and in rendering an account of this we are led to recognise the interesting group of artistic or æsthetic emotions. The same outward reference to conduct and creations brings to light the so-called moral sense in man, whose foundations in the mental system have accordingly to be examined.

"Combining together these various indications, or sources of discrimination, —outward objects, diffusive mode or expression, inward consciousness, resulting conduct and institutions—I adopt the following arrangement of the families or natural orders of emotion."

Here, then, we have confessedly adopted, as bases of classification, the most manifest characters of the emotions as discerned subjectively and objectively. The mode of diffusion of an emotion is one of its outside aspects; the institutions it generates form another of its outside aspects; and though the peculiarities of the emotion as a state of consciousness, may seem to constitute its intrinsic and ultimate nature, yet such peculiarities as are perceptible in an ordinary act of self-consciousness, must also be classed as in a sense superficial peculiarities. It is a familiar fact that various intellectual states of consciousness turn out, when analysed, to have a nature widely unlike that which at first appears; and we believe the like will prove true of emotional states of consciousness. Just as one concept of space, which seems on first consideration to be a definite, undecomposable concept, is yet resolvable into experiences quite different from that resulting state of consciousness which we call space; so, in all probability, the sentiment of affection or reverence is compounded of elements that are severally distinct from the whole which they make up. And much as a classification of our ideas which dealt with the idea of space as though it were ultimate, would be a classification of ideas by their externals; so is a classification of our emotions, which, regarding them as simple, describes their aspect in ordinary consciousness, a classification of emotions by their externals.

Thus, then, Mr. Bain's grouping is throughout determined by the most manifest attributes—those objectively displayed to us in the natural language of the emotions, and in the social phenomena that result from them, and those subjectively displayed in the aspects they assume in an unanalytical consciousness. And the question is—Can they be correctly grouped after this method?

We think not; and we believe that had Mr. Bain carried farther an idea with which he has set out, he would have seen that they cannot. As already said, he avowedly adopts "the natural-history-method:" not only referring to it in his preface, but in his first chapter giving examples of botanical and zoological classifications as illustrating the mode in which he proposes to deal with the emotions. This we conceive to be quite a philosophical conception; and have only to regret that Mr. Bain has overlooked some of its most important implications. For in what has essentially consisted the progress of natural history classification? In the abandonment of grouping by external, conspicuous characters; and in the making certain internal, but all-essential-characters the bases of groups. Whales are not now ranged along with fish, because in their general forms and habits of life they resemble fish; but they are ranged with mammals, because the type of their organization, as ascertained by dissection, corresponds with that of mammals. No longer considered as sea-weeds in virtue of their forms and modes of growth, zoophytes are now shown, by examination of their economy, to belong to the animal kingdom. It is found, then, that true classification involves analysis. It has turned out that the earlier classifications, guided by general resemblances, though containing a large amount of truth, and though very useful provisionally, were yet in many cases radically wrong; and that the real affinities of organisms, and the real homologies of their parts, were to be made out only by examining their hidden structures. Another fact of great significance in the history of classification is also to be noted:—we mean the fact that very frequently the true relations of organisms cannot be made out even by an exhaustive analysis, if that analysis is confined to the adult structure; but that in many cases it is needful to examine the structure in its earlier stages, and even in its embryotic stage. So difficult was it, for instance, to determine the true position of the Cirrhipedia among animals, so long as mature individuals only were examined, that Cuvier fell into the mistake of classing them with Mollusca, even after an anatomical examination of them; and it was only when their early forms were discovered, that they were clearly proved to belong to the Crustacea. So important, indeed, is the study of development as a means to classification, that the first zoologists now hold it to be the only absolute criterion.

Here, then, in the advance of natural-history classification, are two fundamental facts, which should be borne in mind in classifying the emotions. If, as Mr. Bain rightly assumes, the emotions are to be grouped after the natural-history method, then should it be the natural-history method in its complete, and not in its rude form. Mr. Bain will doubtless agree in the position, that a correct account of the emotions in their natures and relations, must correspond with a correct account of the nervous system—must form another side of the same ultimate facts. Structure and function must necessarily harmonize. Structures which have with each other certain ultimate connexions, must have functions that have answering connexions. Structures that have arisen in certain ways, must have functions that have arisen in parallel ways. And hence if analysis and development are needful for the right interpretation of structures, so must they be needful for the right interpretation of functions. Just as a scientific description of the digestive organs must include not only their obvious forms and connexions, but their microscopic characters, and also the ways in which they severally result by differentiation from the primitive mucous membrane; so must a scientific account of the nervous system include its general arrangements, its minute structure, and its mode of evolution; and so must a scientific account of nervous actions include the answering three elements. Alike in classing separate organisms, and in classing the parts of the same organism, the complete natural-history method involves ultimate analysis, aided by development; and Mr. Bain, in not basing his classification of the emotions upon characters reached through these aids, has fallen much short of the conception with which he set out.

"But," it will perhaps be asked, "how are the emotions to be analysed, and their modes of evolution to be ascertained? Different animals, and different organs of the same animal, may be readily compared in their internal and microscopic structures, as also in their development; but functions, and especially such functions as the emotions, do not admit of such comparisons."

It must be admitted that the application of these methods is in this case by no means so easy. Though we can note differences and similarities in the internal formations of two animals, it is difficult to contrast with any precision their mental states. Though the true morphological relations of organs may be made out by the observation of embryos, yet, where those organs are inactive before birth, we cannot completely trace the history of their actions. Further, it is obvious that the pursuance of inquiries of the kind indicated, raises questions which science is not yet prepared to answer; as, for instance—Whether all nervous functions, in common with all other functions, do not arise by gradual differentiations, as their organs do? Whether the emotions are not, therefore, to be regarded as divergent modes of action that have become unlike by successive modifications? Whether, as two organs which originally budded out of the same membrane have not only become different as they developed, but have also severally become highly compound internally, though externally simple; so two emotions, simple and near akin in their roots, may not only have grown unlike, but may also have grown involved in nature, though seeming homogeneous to consciousness. And here, indeed, in the inability of existing science to answer these questions which underlie a true psychological classification, we see most clearly how purely provisional any present classification is likely to be.

Nevertheless, even now, classification may be aided by development and ultimate analysis to a considerable extent; and the great defect in Mr. Bain's work is, that he has not systematically availed himself of them as far as possible. Thus we may, in the first place, study the evolution of the emotions up through the various grades of the animal kingdom: observing which of them are earliest and exist with the lowest organization and intelligence; in what order the others accompany higher endowments; and how they are severally related to the conditions of life. In the second place, we may note the emotional differences between the lower and the higher human races—may regard as earlier and simpler those feelings which are common to both, and as later and more compound those which are characteristic of the most civilized. In the third place, we may observe the order in which the emotions unfold during the progress from infancy to maturity. And lastly, comparing together these three kinds of emotional development, displayed in the ascending grades of the animal kingdom, in the advance of the civilized races, and in individual growth, we may see in what respects they harmonize, and what are the implied general truths.

Having gathered together and generalized these several classes of facts, we should have better means of studying the emotions analytically. Setting out with the unquestionable assumption that every new form of emotion making its appearance in the individual or the race, is a modification of some pre-existing emotion, or a compounding of several pre-existing emotions, we should be greatly aided by knowing what always are the pre-existing emotions. When, for example, we find that the love of accumulation is absent in the lower animals and in infancy—when we see that an infant may exhibit anger, fear, wonder, while yet it manifests no desire of permanent possession, and that without any conception of property a brute may show attachment, jealousy, love of approbation; we may suspect that the love of property is compounded out of simpler and deeper feelings. We may conclude that as, when a dog hides a bone, there must exist in him a prospective gratification of hunger; so there must similarly at first, in all cases where anything is secured or taken possession of, exist an ideal excitement of the feeling which that thing will gratify. We may further conclude that when the intelligence is such that a variety of objects come to be utilized for different purposes—when, as among savages, divers wants are satisfied through the articles appropriated for weapons, shelter, clothing, ornament; the act of appropriating comes to be one constantly involving agreeable associations, and one which is therefore pleasurable, irrespective of the end subserved. Add to which, that when, as in civilized life, the property acquired is of a kind not conducing to one order of gratification in particular, but is capable of administering to all gratifications, the pleasure of acquiring property grows more distinct from each of the various pleasures subserved—more completely differentiated into a separate emotion.

This illustration, roughly as it is sketched, will show what we mean by the use of comparative psychology in aid of classification. Ascertaining by induction the actual order of evolution of the emotions, we are led to suspect this to be their order of successive dependence; and are so led to recognise their order of ascending complexity, and by consequence their true groupings.

And thus, in the very process of arranging the emotions into grades, beginning with those involved in the lowest forms of conscious activity and ending with those peculiar to the adult civilized man, the way is opened to that ultimate analysis which alone can lead us to the true science of the matter. For when we find not only that there exist in a man various feelings which do not exist in a child, but also that the European is characterized by some sentiments which are wholly or in great part absent from the savage—when we see that, besides the new emotions that arise spontaneously as the individual becomes completely organized, there are new emotions making their appearance in the more advanced divisions of our race; we are led to ask—How are new emotions generated? It is notorious that some of the lowest savages, as the Australians, have not even the ideas of justice or mercy: they have neither words for them nor can they be made to conceive them; and the manifestation of them by Europeans they ascribe to fear or cunning. Further, it is a familiar fact that there are æsthetic emotions common among ourselves, that are scarcely in any degree experienced by the inferior races; as, for instance, those produced by music. To which instances may be added the less marked but more numerous contrasts that exist between the various civilized nations in the degrees of their several emotions. And if it is manifest, not only that all the emotions are capable of being permanently modified in the course of successive generations, but also that what must be classed as new emotions may be brought into existence; then it is clear that nothing like a true conception of the emotions is to be obtained until we understand how they are evolved.

Comparative psychology, while it raises this inquiry, also prepares the way for answering it. For when observing the differences between races we can scarcely at the same time fail to observe how these differences correspond with differences in their conditions of existence, and therefore in their daily experiences. To take first the minor illustrations, we see how conquest fosters the martial feeling, and how a love of the sea is generated by conditions that necessitate maritime activity. We see how nations that have long had free institutions acquire an independence of character strongly contrasted with that servility characterizing nations that have always been governed despotically. But chiefly in the differences between the emotional natures of savage and civilized, may we note the effects of changed conditions. Among the lowest races of men, the love of property stimulates to the attainment only of such things as satisfy immediate desires, or desires of the immediate future. Improvidence is the rule: there is little effort to meet remote contingencies. But the growth of established societies having gradually given security of possession, there has been an increasing tendency to provide for coming years; a constant exercise of the feeling which is satisfied by a provision for the future; and a growth of this feeling so great that it now prompts accumulation to an extent beyond what is needful. Note, again, that under the discipline of social life—under a comparative abstinence from aggressive actions, and a performance of those mutually serviceable actions implied by the division of labour—there has been a development of those gentle emotions of which inferior races exhibit but the rudiments. Savages delight rather in giving pain than pleasure—are almost devoid of sympathy. While among ourselves philanthropy organizes itself in laws, establishes numerous institutions, and dictates countless private benefactions.

From which and other like facts, does it not seem an unavoidable inference that new emotions are developed by new experiences—new habits of life? All are familiar with the truth, that in the individual each feeling may be strengthened by performing those actions which it prompts; and to say that the feeling is *strengthened*, is to say that it is in part *made* by these actions. We know further, that not unfrequently individuals, by persistence in certain special courses of conduct, acquire special liking for such courses, disagreeable as these may be to others; and these whims, hobbies, or morbid tastes, are nothing but incipient emotions corresponding to these special activities. We know that emotional characteristics, in common with all others, are more or less clearly hereditary; and that the differences between civilized nations descended from the same stock, show us the cumulative results of small modifications hereditarily transmitted. And when we see that between savage and civilized, races which diverged from each other in the remote past, and have for a hundred generations followed modes of life becoming ever more unlike, there exist still greater emotional contrasts; is it not clear that the more or less distinct emotions which characterize the civilized races, are the organized results of certain daily-repeated combinations of mental states which social life involves? Must we not say that habits not only modify emotions in the individual, and not only beget tendencies to like habits and accompanying emotions in descendants, but that when the conditions of the race make the habits persistent, this progressive modification may go on to the extent of producing emotions so far distinct as to seem new? And if so, we may suspect that such new emotions, and by implication all emotions analytically considered, consist of aggregated and consolidated groups of those simpler feelings which habitually occur together in experience: that not only do they result from combined experiences, but are constituted of them. When in the circumstances of any race some one kind of action or set of actions, sensation or set of sensations, is usually followed or accompanied by various other sets of actions or sensations, and so entails a large mass of pleasurable or painful states of consciousness; these by frequent repetition become so connected together that the initial action or sensation brings the ideas of all the rest crowding into consciousness, producing in a degree the pleasures or pains that have before being felt in reality. And when this relation is not only frequently repeated in the individual, but occurs in successive generations, all the many nervous actions involved tend to grow organically connected. They become incipiently reflex; and on the occurrence of the appropriate stimulus the whole nervous apparatus which in past generations was brought into activity by this stimulus, becomes nascently excited. Even while yet there have been no individual experiences, a vague feeling of pleasure or pain is produced, constituting what we may call the body of the emotion. And when the experiences of past generations come to be repeated in the individual, the emotion gains both strength and definiteness; and is accompanied by the appropriate specific ideas.

This view of the matter, which we believe the established truths of Physiology and Psychology unite in indicating, and which is the view that generalizes the phenomena of habit, of national characteristics, of civilization in its moral aspects, at the same time that it gives us a conception of emotion in its origin and ultimate nature, is well illustrated in the mental modifications undergone by animals. It is, for instance, a well-known fact, that on newly-discovered lands not inhabited by man, birds are so devoid of fear as to allow themselves to be knocked over with sticks; but that in the course of generations they acquire such a dread of man as to fly on his approach; and that this dread is manifested by young as well as old. Now, in the first place, it is clear that this change is the result of accumulated experiences; and that each experience has a share in producing it. Whence it follows that in each individual bird that had escaped with injuries inflicted by man, or that had been alarmed by the outcries of other members of the flock (gregarious creatures of any intelligence being necessarily more or less sympathetic), there had been established an association of ideas between the human aspect and the pains, direct and indirect, suffered from human agency. It follows further, that the state of consciousness which impels the bird to take flight, is at first nothing more than an ideal reproduction of those painful impressions which before followed man's approach; that such ideal reproduction has become more vivid and more massive as the quantity of painful experiences has increased; and that thus the emotion in its incipient state is nothing else than an aggregation of the revived pains before experienced. In the second place remark, that as in the course of generations the young birds of this race begin to display a fear of man before yet they have been injured by him, it is an unavoidable inference that the nervous system of the race has been organically modified by these experiences: we have no choice but to conclude that when a young bird is thus led to fly, it is because the impression produced on its senses by the approaching man entails through an incipiently reflex action, a partial excitement of all those nerves which in its ancestors had been excited under the like conditions; that this partial excitement has its accompanying painful consciousness; and that the vague painful consciousness thus arising constitutes emotion proper—*emotion undecomposable into specific experiences, and therefore seemingly homogeneous.*

If such be the true explanation of the fact in this case, then is it in all cases. If emotion is so generated here, then is it so generated throughout. We must perforce conclude that the emotional modifications displayed by different nations, and those higher emotions by which civilized are distinguished from savage, are to be accounted for on the same principle. And concluding this, we are led strongly to suspect that the emotions in general have severally a like origin.

Perhaps we have now made sufficiently clear what we mean by the study of the emotions through analysis and development. At the same time we venture to think we have justified the positions that, without analysis aided by development, there cannot be a true natural history of the emotions; and that a natural history of the emotions based on external characters can be but provisional. Mr. Bain, in confining himself to an account of the emotions as they exist in the adult civilized man, has neglected those classes of facts out of which the science of the matter must chiefly be built. It is true that he has treated of habits as modifying emotions in the individual; but he has not recognised the fact, that where conditions render habits persistent in successive generations, such modifications are cumulative: he has not hinted that the modifications produced by habit are emotions in the making. It is true also that he occasionally refers to the characteristics of children; but he does not systematically trace the changes through which childhood passes into manhood, as throwing any light on the order and genesis of the emotions. It is further true that he here and there instances national traits in illustration of his subject; but these stand as isolated facts, having no general significance: there is no hint of any relation between them and the national circumstances; while all those many moral contrasts between lower and higher races which throw great light on classification, are overlooked. And once more, it is true that many passages of his work, and sometimes indeed whole sections of it, are analytical; but his analyses are incidental—they do not underlie his entire scheme, but are here and there added to it. In brief, he has written a Descriptive Psychology, which does not appeal to Comparative Psychology and Analytical Psychology for its leading ideas. And in doing this, he has not only of necessity omitted much that should be included in a natural history of the mind, but to that part of the subject with which he has dealt, he has given an organization that is necessarily superficial.

Even leaving out of view the absence of those methods and criteria on which we have been insisting, it appears to us that meritorious as is Mr. Bain's book in its details, it is seriously defective in some of its leading ideas. The first paragraphs of his first chapter quite startled us by the strangeness of their definitions—a strangeness which can scarcely be ascribed to laxity of expression. The paragraphs run thus:—

"Mind is comprised under three heads—Emotion, Volition, and Intellect.

"EMOTION is the name here used to comprehend all that is understood by feelings, states of feeling, pleasures, pains, passions, sentiments, affections. Consciousness, and conscious states also for the most part denote modes of emotion, although there is such a thing as the Intellectual consciousness.

"VOLITION, on the other hand, indicates the great fact that our Pleasures and Pains, which are not the whole of our emotions, prompt to action, or stimulate the active machinery of the living framework to perform such operations as procure the first and abate the last. To withdraw from a scalding heat and cling to a gentle warmth, are exercises of volition."

To take first the last of these definitions, we cannot but feel astonished that Mr. Bain, familiar as he is with the phenomena of reflex action, should have so expressed himself as to include a great part of them along with the phenomena of volition. Not only does he seem to be ignoring the discriminations of modern science, and returning to the vague conceptions of the past, but he is comprehending under volition what even the popular speech would hardly bring under it. If you were to blame any one for snatching his foot from the scalding water into which he had inadvertently put it, he would tell you that he could not help it; and his reply would be endorsed by the general experience that the withdrawal of a limb from contact with something extremely hot is quite involuntary—that not only does it take place without volition, but that it takes place in defiance of an effort of will to maintain the contact. How, then, can that be instanced as an example of volition which occurs even when volition is strongly antagonistic? We are quite aware that it is impossible to draw any absolute line of demarcation between automatic actions and those which are not automatic. Doubtless we may pass gradually from the purely reflex through the consensual to the voluntary. Taking the case Mr. Bain cites, it is manifest that from a heat of such moderate degree that the withdrawal from it is wholly voluntary, we may advance by infinitesimal steps to a heat such as to compel involuntary withdrawal; and that there is a stage at which the voluntary and involuntary actions are mixed. But the difficulty of absolute discrimination is no reason for neglecting the broad general contrast, any more than it is for confounding light with darkness. If we are to include as examples of volition, all cases in which pleasures and pains "stimulate the active machinery of the living framework to perform such operations as procure the first and abate the last," then must we consider sneezing and coughing as examples of volition. If Mr. Bain does not mean this, then his mode of expressing himself is lax to a degree that is especially improper in a scientific work. If he does mean it, then his conception of will seems to us more vague even than that which is current.

A parallel criticism applies to his definition of Emotion. Here, too, he has departed from the ordinary acceptation of the word, and, as we think, in the wrong direction. Whatever may be the interpretation that is justified by its derivation, the word Emotion has come generally to mean that kind of feeling which is not a direct result of any action on the organism, but is either an indirect result of such action or arises quite apart from any such action. We commonly use it to indicate those sentient states which are independently generated in consciousness; as distinguished from those generated in our corporeal framework, and known as sensations. Now this distinction, tacitly made in common parlance, is one which Psychology cannot well reject; but one which it must adopt, and to which it must give scientific precision. But Mr. Bain appears to ignore any such distinction. Under the term "emotion" he includes not only passions, sentiments, affections, but all "feelings, states of feeling, pleasures, pains"—that is, all sensations. This does not appear to be a mere lapse of expression; for when in the opening sentence he asserts that "mind is comprised under the three heads—Emotion, Volition, and Intellect," he of necessity implies that sensation is included under one of these heads; and as it cannot be included under volition or intellect, it must be classed with emotion: as it clearly is in the next sentence.

We cannot but think this an entirely retrograde step. Though distinctions which have been established in popular thought and language, are not unfrequently merged in the higher generalizations of science (as, for instance, when crabs and worms are grouped together in the sub-kingdom *Annulosa*), yet science very generally recognises the validity of these distinctions, as real though not fundamental. And so in the present case. Such community as is disclosed between sensation and emotion, by an ultimate analysis, must not shut out the broad contrast that exists between them. If there needs a wider word, as there does, to signify any sentient state whatever, then we may fitly adopt for this purpose the word currently so used, namely, "Feeling." And considering as Feelings all that great division of mental states which we do not class as Cognitions, may then separate this great division into the two orders, Sensations and Emotions.

And here we may, before concluding, briefly indicate the leading outlines of a classification which reduces this distinction to a scientific form, and developes it somewhat further—a classification which, while suggested by certain fundamental traits reached without a very laborious inquiry, is yet, we believe, in harmony with that disclosed by detailed analysis.

Leaving out of view the Will, which is a simple homogeneous mental state forming the link between feeling and action, and not admitting of subdivisions, our states of consciousness fall into the two great classes of COGNITIONS and FEELINGS.

COGNITIONS, which are those modes of mind in which we are occupied with the *relations* that subsist among our feelings, are divisible into three great sub-classes.

Presentative cognitions; or those in which consciousness is occupied in localizing a sensation impressed on the organism—occupied, that is, with the relation between this presented mental state and those other presented mental states which make up our consciousness of the part affected: as when we cut ourselves.

Presentative-representative cognitions; or those in which consciousness is occupied with the relation between a sensation or group of sensations and the representations of those various other sensations that accompany it in experience. This is what we commonly call perception—an act in which, along with certain impressions presented to consciousness, there arise in consciousness the ideas of certain other impressions that are ordinarily connected with it: as when from its visible form and colour we mentally endow an orange with all its other attributes.

Representative cognitions; or those in which consciousness is occupied with the relations among ideas or represented sensations: as in all acts of reflection.

FEELINGS, which are those modes of mind in which we are occupied, not with the relations subsisting between our sentient states, but with the sentient states themselves, are divisible into three parallel sub-classes.

Presentative feelings, ordinarily called sensations, are those mental states in which, instead of regarding a corporeal impression as of this or that kind, or as located here or there, we contemplate it as pleasure or pain: as when eating.

Presentative-representative feelings, embracing a great part of what we commonly call emotions, are those in which a sensation or group of sensations arouses a vast aggregation of represented sensations, partly of individual experience, but chiefly deeper than individual experience, and, consequently, indefinite. The emotion of terror may serve as an example. Along with certain impressions made on the eyes or ears or both, are recalled into consciousness a crowd of the pains to which such impressions have before been the antecedents; and when the relation between such impressions and such pains has been habitual in the race, the definite ideas of such pains which individual experience has given are accompanied by the indefinite pains that result from inherited experience—vague feelings which we may call organic representations. In the infant, crying at a strange sight or sound while yet in the nurse's arms, we see these organic representations called into existence in the shape of dim pains to which individual experience has yet given no specific outlines.

Representative feelings, comprehending not only the ideas of the feelings above classed when they are called up apart from external objects, but more especially comprehending that higher class of feelings which we ordinarily distinguish as sentiments. In these—as, for example, in the sentiment of justice—sensations have no share: there is no presentative element. This feeling is compounded out of mental states that are themselves wholly, or almost wholly, representative; it involves representations of those lower emotions which are produced by the possession of property, by freedom of action, &c.; and thus is in great part even re-representative.

This classification, here but roughly indicated and capable of much further expansion, will be found in harmony with the results of detailed analysis aided by development. Whether we trace mental progression through the grades of the animal kingdom, through the grades of mankind, or through the stages of individual growth, it is obvious that the advance, alike in cognitions and feelings, is, and must be, from the presentative to the more and more remotely representative. It is undeniable that intelligence ascends from those simple perceptions in which consciousness is occupied in localizing and classifying sensations, to perceptions more and more compound, to simple reasoning, to reasoning more and more complex and abstract—more and more remote from sensation. And in the evolution of feelings there is a parallel series of steps. Simple sensations; sensations combined together; sensations combined with represented sensations; represented sensations organized into groups in which their separate characters are very much merged; representations of these representative groups, in which the original components have become still more vague. In both cases, the progress has necessarily been from the simple and concrete to the complex and abstract; and as with the cognitions, so with the feelings, this must be the basis of classification.

The space which we have here occupied with criticisms on Mr. Bain's work, we might have filled with exposition and eulogy, had we thought this the more important. Though we have freely pointed out what we conceive to be its defects, let it not be inferred that we question its great merits. We repeat that, as a natural history of the mind, we believe it to be the best yet produced. It is a most valuable collection of carefully elaborated materials. Perhaps we cannot better express our sense of its worth than by saying that, to those who hereafter give to this branch of Psychology a thoroughly scientific organization, Mr. Bain's book will be indispensable.

REVIEW VI.

Professor Schroeder van der Kolk on the Minute Structure and Functions of the Spinal Cord and Medulla Oblongata, and on the Proximate Cause and Rational Treatment of Epilepsy. Translated from the original (with emendations and copious additions from manuscript notes of the author), by WILLIAM DANIEL MOORE, A.B., M.B., T.C.D., Honorary Member of the Swedish Society of Physicians, and of the Norwegian Medical Society. (New Sydenham Society.) *London*, 1859. pp. 291.

ON a former occasion* we gave a summary of the researches of the distinguished Dutch Professor into the structure and functions of the spinal cord, and to the conclusions to which he had been led up to the year 1854. A portion only of the present volume is occupied by the work to which we then drew attention; above three-fourths of it are devoted to two new essays—the one containing an inquiry into the structure and functions of the medulla oblongata; the other having for its object the elucidation of the seat and nature of epilepsy. It will be unnecessary for us to go over the ground trodden in our former article, but before introducing Professor Schroeder van der Kolk's recent researches to our readers, we may remind them that among the views formerly enunciated was the following:—The medulla oblongata appears to be the central point where reflex influences cross to either side, and upon the irritation of which general spasms, as convulsions and epilepsy, seem to depend. This passage may be regarded as the text of the second and third discourse, which, like the first, have been very ably and agreeably rendered into English by Dr. William Moore, to whose talent for languages the literature of this country has already so often been indebted for the interpretation of foreign works.

It appears that Schroeder van der Kolk was chiefly stimulated by Stilling's great works on the Medulla Oblongata (Erlangen, 1843) and the Pons Varolii (Jena, 1846) to enter upon a minute investigation of the former, in the first instance, with almost exclusive reference to pathology.

"However," the author informs us, "the course of the investigation led me from one part to another; the solution of one question gave the key to another; what the structure of man did not reveal, I found demonstrated distinctly and clearly, and beyond my expectations, in animals; and thus under my hands the essay gradually enlarged, and in it I think I have arrived at some, as I hope, not unimportant results, and have as far as possible solved most of the anatomical and physiological questions upon which I have, in the third part, wholly rested my pathological views."

We now proceed as briefly as we can, consistently with clearness, to lay before our readers the quintessence of the author's teaching.

He first dwells upon the points of difference which distinguish the structure of the medulla oblongata from the spinal cord. The central canal of the latter inclines backwards in the medulla oblongata, so as

* British and Foreign Medico-Chirurgical Review, vol. xix. p. 85.

to open into the fourth ventricle. The anterior grey horns accompany the canal, and thus come to form the floor of the fourth ventricle, from which the hypoglossus arises; at the same time the posterior cornua are spread out laterally, also contributing to form the floor of the fourth ventricle. There is, therefore, a complete displacement of the posterior columns, and we likewise meet with new parts, as is evident even on a superficial comparison of the size of the cord and of the medulla oblongata. The latter is considerably larger than the former, and this increased thickness is due to the superaddition of parts not present in the cord. These are the corpora restiformia, the posterior pyramids which descend from the cerebellum, the root of the trigeminus and the corpora olivaria. The medulla oblongata is also much increased in size by the nuclei and ganglionic groups, whence Schroeder van der Kolk shows the nerves of sensation to arise. Moreover, it contains transverse arched fibres, which unite the two sides of the medulla oblongata, and certain longitudinal fibres descending from above and terminating in the medulla oblongata. Hence, according to our author, "we may regard the medulla oblongata as the nodus vitæ, or as a central point, where many different bundles of fibres end or take their origin in various ganglionic groups, which hence diffuse their influence over so many different parts of the body."

The question of the decussation and origin of the various nerves of the medulla oblongata receives much attention. The author has satisfied himself with regard to the hypoglossus that it does not decussate, but that it is wholly lost in the hypoglossal nucleus; the same is the case with the other sensitive nerves, and the impression produced on them is conveyed to the opposite side by marginal fibres, so that in fact an indirect decussation is effected; and the result is the same in this respect as if the nerves themselves interchanged their fibres. Of all the nerves of the medulla oblongata, none has so intimate a connexion with its fellow as the facial, which might almost have been assumed *à priori* from the nearly uniformly bilateral functions of this nerve. It decussates in part directly; a part of its fibres arise from its nucleus. The auditory nerve is naturally the next nerve that is passed under review. Its nucleus or ganglionic cells are very large, only requiring a magnifying power of 8 or 10 diameters to render them plainly visible. Some of the fibres arising from them pass into the cerebellum with a number of fibres derived from the cerebellum and corpus restiforme; fibres also pass from the nucleus of the auditory to that of the facial nerve—a connexion which satisfactorily explains the undoubted reflex influence which is transmitted from the auditory to the facial nerves. This reflex is particularly marked in animals, in whom, with voluntary movement of the ear, there is also manifestly combined an instinctive movement which can only be explained by reference to such a relation as that just spoken of.

"In man, as is well known, there is scarcely any movement of the ears, but the reflex action of terror on the facial nerve," whether induced by an alarming sound, or by other means, "whereby even the frontal and occipital muscles can be rendered extremely tense, and the hairs, as we say, stand on end, has long since, as well as the retention of the voice from rapid inspiration, been graphically described by Virgil, in the well-known line

'Steteruntque comæ, vox faucibus hæsit.' "

The abducent does not, like the other nerves of the medulla oblongata, decussate indirectly through the fibres which arise from their nuclei, but its fibres, instead of passing towards the raphe to meet its fellow, curve outwards away from the raphe; the nucleus in which the nerve terminates being removed further from the latter, and no fibres being seen to pass from the nucleus to the raphe. Should this point receive confirmation, Schroeder van der Kolk considers it to offer a satisfactory explanation of the antagonism between the muscles of the eye, because if the right abducent nerve is connected with the fibres from the brain, which stimulate the left nucleus of the oculo-motor nerve after decussation the, same stimulus would simultaneously bring into action the left internal and right abducent muscle, and *vice versâ.*

The trigeminus establishes connexions with nuclei of all the nerves of the medulla oblongata, a fact which is quite in harmony with the great range of reflex action by which this nerve is distinguished. The various acts of swallowing, breathing, coughing, are so many instances of the reflected action produced through the trigeminus in the glosso-pharyngeal, vagus, accessory, and hypoglossal nerves. The author successively traces the mode of connexion established between the fifth nerve and the nerves just mentioned, in the medulla oblongata, and states that the trigeminus is also closely connected with the accessory ganglia and corpora olivaria. The author also points out that, as a general rule, in every place where fibres are given off performing any special function, such as reflex action, fresh groups of ganglionic cells invariably appear, giving origin to these fibres.

From the uniformity of this occurrence in the origin of the trigeminus, Schroeder van der Kolk infers that, as a general rule, wherever a particular action is to be excited through nervous filaments, special ganglionic cells are required, producing the peculiar nervous action. "In like manner," he adds—though without proffering any special evidence in behalf of this assertion—"in like manner the orders of our will do not pass directly into the motor nerves, but into ganglionic cells, whence the peripheric action arises for the movement of the muscles."

In a chapter entitled "On the Accessory Ganglia, in the Medulla Oblongata," the structure and functions of the olivary bodies are examined and illustrated, and several pathological facts are brought forward in support of the author's views, which lead mainly to this conclusion:—That the olivary bodies are auxiliary ganglia, which by their mutual connexion produce a bilateral action, and by their connexion with the hypoglossi develope the combinations required for articulation. The greater size of these bodies in man than in animals is explicable on this view; while fatty degeneration, induration, and other morbid conditions have been traced in them in patients whose maladies had been characterized by more or less impairment of speech.

A similar view was already entertained by Pinel, who noticed that alterations were as constant in the corpora olivaria in general paralysis, as the alterations in speech which accompany that disease, and therefore inferred that those bodies were intimately allied to the function of articulation.

It is not intended to assert that every impairment of speech is necessarily associated with a lesion of these organs; thus it may depend upon a loss of memory from cerebral disease, the mechanism of the tongue remaining untouched; again, injury of the corpora striata may prevent the will from acting upon the corpora olivaria, and thus induce palsy of the tongue, without a lesion of the latter; or lastly, disease of the pons Varolii may destroy the filaments that connect the corpora olivaria with the higher parts, and thus cause loss of the voice or of speech. In each of these cases the corpora olivaria may exhibit no morbid appearances.

We gather from the next chapter, which is devoted to the comparative anatomy of the corpora olivaria, that they are smaller in animals; and that with the exception of apes, in whom they resemble those of man, though still of smaller size, they present a division on each side; the superior one, situated more outwardly, being on a level, and closely connected with the nucleus of the facial nerve, while the inferior one which approaches closer to the raphe is united with the nucleus of the hypoglossus. Schroeder van der Kolk infers that the superior corpora olivaria are the organs for the reflex expression of passions, because they are largest in carnivora and smallest in herbivora; rodentia occupying in this respect a middle position. He has found the inferior corpora olivaria, which are strictly limited within the roots of the hypoglossus, to be the same in all the animals he has examined, and he concludes that they serve as auxiliary ganglia in deglutition; in man the two are united; hence it is probable that here

"different parts of the corpora olivaria exercise different functions, according to the nerves with whose nuclei they are connected by radiating fibres; for these several functions—as expression, articulation of the voice, and swallowing —may be lost in disease."

Besides the corpora olivaria, we find, both in man and animals, another group of larger ganglionic cells on a level with the facial, which appears to be capable of acting unilaterally, and being closely connected both with the facial and trigeminus, to serve for the reflex action of the latter in winking of the eyelids.

The author dwells somewhat at length on the relations of the origin of the vagus (p. 184). It arises from a nucleus lying external to the nucleus of the hypoglossus; to the outside of the vagus he finds a bundle of longitudinal fibres, near the entrance of the nerve into its nucleus; nervous filaments are observed to pass from this bundle into the vagus and accessory nerves, and to proceed with the trunk of the former outwards; others join the root of the trigeminus, and thus account for the influence which this nerve exerts over respiration.

"Hence," to use Schroeder van der Kolk's own words, "it is easy to explain why, if the nervus vagus be centripetally excited, for example, by means of a rotatory apparatus, respiration ceases, as the stimulus is now conveyed along the nervus vagus to the lateral columns of the medulla, which curve through cross rays again into the anterior grey horns of the spinal cord, to pass into the nuclei, whence the respiratory nerves arise."

The same mechanism also serves to produce the phenomena of ordinary respiration. The vagi convey a stimulus from the lungs to the lateral columns, from where it passes by the phrenics to the diaphragm, causing an inspiratory effort.

Besides the bundle of fibres to which the reflex phenomena of the respiratory act are attributable, there are fibres which serve to convey the orders of the will to the organs of respiration, which as is well known are not removed from the control of volition. The author has distinctly traced a set of marginal fibres round the nucleus of the hypoglossus, the chief nerve of deglutition, which terminate in the nerve nuclei, and decussating with the other side, appear to curve upwards into longitudinal fibres; these he regards as the channels for the operation of the will upon the respiratory apparatus. Of these conditions of our will to the organs of respiration, he states that they decussate, and that they afford fresh proof that the lateral columns of the spinal cord really end here; the corpora pyramidalia, which serve for the movement of the extremities, alone passing, as a continuation of the anterior columns, into the brain. In reference to this point, pathology appears to offer an interesting illustration, in a circumstance that we do not remember to have seen explained elsewhere:

"In hemiplegia after cerebral apoplexy, the arm and leg of the same side are paralysed. We call this, however, very incorrectly, unilateral paralysis, as if the entire half of the body were paralysed; this is not the case; only the muscles of the one half of the face, of the arm and leg, are affected; but the intercostal muscles, the abdominal muscles, and the one half of the diaphragm, are, in hemiplegia, undisturbed in their functions. This peculiarity, to which, in my opinion, sufficient attention has not been paid, is to me a powerful proof that the lateral columns of the spinal cord do not run into the brain, as they should then in apoplexy likewise participate in the subsequent paralysis, which never is the case. As they are brought into action specially by the stimulus received from the nervus vagus, their function does not depend directly on our will, although it may be to a certain extent influenced by the latter." (p. 187.)

A chapter is devoted to the consideration of the nervous channels which co-operate in the act of deglutition, and the centres which preside over it. The author's own recapitulation will better serve to explain his meaning, than if we attempt to extract the quintessence of this chapter by a process of our own:

"Swallowing, when voluntarily excited, is a reflex action, which is always accomplished with rapidity, but is very complicated. The exciting stimulus appears to proceed from the second branch of the trigeminus, namely, the palatine nerve; for division of the lingual or glosso-pharyngeal nerve does not prevent swallowing. But the irritation seems to be conveyed to the corpora olivaria inferiora in animals, and to the corresponding parts in man, whence it is reflected on the hypoglossal and accessory nerves, with which these corpora olivaria are closely connected, and through which the act of deglutition is occasioned by the simultaneous excitation of many muscles into bilateral action. Perhaps also the small root of the trigeminus, by closing the mouth, participates in the act."

Most of the readers of this work will probably anticipate greater satisfaction from the portion to which it now becomes our duty to advert, than from the somewhat intricate inquiries which constitute the first two-thirds of the volume. The author's investigations into the relation of epilepsy to morbid conditions of the medulla are not, however, to be disconnected from the more purely anatomical and physiological researches which we have sought succinctly, and we trust intelligibly, to place before our readers. While, however, we cannot deny that considerable interest attaches to this part of the author's inquiries, and believe them to advance us a step in the right direction, we cannot in justice to other workers in the same field allow that similar views have not been already suggested; nor does there appear to be sufficient ground for assuming that Schroeder van der Kolk has fairly enabled us to overcome epilepsy. With regard to the former point we need only quote a passage from a single author of our own country to show that the medulla oblongata is not now for the first time held to be a centre of convulsive action. We find Dr. Todd,* some time before the year 1847, expressing himself thus:

"Irritation of any part of the medulla oblongata excites convulsive movements in muscular parts which receive nerves from it, and through the spinal cord, in the muscles of the trunk. Spasm of the glottis, difficulty of deglutition, irregular acts of breathing, result from irritation of the medulla oblongata; and if the excitement be propagated to the cord, convulsions will become more or less general."

In 1854, as the author reminds us, and as we stated in the article previously quoted,† he maintained that the medulla oblongata is the principal centre where the reflex influence is transferred to either side, and upon the irritated condition of which depend all widely-diffused spasms, as convulsions, epilepsy, &c. With a view to establishing the pathology of epilepsy upon a more definite basis, Schroeder van der Kolk has examined microscopically the medulla oblongata of fourteen epileptic patients. He observes that the medulla oblongata is distinguished from the brain and spinal cord by its bilateral action, owing to the great number of commissural fibres and the many ganglionic groups and auxiliary ganglia which it contains. Unilateral lesions in the brain or spinal cord are manifested by unilateral effects, whereas lesions of the medulla oblongata are ordinarily characterized by bilateral reflex phenomena. Where, however, as is frequently seen in epilepsy, the convulsive action predominates on one side or the other, there the Professor admits a *primary* affection of the brain; an admission which may prove an absence of unfair bias on the part of the author, but very much diminishes the definiteness of the results to

* The Cyclopædia of Anatomy and Physiology. Edited by R. B. Todd, M.D. Vol. iii. 1839—1847, p. 722.

† Medico-Chirurgical Review, vol. xix. p. 97.

which at first sight Schroeder van der Kolk's researches might appear to have conducted us in regard to the causation of epilepsy. In the convulsions of children and in the epilepsy of older subjects, the face and respiratory organs are the mirror from which the disease is first reflected. It is in the range of the facial, accessory, hypoglossal, and the portio minor of the fifth pair, that the first symptoms of epilepsy show themselves, more or less violent disturbances of respiration attending them. When the attacks become more severe, the spasms extend to the chest and abdomen, and are attributable to the lateral columns of the spinal cord which arise in the medulla oblongata and the vagi being involved; while the spasms of the extremities which also ensue are to be set down to irritation of the anterior columns, "which, as corpora pyramidalia, pass through the medulla oblongata." An exalted sensibility of the medulla oblongata, in short, is, according to our author, the starting point and source of the epileptic paroxysm.

The occurrence of an epileptic paroxysm has long since been compared to the discharge of electricity from an overcharged battery. Dr. Schroeder van der Kolk now informs us that this view is literally the correct one, inasmuch as the groups of ganglionic cells act upon the same principle as batteries. But there is this difficulty, that the ganglionic cells are connected on the one side with the nerve filaments which are derived from the brain, on the other with nerve filaments accompanying the sensitive nerves, which the author describes as reflex nerves. Some time elapses before the stimulation of the reflex filaments is followed by its appropriate effects, whereas the effect of the will upon the muscles, which is conveyed by the ganglionic corpuscles, is instantaneous. The author suggests that the difference may be due to a peculiar nature and action of the reflex cells. This activity, however, is manifestly supported and restored by the arterial blood, which is abundantly supplied to the grey matter both of the brain and the spinal cord. This supply is greater in the corpus ciliare of the olivary bodies than anywhere else, and the author has noted generally that the groups of ganglionic cells, with the accessory ganglia, are uncommonly rich in bloodvessels. He considers his theory borne out by the circumstance that in childhood, "where the metamorphosis of tissue takes place more quickly, whilst the vascularity is also greatest," the sensibility of the whole nervous system is so much more intense, and the slightest irritation frequently induces convulsions. We would only observe upon this hypothesis, that the greater arterialization of the children in whom convulsions occur is very mythical, seeing that in almost all, if not in all, the cases that come before us, we find a combination of circumstances that tend to produce a venous condition, rather than the reverse, and that our attempts at a permanent cure will fail so long as we do not secure a full aeration or oxygenation of the blood. However, it is rather our wish to represent our author's views than to enter into a controversy upon them; the more so, as the facts which he lays before us deserve every attention, and have evidently been collected with great care.

Referring to Brown-Séquard's experimental production of epilepsy, Schroeder van der Kolk states that it is corroborative of his doctrine, inasmuch as the paroxysm produced after unilateral section of the spinal cord by irritation of the opposite cheek could only be excited after the lapse of three weeks from the receipt of the injury, the morbid sensibility induced by the division requiring that period to travel up to the medulla oblongata; moreover, the surest means of exciting the fits was by inducing reflex irritation through the agency of the second and third branches of the trigeminus. To produce these effects, no disorganization of the medulla is necessary; an increased excitability, combined with congestion "and chemical change" (of what kind is not stated), sufficing for the purpose. Kussmaul and Tenner's explanation of the proximate cause of epilepsy, which they seek in an anæmic condition of the mesocephale and adjoining parts of the brain, is objected to by our author; but his arguments appear to us wanting in stringency, and not sufficient to overturn the fact that the means adopted to induce anæmia certainly are followed by convulsions, as we even find Schroeder van der Kolk admitting at an earlier part of the volume. The unconsciousness of epilepsy he regards, not as a cause of epilepsy, but as the result of the impeded respiration and the influence of the excited ganglionic corpuscles upon the vaso-motor nerves of the brain. He therefore admits the trachelismus of Marshall Hall as an element in the production of unconsciousness.

The pathological anatomy of epilepsy must always be approached with a clear conception of the necessity of distinguishing between morbid conditions that may have resulted from the paroxysms and those that may be essential to their production. Among the former, we may reckon general congestion of the brain and "increased gravity owing to the more albuminous condition of the intercellular fluid so frequently present in epileptic patients." Among the latter, Schroeder van der Kolk notices, first, hardening or softening of the medulla oblongata; and next, as of especial importance, the changes in the bloodvessels of the medulla oblongata.

In all autopsies of epileptics, whether the patients died in or out of the fit, our author found great redness and vascular tension in the fourth ventricle, penetrating into the medulla oblongata to a greater or less depth; in most cases, the hyperæmia extended to the corpora olivaria. Having been induced in one case to measure the width of the vessels in the medulla oblongata, he found that the capillaries running in the course of the hypoglossus were considerably larger than those in the course of the vagus, and he regarded this as an explanation of the fact that this patient had invariably bitten his tongue in the fit.

"For as this phenomenon, although very frequent in epilepsy, does not occur in all cases, it appeared to me, that if during the fit the vessels, which are so much wider, conducted more blood to the nucleus of the hypoglossus and the corpus olivare, which is in such close connexion with it, it was not improbable that these parts would be still more irritated, which might cause convulsive movements of the tongue, and the protrusion of this organ between the teeth, and thus occasion it to be constantly bitten during the attack of epilepsy."

Schroeder van der Kolk followed up this clue, and the general result of his inquiries leads to this—that in those patients who bit their tongue, the capillaries of the hypoglossus, the corpus olivare, and the raphe were considerably enlarged as compared with those of the vagus; while the reverse was the case in those who did not bite their tongue. We have not space to enter into this matter more in detail; those who are interested in the question have doubtless already read enough to induce them to supply the deficiencies of our very meagre outline by reference to the volume.

It only remains for us to say a word upon what the author calls the rational treatment of the disease. What has preceded will at once suggest that derivatives are the main agents upon which he relies; digitalis and other sedatives are lauded, but setons, issues, and the actual cautery applied in the vicinity of the medulla oblongata, are his sheet-anchors. He admits that nitrate of silver, zinc, and other remedies may have occasionally cured epilepsy; but when they have done so, he attributes the effect to the sedative effect they have exercised upon the intestines or upon the organs whose exalted sensibility proved an irritant of the medulla oblongata, probably in the same way as the semi-divided spinal cord of Brown-Séquard's epileptic rabbits.

We, too, have used setons and issues, as many have done before us, sometimes with advantage, more frequently without benefit to our patients. That there are many cases in which they constitute a part of a rational treatment, we believe; but we must demur to this being made our chief remedy, until empiricism or science demonstrates them to be so. With all deference, we would say that the distinguished Utrecht Professor fails to establish a uniformity, either in the morbid process or in the treatment; though we can scarcely refuse to admit that he goes far to demonstrate the site of the morbid condition which induces the epileptic paroxysm, or, to use an antiquated term, its proximate cause.

REVIEW VII.

On the Diseases and Injuries of the Joints—Clinical and Pathological Observations. By THOMAS BRYANT, F.R.C.S., Assistant-Surgeon, Surgical Registrar, and Lecturer on Operative Surgery at Guy's Hospital.—*London*, 1859. pp. 273.

IT is almost superfluous to say of a work by one of the surgeons of Guy's Hospital, that it is based upon honest experience and sound pathology. The profession owes so much to the contributions of this school that such praises have become commonplace. A great reputation, however, is, in one sense, a dangerous antagonist, and there is no wonder if people are apt to ask of a book emanating from so celebrated a school, whether it quite sustains the well-won reputation of its hospital. We are not disposed to say that Mr. Bryant's book does not: true, we expected something more; we had hoped for a complete treatise instead of a mere series of Clinical and Pathological Observations; but Mr. Bryant is the best judge as to whether his present experience justifies him in attempting the more ambitious task, and if it does not, we must be content with a less systematic work. If in the following observations we appear to dwell more on the defects than the merits of this book, it is only with a hope that we may stimulate Mr. Bryant to continue labours which have already borne good fruit, and to apply them to the production of something which shall last beyond the ephemeral collections of "cases," and "observations"—things that pass out of notice almost as soon as published—and which shall clearly indicate the progress of our science on a matter on which no good English book has been written during the many years which have elapsed since Sir B. Brodie first put this subject on a scientific foundation.

The author disclaims for his work the title of a treatise, and certainly could not have made good any such assumption; yet it is more than a mere collection of cases illustrated by remarks, although notes of cases form far too large a proportion of the matter. A book which, in 273 small octavo pages, contains notes, more or less full, of eighty-eight cases, cannot be pleasant reading. It is notorious that almost every reader skips over the paragraph headed CASE, in order to arrive at the deduction, which he can remember, freed from the tedious details, which he is sure to forget. Our first suggestion, therefore, to Mr. Bryant would be to discard the formal notes of at least two-thirds of his cases, and trust to his text for embodying the results of his experience. No one will be in any danger of suspecting that he does not write from a long and studious observation of nature, and most of these cases prove nothing more. In the next place, it will not, we hope, be considered presumptuous in us to suggest that style should be a little more attended to in the present day. Slipshod English and inaccurate grammar were little thought of in the time of Astley Cooper, but we have reformed all this, and it is now expected of a surgeon that he should write like a member of a learned profession. Such phrases as "chronic disease in a passive or *curative* condition," "forcible extension was employed with *only* a good result," "neither generally recommending *or* condemning," "*mobility* of the articulation may be painful," might be quoted out of almost every page of Mr. Bryant's work; while the oddest printer's errors have been allowed to stand, such as writing the name of the celebrated Russian surgeon "Perigott" (p. 146), "more than he could bare" (p. 94), and the like. It is true that the primary aim of a surgical book is to teach practical surgery, but a little care would be well bestowed in putting the instruction in such a form as a man of liberal education would read with pleasure.

Leaving these smaller matters, let us see what Mr. Bryant has to say upon a part at least of his subject.

Synovitis and the chronic degeneration of the synovial membrane are first treated of, but without any attempt at originality, at least in the part which treats of inflammation. In considering the termination of synovitis, Mr. Bryant says: "When the joint has suppurated there can be no doubt about the propriety of freely giving exit to its purulent contents by a liberal incision." (p. 27.) This is precisely one of the points which, in a collection of 'Clinical Observations,' should have been most fully illustrated by cases and statistics. If there is "no doubt" in the minds of surgeons about opening large joints in a state of suppuration, we can testify from personal observation that there is often much hesitation; and a good series of cases in which knee-joints had been saved from amputation or resection by timely incisions, would have done much to encourage the reader in the adoption of a course which is certainly recommended by reason, and we believe sanctioned by great success. Further, it is a point of much interest to determine the general consequences of such incisions. In children they are undoubtedly often followed by complete cure, and in adults it is not at all clear that incurable anchylosis usually follows. The subject is dismissed by Mr. Bryant in a single sentence, and without any allusion to the important difference in prognosis occasioned by the age of the patient.

On the question of the existence of synovial membrane over the articular cartilages in the natural condition, Mr. Bryant's opinion is in the affirmative:

"In a case," he says, "where the ankle had evidently but recently become inflamed, and presented a synovial membrane which was most exquisitely injected, films of recent but firm fibrinous material were poured out over the surface of the cartilage, and beneath this were fine radiating capillary vessels proceeding from the margin; in one spot I carefully raised the deposit of lymph, leaving the injection as clear as ever; it became evident that these capillaries were not, therefore, on the new-formed membrane, but existed either in the cartilage or upon a membrane covering it. This latter was undoubtedly the case; and the fact goes, I think, positively to prove the existence of a layer of synovial membrane over the articular cartilage. But this was not all; anxious to make a microscopical examination of the part, I made a thin section through the cartilage and its vascular covering; the swollen synovial membrane became distinctly visible, covering the cartilage which had undergone the granular form of degeneration, and with care the membrane was separated from its cartilage by means of needles." (pp. 20, 21.)

The observations of Mr. Bryant on the subject of chronic degeneration of the synovial membrane lead him to conclude, with most pathologists of the present day, that the disease is a mere result of chronic inflammation, and that there is nothing peculiar about it. He describes, however, two forms of the disease—the "gelatiniform," and the "pulpy," of which the inflammatory origin of the former he thinks more clearly proved than of the latter; but we cannot say that he has established any rational ground for the division which he makes, and to us they seem merely two different appearances of the same thing.

Mr. Bryant's views on the pathology of the articular cartilages are probably known to most of our readers, by a paper read before the Medico-Chirurgical Society, and of which an abstract will be found in their 'Proceedings,' vol. i. p. 70. They may be thus summed up in Mr. Bryant's own words:

"The diseases to which articular cartilages are liable may be thus classed. Like other tissues, they may undergo *hypertrophy* or *atrophy*, using the latter in its simplest sense. Inflame and ulcerate they cannot, as the presence of vessels in the tissue is generally considered necessary for such processes. But to *granular, fatty*, and *fibrous* degeneration they are peculiarly liable; and in these forms of degeneration may be included the processes which have been so variously described by different authors." (p. 42.)

We will not quote more largely from Mr. Bryant's work on this head, as no doubt his views are familiar to most of our readers. The principal fact on which Mr. Bryant dwells is, that as cartilages derive their nutrition from other structures (either the bones or synovial membranes, as he believes), so their diseases are secondary to the affections of these structures. But this order of events is, according to his own showing, not universal.

"There is a form of disease which approaches the nearest to primary disease of the cartilages, in which this structure rapidly disappears, followed by, or connected with, suppuration of the joint; it is seen in patients of middle age who have had an attack of rheumatism, which at last settled in one joint. . . . The disease is at first confined entirely to the cartilage, involving the synovial membranes in a secondary manner. The cartilage may disappear, and in healthy subjects ankylosis may rapidly take place," &c. (p. 63.)

In these cases, then, we have, according to Mr. Bryant's statement, an acute disease attended with all the symptoms of inflammation, commencing in the cartilage, and producing its absorption. Whether the old term "ulceration" be retained for this, or the new one proposed by Mr. Bryant, "granular degeneration," be substituted, seems to us more a verbal than a practical question. For ourselves, as the old term at any rate keeps alive in the minds of those who use it the essential idea of the disease—viz., that it is one of the sequelæ of acute inflammation, while the new name rather keeps this connexion out of sight, we should prefer the old term, bearing in mind, of course, that inflammation and ulceration of non-vascular tissues differ in certain easily-described particulars from the usual instances of those processes.

This subject has been most ably treated by Mr. Barwell, in the last number of this Review, and to that paper we would refer our readers for a proof that the terms "inflammation" and "ulceration" may be as rationally used of cartilages as of any other structures. We would refer to the section on "Treatment" appended to Mr. Bryant's account of the diseased condition of the cartilages, as a specimen of the ill effects of the subordinate importance which he has attached to these structures. It has seldom been our lot to see anything in a practical surgical work more meagre and unsatisfactory. In fact, all the chapters on treatment are very slight. This perhaps is intentional in a work which professes to be merely a collection of "observations," but it is not the less to be regretted. We believe a student might read the work from beginning to end, without being aware that any one had ever made an issue for the cure of a diseased joint; and in the only passage, as far as we can discover, where mercury is distinctly spoken of, its use to salivation is unhesitatingly condemned, and perhaps rightly enough in the particular affection which Mr. Bryant is there speaking of—viz., chronic affection of the articular extremities; but surely some opportunity should have been afforded to the reader of learning his author's opinions on two such very important points as the use of issues and salivation in treating the disease which, if Mr. Bryant will not permit us to call it acute inflammation of the cartilages, we hardly know what he would have us denominate it.

On the subject of diseases of the articular ends of the bone, Mr. Bryant is, we think, more happy, and puts in a strong light the important fact that "the majority of the cases which are described by surgeons as strumous or scrofulous disease of a joint depends upon a chronic inflammation in the bone." (p. 72.)

The whole of this section on inflammation of the articular extremities of bone is well worthy of perusal, and its defects only lead us the more to regret that Mr. Bryant did not delay the publication of his book until he had worked it up into a complete treatise on the whole subject. No one who reads this chapter attentively can doubt that Mr. Bryant's experience has been as ample as his industry has been untiring. But, on the other hand, no one can say that the diagnosis is clearly laid down, or the treatment fully and logically prescribed.

On the subject of amputation or excision, Mr. Bryant has a very interesting chapter. He speaks strongly, perhaps too strongly, against excision of the hip; and is hardly more favourable to excision of the knee. On the subject of the comparative mortality of excision of the knee and amputation he makes the following rather startling observations:—

"In a paper which I have read before the Medical and Chirurgical Society, I demonstrated that the deaths from amputation of the thigh, including these two last specified classes of cases—[i.e., if we understand Mr. Bryant's meaning, amputations for diseases, or 'pathological,' as he terms them], was [*sic*] about one in seven; and this is the least favourable view of the operation. Mr. Butcher and Mr. Price, avowed advocates for excision, give the fatality [a term which Mr. Bryant always uses instead of 'mortality'] of excision of the knee as one in five, and this shows the most favourable aspect. In the hands of some experienced surgeons the fatality has been more than half. Unless, therefore, the advantages to be gained by preserving the leg can be proved to be so great as to counterbalance this great difference in the risk of the operation of amputation, as performed in a large London hospital, I think it will be difficult to prove that the operation of excision should, as a rule, be preferred." (p. 143.)

We think this argument is pressed rather too far, but the fact is a striking one, and the data on which it is asserted are deserving of very attentive sifting. If the cases on which the returns were made are fairly comparable with each other, one great argument for their favourite operation will be taken out of the mouths of the partisans of resection, and it will be restricted to exceptional cases. Mr. Bryant is not, however, by any means an opponent of conservative surgery, and speaks in terms of just praise of resection of the elbow and shoulder.

A few pages on loose bodies in the joints, and a valuable chapter on inflammation external to the joints, and "bursitis" (a barbarous word, which we trust will not pass into common use), conclude the portion of the work which treats of diseases of the joints, the whole being summed up in a statistical table (p. 174), which shows indeed the large materials from which Mr. Bryant has worked, since it includes 974 cases, but is useless for drawing any practical deduction as to the success of treatment, the majority of the cases (538) being returned as "relieved," a vague way of saying that the case was left incomplete, A large part of the book consists of a collection of cases of injuries of the joints. It is very practical and good; but we fear that our space will not permit of our noticing it more particularly.

In taking leave of Mr. Bryant we would express a sincere hope that we have not seen the last of him. We have not been able to speak in terms of unqualified praise of this work; but we have no doubt that if Mr. Bryant will bestow rather more pains on the style of his writings, and if he will avoid the error committed here, of overlaying his matter with a heap of material, his great experience and diligent observation will secure to him deserved success as a surgical writer.

Review VIII.

Report on the Pathology, Therapeutics, and General Ætiology of the Epidemic of Yellow Fever which prevailed at Lisbon during the latter half of the year 1857. By Dr. Robert D. Lyons. Presented to both Houses of Parliament by command of Her Majesty. pp. 127.

This is a valuable addition to the history of yellow fever, and the thanks of the profession are due to the author for it, especially considering the disinterested manner in which he entered on the inquiry, going to Portugal not as a Government *employé*, but *suâ sponte* as an unpaid volunteer; and, moreover, taking into account the zeal and energy with which he conducted it and carried it out. Another praiseworthy quality belonging to him that requires mention is, that he engaged in the study of the disease with a mind, as he assures us, unfettered by any predetermined conclusion as to its origin and propagation—a merit this the more commendable, the more rare—sad that we should have to say so, reflecting how much the contrary conduct has stood in the way of truth, and has aggravated the difficulties of an investigation in itself by no means easy.

That the epidemic was true yellow fever is shown by the author in the clearest manner; indeed, so well marked were its symptoms that there was no room for question respecting its nature. The first cases of it occurred on the 19th of September, the last early in January. During this period, according to the most reliable returns, between 16,000 and 17,000 of the population of the city were attacked—about 1 in 12·125 of the whole—of whom about 5500 died—1 in 3—which is near the average mortality from the disease as it has occurred at different times in the West Indies, the Continent of America, and the South of Europe.

Though the disease was generally considered to have been imported and contagious, and this not only by the people at large, but by many of the educated class, and by many respectable members of the medical faculty, yet the author sought in vain for satisfactory proof; all his inquiries had negative results. His words deserve to be quoted. He says:—

"After most careful inquiry amongst various official persons, and in all quarters in which reliable evidence could be expected in such matters, I am obliged to state, that in no one instance did I obtain such a consistent assemblage of facts, or such an array of well-supported allegations as would, in my mind, warrant the conclusion that the importation theory was moderately well founded. On the contrary, so vague, and in some particulars so conflicting, were the allegations as to the time, place, and other circumstances attending the reported importation of the disease, that I do not believe that the popular opinion so generally held as to the importation of the epidemic, admits of being reduced to anything like a uniform or consistent statement in writing.

"Of the reports in circulation as to the alleged importation, the two most generally received were—firstly, that the disease was communicated to a person in the Custom House engaged in the examination of the baggage and personal effects of passengers arriving from the Brazils; secondly, that the manner of importation was by certain cargoes of hides from the Brazils. In connexion with the first report, it is to be observed that there was no uniformity in the accounts given by different persons of the circumstances of time and place under which the importation was alleged to have occurred. There was no concurrence of testimony as to the particular ship, the date of her arrival, the port she sailed from, or the health of the passengers she brought. By one set of persons the ship specified was the Brazilian mail steamer *Tamar*, by others a different ship was particularized with equal confidence." (p. 6.)

To this may be added in confirmation his summary statement of the chief circumstances, which could be established on unquestionable authority, as to the times and places of, and the other particulars attending, the outbreak of the disease. On account of the important nature of this information we shall give it in the author's own words:

"1. It is proved beyond possibility of doubt, that true yellow fever of a malignant and fatal character, prevailed both in Lisbon and Oporto during the year 1856.

"2. Of 311 cases recorded to have occurred in Lisbon in 1856, somewhat more than two-thirds were observed in the districts St. Roque and Bica, far removed from the river margin.

"3. The three districts attacked in 1856, Belan, St. Roque, and Los Anjos (Bica), are widely separated from each other, and present no recognisable connexion, commercial or otherwise, amongst themselves (see map of Lisbon), nor with the interior by way of sea traffic. These districts are likewise on different elevations; one, that of St. Roque, being some one hundred to one hundred and fifty feet above the Tagus level.

"4. The yellow fever of Oporto (1856) is attributed by Dr. Gonveia Ozorio, of that city, to the filth of the Quarter Miragaia, in which the disease prevailed, and not to importation.

"5. While, in general, the parts of the city chiefly attacked in this Lisbon epidemic of 1857, were the thickly inhabited districts in the flat of the town bordering the river, it is undoubted that ravages equally great, if not more severe, were committed in certain districts having more or less elevation above the Tagus.

"6. The districts of the Mouraria, Alfama, St. Roque, and St. Catherine, which were greatly scourged by the epidemic, are on various elevations, and are more or less separated from the flat of the city and the river margin.

"7. While there is no uniform testimony as to the time, place, and other circumstances of the alleged importation of the epidemic in 1856 and 1857, it is certain that no case of the disease was received into the Lazaretto in either of these years, or in any recent year.*

"8. All the parts of the city largely attacked by the epidemic, present in common certain conditions of insalubrity, which may be classed as follows:—

"A. Defective water supply.

"B. Total absence of, or more commonly extremely deficient, sewerage.

"C. Total absence or incompleteness of house-drains, privies, and a consequently unclean state of the streets.

"D. Badly-constructed dwellings, with deficiency of light and air, and want of thorough ventilation.

"E. Absence or defective condition of tertiary and secondary sewers; when present, such sewers, as well as those of the largest section, were proved by repeated inspection on the part of the Fourth Commission of the Sanitary Congress, and by my own personal observation on several occasions, to be in great part wholly ineffective, being choked with soil, sand, and rubbish, and giving exit at their main embouchures on the Tagus, either to little or no fluid or solid material, or to only a small quantity of clear and almost pure water.†

"9. The state of the shore of the Tagus, for fully one-half of the extent of the city along the river side, is such as to be a highly effective cause of insalubrity, not only to the adjacent districts, but to the whole capital. The vast area of extremely fetid decomposing mud left exposed at low water, exhales noxious gases very prejudicial to health.

"10. It is to be observed, that, notwithstanding the free communication between the shipping, and the Tagus and the shore, and between the city and the adjacent districts in the immediate vicinity, and at more remote distances, to which many thousands of the inhabitants (probably 30,000) withdrew, there is no evidence whatever of the disease having been conveyed to the shipping or to the interior.

"The freest communication was kept up, even during the height of the epidemic, between Lisbon and Cintra, and many other favourite places of resort, to which the citizens retired in great numbers; yet no cases can be adduced to show that the disease spread, or was carried by contagion or otherwise, from Lisbon to such localities. Cintra was, perhaps, the place most frequented, and with which much free daily communication was consequently kept up; but I could obtain no reliable evidence that one single case of genuine yellow fever occurred in that town.

"I have heard statements to the effect that the disease was propagated by contagion to some minor towns, but I have seen no evidence, documentary or otherwise, to prove the truth of such allegations, or render them even probable. On the contrary, the uniform testimony of popular report (so far as it is at all reliable, or worth noticing) went to show that the disease was not propagated beyond the walls of Lisbon." (pp. 6, 7.)

These local circumstances, so unfavourable to health, so favourable to the production of disease, and of such a disease as the dire one in question, on cursory consideration, may appear adequate to account for its origin; but, if we keep in mind that all of them are of a persistent kind, liable to little variation from year to year, and yet the malady, as an epidemic, has been of rare occurrence, the last being only the third recorded, formally and by name, from the year 1191 to the

* See Appendix VI. pp. 15, 22, where it is stated as follows: "The inspector of the Lazaretto, who has resided there forty-two years, affirmed in the most positive manner that there has never been one single person of those undergoing quarantine who was attacked with an epidemic disease."

† For details see Procès Verbal No. 1, and Procès Verbal No. 2, of Visits of Inspection of Fourth Commission, Appendix VI., pp. 119, 120.

present time, as having broken out in the Portuguese capital, we cannot be satisfied with this ætiology. The author, with great industry, has brought together a large amount of information respecting the climate of Lisbon in connexion with the appearance of the epidemic. The results as expressed in tables, including most meteorological phenomena which can be measured by instruments and tests, are not without their value, but they do not appear to throw any satisfactory light on the invasion of the disease. Little else is shown than that at the time it occurred and whilst it lasted, the atmospheric temperature was a little above the average, and the degree of atmospheric moisture a little higher than usual, that following a fall of rain a little in excess of the ordinary quantity. We express doubt in this matter, reflecting on the appearance of the disease at other seasons and in other countries, and under climatological conditions the opposite of those just referred to, especially in our West India Colonies. One of the most severe epidemics there was that of 1847–48, in Barbadoes, where it broke out without any grounds for supposing that it was imported, and was entirely confined to the garrison, which was in constant communication with the town, and this at the coolest season of the year, and when the weather was very agreeable, and it might be supposed favourable to health. In seeking for causes, especially of diseases, how much caution is required! If the inductive method is needed in one inquiry more than another, it is surely in this, in which the imagination is so apt to overpower the reason, and amidst the panic of dread any plausible circumstance is likely to be seized on as the *causa mali.* At present we are of opinion, that on the inductive plan the only conclusion we can arrive at is, that we are ignorant of the immediate cause of the disease, and that our sound knowledge is limited to the conditions, such as those described by the author, conducing to its production; conditions of the first importance, insomuch that, if corrected or removed, the great probability is the public health will be secured. In justice to Dr. Lyons, we must say that the caution we hold to be necessary, has been observed by him, he having pointedly forborne fixing on any one cause, or combination of causes, excepting as auxiliary, for the origin of the epidemic.

It is gratifying to find, and highly creditable to the humanity of the Portuguese, that though the disease was considered contagious, yet the sick were not forsaken by their friends, but were as assiduously nursed as if no apprehension had been felt of danger in the performance of the duty. The author attributes this in part to the conduct of the Government in giving to the public daily returns of the progress of the disease, thereby checking that tendency to aggravation of the evil which, when the fears are concerned, is sure to arise from flying rumour—that *Fama volans* which the poet has so well and truly described:

> "Magnas it Fama per urbes:
> Fama, malum quo non aliud velocius ullum:
> Mobilitate viget, viresque acquirit eundo.
> Parva metu primò; mox sese attollit in auras,
> Ingrediturque solo, et caput inter nubila condit."

The part of the monograph which we have read with most interest is the first section of the Report, giving the pathology and medical history of the epidemic. As in most other epidemics, the disease exhibited many varieties. The most characteristic of these, according to Dr. Lyons, were the following: 1. The algid form; 2. The sthenic; 3. The hæmorrhagic; 4. The purpuric; 5. The typhus form.

Dr. Lyons expresses his belief, and we are disposed to agree with him, that, owing to want of due attention to these different forms, and we might add other occasional forms, much discrepancy of statement and opinion has arisen amongst authors in their account of this disease as it has appeared in different localities.

For the details of the symptoms of these five varieties of yellow fever, we must refer to the work itself. All that our limits will allow us to do is to notice such as are most remarkable.

In the algid form, in a large proportion of cases, the hæmorrhagic tendency was present at an early stage, and in a very marked degree. Black vomit was a constant phenomenon. The temperature, not only of the extremities and surface, was below the natural, but also in the axilla, sometimes as much as 2° lower. In extreme cases the lips, the breath, and even the tongue, were cold. This depression of the animal heat was accompanied by a feeble action of the heart and prostration of strength. It is in this form that the disease shows itself in its greatest horrors. We shall quote one case to give those happily not familiar with yellow fever an idea, however imperfect, of its terrible aspect, transcribed, we are told, from the note-book of the author:

"A young girl, aged sixteen, was admitted into hospital on the 10th of December, at ten P.M., having been ill since the 4th. She was described as having hysteric symptoms on admission. When seen at the hour of visit (nine A.M.), on the 11th, she was still in a profoundly algid state, though all proper means had been used to restore reaction. She lay cowering and shivering under the bed-clothes. She was quite pulseless, the hands and feet were cold, and the hands soiled with blood; the face was of a bluish livid tint; the eyes darkly congested and dull; the alæ of the nose, lips, and teeth covered with bloody sordes; the tongue was moist, slimy, and bloody. There was complete absence of yellow colouration of any kind; the surface generally was semi-cyanozed, and everywhere thickly covered with well-marked purpuric spots. Dark blood oozed from the nose and gums, and black vomit supervened, and likewise vaginal hæmorrhage. There was retention of urine. She was still hysterical at the hour of visit—trembling, fearful, and disposed to cry. She retained her faculties of sense, could understand questions, and made efforts to reply, but could not articulate. There was but one end for such a case. She died on the following day." (p. 17.)

Of the sthenic form the cases were less numerous and less formidable than of the algid. They were commonly well marked by febrile symptoms, with an increase of temperature, varying in the axilla from 102° to 105°, with an increased quickness of pulse and increased cardiac action. Yet the two—the temperature and pulse—were not in harmony. This is shown in the following table; the numbers denote different cases:

						Degrees.
No. 1 . .	Pulse,	113	full	...	Thermometer in axilla,	102
„ 2 . .	„	100	„	...	„	100
„ 3 . .	„	112	„	...	„	104
„ 4 . .	„	104	„	...	„	103
„ 5 . .	„	92	„	...	„	101
„ 6 . .	„	100	„	...	„	103½
„ 7 . .	„	104	„	...	„	103
„ 8 . .	„	100	„	...	„	104½
„ 9 . .	„	84	„	...	„	99
„ 10 . .	„	70	„	...	„	100½

The hæmorrhages in this form were not so profuse as in the algid, nor so numerous as in the hæmorrhagic form; or there was a complete absence of any tendency to cutaneous extravasation. In this class of cases recovery was most frequent; and it was in them only that, to quote the words of the author, "the therapeutic measures employed were really at all efficacious and attended with manifest benefit."

The next form, the hæmorrhagic, was of all the most fatal, and often so at an early period, within five to seven days. In some of the most characteristic cases the pulse was 92; the temperature 101°.

"The transition from the sthenic to the hæmorrhagic form was sometimes marked by a diminution in the rate and volume of the pulse and a lowering of the thermometer; the patient's strength sank visibly at the same time; and in some cases a clammy sweat bedewed the face for one or two days. This state of things was followed by, as it were, an explosive universal lesion of the vascular system, leading to profuse hæmorrhages at all available points." (p. 27.)

"We have observed," the author states, "in combination in a single case,

1. Hæmorrhage from the eyelid.
2. „ the nose.
3. „ the gums.
4. Black bloody crusts on the tongue.
5. Hæmorrhage from stomach—black vomit.
6. „ intestines.
7. „ blistered surfaces.
8. „ the vagina was superadded in some cases of females." (p. 28.)

The purpuric form was distinguished from the algid—in which ecchymosis was a constant occurrence—by a pyrexial state, a frequent pulse, and a hot skin. The cases of it were few. One is given as an example, which we shall quote, with the author's remarks on it. He prefaces with the observation that it was one of the most remarkable of those which came under his notice:

"The patient, a man aged forty to forty-five, had presented the usual history and symptoms; on the eleventh day bleeding from the gums still continued. The skin was hot and everywhere yellow, as were likewise the conjunctivæ. The day following a large purpuric patch, four to six inches square, was observed in the left axilla, spreading to the front of the chest, which further presented several smaller patches of similar colour, extending to and a little beyond the mesian line. Slight but manifest œdema of the whole anterior part of the chest was observable. The skin was hot; the pulse only 70, but full. Two or three days subsequently a similar but much larger patch appeared in the right axilla, which eventually occupied the whole lateral aspect of the right side of the chest, and extended to within a short distance of the crest of the right ilium; it likewise encroached upon the back. The colour of the patches varied from a reddish to a bluish purple; the borders were gradually shaded off in successive tints, till the ordinary yellow surface was reached, on either side of the patch. The pulse had fallen to 52, the tongue was moist, but the skin still remained hotter than natural. The patient was still very yellow. Ascites subsequently appeared. The case lingered out for more than ten days after the first observation of the purpuric spots, and ultimately died.

"The patches observed in this remarkable case were precisely similar to those so often seen in the epidemic purpura hæmorrhagica of Ireland. Putting apart the yellow colouration, and if we bear in mind the presence of bleeding from the gums and the spongy state of the gingival membrane, in both diseases the parallel was very close indeed.

"This class of cases," the author adds, "is of great interest in connexion with the question of the cause of the colouration in yellow fever. The purple patches were manifestly caused by different subcutaneous effusions of the colouring matter of the blood, and all the varieties of shades of colour and tints were observable, from bluish-purple to yellow at the circumference of the patches, where the tint passed into that of the surrounding skin." (p. 30.)

With this remark we are inclined to agree, admitting, as we think proved, that the ordinary hue—that which has given a name to the epidemic—is owing to the colouring matter of bile, as in the instance of common jaundice.

The last form (the typhous form) was the rarest of all; cases of it were of "extreme rarity." They were marked by stupor, nervous depression, muttering delirium, superadded to the hæmorrhagic phenomena; occasionally occurring in the early stage of the disease, occasionally at a later period, and after pyæmia. The few instances which came under the observation of the author all ended in death. On the post-mortem examination in each, the mucous membrane of the intestines was found free from follicular lesion.

Under the head of general phenomena common to all the forms, the following are described as worthy of note:—1. A constipated state of bowels, which the author considers peculiar to the inhabitants of Lisbon. 2. In some cases an almost complete suppression of urine, whilst in others it was normal and abundant—normal in specific gravity and reaction; in others loaded with lithates; in others coagulated by heat and nitric acid; and was occasionally brownish-red, smoke-coloured, or variously tinged, from more or less admixture of blood elements and of bile. The suppression of urine was confined to the algid cases; the abundance of lithates and other secretions to the sthenic cases; the albuminous condition was not special to any one class—it was found more or less in all of them; and the same remark applies to the presence of bile. 3. A swelling of one or other of the parotid glands, of an inflammatory kind, ending in suppuration, often exhausting and fatal.

On the treatment of yellow fever the author is very concise, and we shall follow his example, having, like him, but little confidence in any of the very many modes hitherto tried. Quinine and bark, which were largely used, he thinks unfavourably of. From the reported efficacy of the former in the epidemics of British Guiana, the atmosphere of which is more or less malarious or productive of ague and remittent fever, we are disposed to have faith in it under similar climatic influences. As a palliative, the author reports favourably of the perchloride of iron in checking the hæmorrhagic tendency.

The second section of the Report—that which treats of the pathological anatomy of the disease—is hardly less valuable than the one preceding it, being laborious and minute in details, and inspiring confidence from accuracy of description.

We shall first bring under notice some of the more remarkable negative results of the inquiry. No indications were detected of inflammatory lesions, none of active hyperæmia; no lymph in any case was found effused. The mucous surfaces appeared "intact," and were generally free from ulceration. No vessels were found ruptured.

Of the positive results, the most striking and constant were the hæmorrhagic phenomena, in the form of extravasated blood. This was found to occur to the greatest extent in the most vascular parts and those offering the least resistance, such as the stomach and intestines, the mucous tissues generally, and the cutaneous; and, *vice versâ*, it was in a less degree noticeable, but not without exceptions, in the least vascular organs, such as the serous membranes, and in the least yielding, such as the brain, the liver, the pancreas, the kidneys. The lesions discovered in the several organs and tissues, which are described *seriatim*, being little more than those just noticed, we shall pass over the details, with the exception of those relating to the liver and the bloodvessels, which (especially the former) have had the careful attention of the author.

He found it in every instance more or less altered in colour; a rich fawn, yellow, or buff was its most prevailing hue. Its substance was pretty firm and resistent to the touch; the viscus itself seldom much enlarged, and equally rarely much diminished in volume. Subjected to the microscope "in minute fine sections," or "in matter scraped from the hepatic texture," these

"Exhibited the hepatic cells filled with globular oily and fatty matter. The natural appearance of the cell was completely altered, its outlines obscured, and its nucleus rendered invisible. It was surcharged with molecular and globular oily matter, while the whole field and the interspaces between the cells were filled with similar and equally abundant oily and fatty elements." (p. 45.)

With the intent of determining the amount of the fatty deposit, he employed two methods, one hydrostatic, the other chemical. In a table he gives the results of the first, from which it appears that, with the exception of 2 out of 34 tried, the specific gravity was less than that of the healthy liver, ranging in the ratio of from 6½ to 9 to that of 10, the latter number standing for the sound organ. The chemical method consisted in extracting the fatty matter by means of ether. The results of ten trials given, to some extent confirm the preceding, as in all of them, with the exception of three, the quantity of fatty extract obtained was in excess of that from the organ supposed to be in its healthy state.

We think that the author is probably correct in his conclusion that the liver in this disease is often unduly loaded with fatty matter—a point already adverted to by Dr. Blair.* But that the liver is constantly so loaded, appears to us questionable : in other words, we hold it doubtful that fatty degeneration, or indeed any organic lesion of this viscus, is an essential accompaniment of the disease.† Not to mention other reasons, we think our doubt is supported by the rapid manner in which convalescence is witnessed in the great majority of those who are so fortunate as not to fall victims to the malady. The subject is worthy of further research.

Relative to the condition of the bloodvessels, the author's researches have been altogether of a negative kind, leading him to the conclusion that "the hæmorrhagic action" is "confined to vessels approaching the capillary condition, if it did not take place directly and exclusively from the ruptured capillaries." (p. 42.) This remark he makes when treating of the gastric membrane, but it is equally inferential from his observations on the other organs and their contents, especially the fluid *débris* found in the intestines. Need we say that we should have been better satisfied had the author by his microscopical researches confirmed his conclusion, remembering as we do that what he has inferred has been proved to be the fact by Dr. Blair, as described by him in a letter written in 1856, attached to the Report already referred to? And in noticing this, we feel rather surprised that Dr. Lyons in no instance makes mention of Dr. Blair's labours, as if totally ignorant of them. We regret it the more, believing that he might have derived many valuable suggestions from them, and that, had he assiduously sought for ruptured capillaries with a suitable magnifying power, he could hardly have failed to have detected them. We would specially recommend this letter of Dr. Blair's to his notice, as well as an examination of the cabinet of microscopical objects illustrative of the pathology of the disease, prepared by this zealous inquirer, which are deposited in the museum of the Royal College of Surgeons, and amongst which are preparations showing the lesions of the capillary vessels.

For many observations made by Dr. Lyons on the black vomit, the contents of the gall-bladder and of the intestines, we must refer to the work itself, merely mentioning that they are generally in accordance with those of former inquirers, especially of Dr. Blair, though hardly as searching as his. On the state of the blood during the course of the disease, no information is given; it does not appear to have had the author's special attention. This, too, we regret, as in opening the monograph we had hoped to have found some positive results respect-

* Vol. xvii., Appendix, p. 58.

[† The recent observations of acute fatty degeneration of the liver in temperate climes, if established, go far to support the view that fatty degeneration of the liver may be essential in yellow fever.—Ed.]

ing so important a fluid. But we must recollect that there is a limit to labour, and be thankful rather for what has been done, than dissatisfied for what has been left undone. In quitting this part of the subject we are fain to remark, that reviewing the lesions as described by the author in his account of the pathological anatomy of yellow fever, there seems to us to be a very satisfactory accordance with the symptomatology of the disease; and if we add to his observations those of Dr. Blair respecting the condition of the capillaries, we are disposed to consider the phenomena—the symptoms—pretty well explained, and not a little inclined to that theory propounded by Dr. Blair of its nature, that is, of the series of morbid actions of which it consists. This theory, if we may so call it, is the following:

"Finding," Dr. Blair says, in the letter already quoted, "that ecchymosis of the conjunctiva, epistaxis, and some other hæmorrhagic appearances, are common in yellow fever long before the blood has lost any of its fibrin, and finding that even when black vomit is established and the tongue is smeared with blood, the corpuscles are normal in appearance, I cannot but look on the textural lesion of the capillaries as a primary effect of the yellow-fever poison, and as the *cause* of the congestions, ecchymoses, oozings, and hæmorrhages, and all their consecutive mischief."

In a letter, which is now before us, of a later date—viz., of 25th May, 1856—he develops this theory more in detail. We are tempted to transcribe it in part, as it has never yet been published, and as giving his last thoughts on a subject to which he applied all the powers of a mind of more than ordinary vigour. It was written shortly before his setting out on that exploring excursion into the wild interior of British Guiana from which he only returned to die of the fever contracted in prosecuting it. He says:

"My creed, now, may be stated almost as follows: The proximate cause of yellow fever is an aerial poison which enters the system through the mucous membranes, on which it impinges and to which it becomes attached. In some extreme cases all the mucous linings of the eyes, nares, alvine passages, and bronchi are poisoned at once; but more commonly the first application of the poison is only partial. The primary and specific action of the poison is on the capillary vessels, and this action spreads and extends itself until the large viscera and bladder become affected in varying degrees. The mode of action of this poison on the capillaries is, first, as an irritant, and it ends by inducing a physical impairment of the tissue. The consecutive lesions are, desquamation of epithelium, exfoliation of basement membrane, sloughing of the minute capillaries, and deep erosions. The symptoms are first those of an irritant poison, and afterwards are made up of the composite results of hæmorrhage and the circulation of blood which has been contaminated by the impaired functions of the secreting and excreting viscera."

The second part of Dr. Lyons' work relates to the rise and progress of the epidemic. We shall make from it only a few excerpta. The deaths of males, it would appear, were nearly as to two to one to those of the females. The mean age of those who died, calculated on 210 successive entries into hospital, was 33·5571 years. Very few children under ten were attacked, and few old persons over seventy. The mean duration in hospital was six days; of those cured, eight days; of fatal cases, four days. The negro race were in a great measure exempt, seeming to enjoy their usual immunity from attack. In the worst quarters of the town the mortality was forty-two and forty-three per cent.; the mean mortality being about thirty-three per cent. of those attacked.

"The immunity enjoyed by the shipping in the Tagus, notwithstanding the great and most constant intercourse with the town, is very remarkable." The author adds: "I know of but one instance of a British seaman (master of a brig) having become a victim to the disease. The individual in question had been drinking on shore: he died in the British Hospital." (p. 61.)

The third and concluding part, consisting of two sections, treats of the physical history of Lisbon, and of the general climatology of that city. It is a portion of the work that displays much careful research, and it will amply repay the reader who may be interested—and who should not be?—in the history of our fellow-men, and in the sanitary condition of great towns. The topography and climatology of the Portuguese capital is illustrated most amply, even *ad luxuriam*, by maps and graphic charts, and meteorological tables.

Another part of the work deserves notice—it is the Appendix, in which some interesting and useful information is given relative to the epidemics which the city has experienced from the year 1191 to the present time; and also respecting the state—a very bad state—of the sewage and drainage of Lisbon.

We commenced our notice of this monograph in terms of commendation; we cannot but reiterate them in conclusion, believing that the author made the most of his brief opportunity, thereby affording an example of how much may be accomplished where zeal is coupled with ability.

Review IX.

Mémoires de la Société de Chirurgie de Paris. Tome cinquième, Fascicales 1 et 2. *Paris*, 1858–9. 4to. pp. 275.

Memoirs of the Society of Surgery of Paris.

Passing over a biographical notice of P. J. Roux by M. Réné Marjolin, and a historical eulogium on Gerdy by M. Paul Broca, we come to an essay on,

I. *Capillary Punctures in the Treatment of Collections of Blood and Pus.* By M. Vollemier.—The employment of capillary punctures, Dr. Vollemier states, has furnished him with some excellent results during the three years he has had recourse to them. The term capillary, however, does not properly indicate the size of the instruments employed, but it denotes well enough the size of the openings themselves, which are as small as they can be to admit the passage of blood, serosity, or pus.

1. *Capillary Punctures in Effusions of Blood.*—Twelve cases are related in illustration of the practice. In the first in which it was resorted to, a large effusion of blood had followed the passage of a wheel over the leg, and the most fluctuating point was punctured,

twenty days after, in six places, with a good-sized needle. The punctures were repeated on four occasions, the fluid discharged becoming more and more serous; and the patient at the end of twelve days was considered as cured. In the other cases, the blade of an exploratory trocar, one or one and a half millimetre in diameter, was substituted for the needle, with advantage, its canula only being used when considerable thickness of tissue had to be traversed. We should avoid the portions of skin which are reddened and inflamed, or fistulæ may result; and the penetration should not be made too near the base of the tumour, where the tissues are very thick, while in withdrawing the trocar care must be taken not to convert the direct track into an oblique one. As a general rule, it is best to wait until the tumefaction has become defined, the serous portion of the blood has separated, and fluctuation is manifest; but if, from distension or thinness of the skin, bursting is threatened, puncture should at once be performed, evacuation of the sac being the best preventive of its inflammation. In general, three or four, and sometimes one, punctures will evacuate a considerable collection, and two days should be allowed to intervene between each operation; and care should be taken not to pass the trocar into the old spots. The fluid ordinarily issues as after the bites of leeches, and its flow should be aided by moderate compression.

2. *Capillary Punctures in Purulent Collections.*—The success obtained in sanguineous effusions induced the author to extend the practice to purulent collections. He found that a small trocar without any canula best secured the exit of the pus; and the precaution of not repeating the puncture at the same spot is not here necessary. Indeed, the aperture may become fistulous for a time with advantage, all excess of inflammation being obviated by means of cataplasms.

The author dilates especially upon the employment of his method in abscesses about the neck, in abscesses in the groin, and in critical abscesses following fever, and illustrates his remarks by numerous cases.

II. *Remarks on the Fatal Results of V-shaped or Cuneiform Fractures.* By M. Gosselin.—In this paper M. Gosselin does not enter into the anatomical details of the V-formed fractures of the tibia. These have been sufficiently adverted to in the 'Bulletin' of the Society (tome vi. pp. 259 and 262), and in the 'Gazette des Hôpitaux' (1855, p. 218); and his object now is to call attention to certain consequences of these fractures which have not attracted the notice they deserve.

The first point to be now noticed is the relationship prevailing between the V-form of the fracture and the more or less multiplied and extensive lesions of the lower fragment. The fissuring or cleaving of this is produced by the elongated point of the upper fragment penetrating between the two branches of the lower V, and acting upon it like a wedge. The term *cuneiform* fracture proposed by M. H. Larrey is therefore a just one, although perhaps that of V-formed better indicates the primary direction which favours, and in some sort prepares for, the wedge-like action of the upper fragment. In regard to their mechanism, there is a resemblance between these fractures and the bicondylian fractures described by Malgaigne and Trélat, in which the two condyles are separated by the penetration of the superior into the inferior fragment. The same resemblance prevails between them and fracture at the base of the great trochanter, described by Hervez de Chégoin and Robert, in which the upper fragment formed by the cervix penetrates into the lower, crushing and splintering the trochanter; as also fractures of the lower end of the radius, made known by M. Vollemier, in which the lower fragment assumes a stellated form through the penetration of the upper one. The peculiarity of the tibia among these various cases is, that the penetration takes place in the diaphysis and not at the end of the bone, so that the lower fragment may present a very long solution of continuity.

Another point calling for attention is the gravity of this description of fracture, at least as far as regards the adult. The six cases which have come under the author's notice all died, although in two of these the fracture was simple; the period of death and the nature of the accidents exhibiting great similarity to what is observed after amputation or gun-shot wounds of the thigh or leg. In examining into the cause of death in these various cases, we must make a distinction between the cases in which death comes on rapidly, say within the four first days, and those in which it takes place at a later period. With respect to the early deaths, there does not seem to be much agreement of opinion. They have by turns been attributed to the intensity of the traumatic fever, nervous accidents, anterior predisposition, and the advent of pernicious fever. The author rejects these explanations as too vague and incomprehensible, and attributes the deaths to "poisoning, a kind of traumatic typhus, or traumatic poisoning, to use an expression already employed by M. Chassaignac."

With respect to the deaths which take place from the tenth to the twentieth day, these have been attributed by great numbers of surgeons, from the remotest times, to an intoxication, pus passing into the circulation being indicated as the agent of its production. M. Gosselin, completely adopting this doctrine of purulent infection, draws from such consecutive intoxication an argument in favour of death, when it takes place at an early period, being also due to poisoning. But it is by no means necessary to adopt the general opinion, that pus as pus is always the poisonous agent; for in many instances patients have died with all the symptoms of purulent infection, without there being any pus in the veins, and without any metastatic abscesses having formed; and at other times there have been purulent infection with metastatic abscess, without pus in the veins. While, therefore, pus may serve as the poison in certain cases of purulent infection, the toxical agent may be different in other cases, and particularly in those in which suppurating phlebitis does not exist.

"What, then, is this untangible poison which, introduced by a wound, may kill a patient in a few minutes, or in a certain number of days after the injury? Glad should I be to be able to name and exhibit it; but that is as impossible as it is to exhibit the poison of farcy, charbon, syphilis, &c. Analogy and reasoning declare that poisoning there is, and it is from analogy and reasoning we are alone able to obtain indications of the nature of the poisonous agent."

Lesions of the bones seem to present an especially favourable condition for the occurrence of this intoxication. It can scarcely be contended that the bones possess a greater power of absorbing any morbid products they may be brought in contact with than the soft parts; and although great lesion of the bone must add to the entire size of the absorbing surface, yet it is difficult to explain why the same effects should not be observed in children when the extent of lesion has been the same. M. Gosselin is disposed to refer the accidents to *a changed condition of the medulla* of the bones, a change which the contact of the air may not be always necessary to produce. This, too, would explain the comparative insusceptibility of children, in whose bones the medulla exists in smaller quantity, and with a different composition. In these V-formed fractures, by reason of the prolongation of the solution of continuity the absorbing surface is increased, and by the mechanism of their production, the medullary substance undergoes much greater injury than in ordinary fracture.

III. *On Coxo-femoral Disarticulation in relation to Military Surgery.* By M. LEGOUEST.—After the battles of the Alma and of Inkermann this operation was performed upon thirteen patients, all of whom died. The case of one of these, a Russian prisoner, is related at some length by the author. He was very nearly being saved, when he had a fall upon his stump, and was carried off by diarrhœa, owing to his being placed amidst a large assemblage of patients confined to a small space, many of whom were the subjects of scorbutus, hospital gangrene, or purulent infection, and almost all were suffering from diarrhœa, dysentery, or cholera. This approach to success has given the author little desire to repeat an operation the value of which, he says, is much less in the eyes of the surgeon than in those of the public; and he thinks its performance should be reserved for organic diseases of the femur incurable by any other means. He has endeavoured to collect all the instances in which this operation has been performed after gun-shot injury. In 30 of these the operation was *immediate*, the whole dying; in 11 it was *mediate*, 8 dying and 3 recovering; and in 3 it was *ulterior*, 2 dying and 1 recovering. Of the first category, some died during the operation itself, others soon after they had been carried to their beds, and all within ten days, except 2 patients related by Larrey, one of whom lived twenty-one and the other thirty days. Should not an operation furnishing such results be positively interdicted? The proportion of three recoveries in eleven mediate operations is, on the other hand, a large one, especially when we consider the mortality from amputation of the thigh, and probably arises from the fact of all the unsuccessful cases not having been made known. Ulterior operations, i.e., those performed about a year after the accident, have succeeded once in three times, analogous as they are to the amputation necessitated by disease of the femur. Now, can this slight amount of success, which is not even so great as it seems, be compared with the results derived from attempts at the preservation of the limb. Unfortunately, many surgeons, almost in spite of themselves, lay more stress upon a successful case of operation than upon a recovery obtained by assiduous care, and by an ingenious combination of all possible appliances, independently of cutting instruments. The success of a dangerous operation is willingly published, while the more humble but more precious triumphs of conservative surgery are unrecorded. This is the case with most of the instances of fracture of the cervix or the trochanters which have been treated with preservation of the limb. M. Legouest has, however, here collected six instances, one of these having occurred in Larrey's practice, another in Sedillot's practice, and four in his own. Of these 6 patients, 3 lived; while in 44 cases of amputation of the thigh, recorded as having been performed by skilful surgeons, and from which all doubtful cases have been eliminated, scarcely can 4 cases of authentic cure be discovered.

The practical conclusions he arrives at are, first, that all immediate amputations—i.e., before the early phenomena of suppurative inflammation set in—should, as laid down by Sedillot in 1841, be rejected. At all events, every effort should be made to obtain some delay. When an operation has to be performed, on account of gun-shot wound of the femur, without lesion of the vessels, with large splinters that cannot be extracted, an amputation through the trochanters, or immediately above them, is preferable to disarticulation. This operation does not deserve the proscription that has been directed against it. The wound is smaller and less deep, and the head of the femur remaining in the normal position, if it does not diminish the danger of the operation, facilitates the subsequent application of prothetic apparatus. When, too, there is intra-articular fracture of the cervix, or fracture of the head of the bone itself, resection of the upper part of the femur should be preferred to disarticulation. This operation has now been performed in 12 or 14 cases, and in more than a third it has succeeded. It is true that it failed in the two cases of gun-shot fracture it was tried in; but how many times had coxo-femoral disarticulation been attempted before four authentic recoveries could be recorded? Complete ablation of the limb should be reserved for cases of fracture with lesion of vessels, for if these remained intact, resection should be preferred. To sum up: in ordinary cases—which are by far the most numerous—we shall derive more advantage from preserving than from removing the limb; and when an operation is determined upon, we should defer it to the latest period consistently with the general condition of the patient.

IV. *Report by* M. Bouvier *on* M. Crocq's *Treatment of Fracture and White Swelling.*—M. Crocq's work on white swelling was published some years since. M. Bouvier believes the author has mistaken several cases of simple arthritis for white swelling, and that the success he attributes to the starch bandage in the treatment of this affection is much exaggerated.

V. *Anatomical Description of an Arterio-venous Aneurism at the bend of the Elbow.* By M. CHARNAL; *with a Report by* M. PAUL BROCA.—This case had long excited great interest in the Paris surgical circles, and had already been made the subject of an interesting paper by M. Follin, in a preceding volume of the 'Memoirs,'* in 1850. In 1856, the patient again applied to the St. Antoine Hospital, having then a slight pulsatile elevation of the skin on the inner side of the biceps tendon. On comparing the two arms there was no notable variation in the pulse or venous distribution. The patient died from pneumonia, and the parts were carefully examined. There was no adhesion of the skin, and the expansion of the biceps tendon was only slightly raised by the aneurism. As the artery approached the bend of the elbow, the external vein became placed in front of it, and at the upper edge of the expansion of the biceps tendon presented a swelling the size of a large pea, which in fact constituted the aneurism. Above and below the swelling the external and internal veins freely anastomosed, and completely covered the artery. At the most projecting part of the swelling the artery and external vein were intimately united, and at this point a communication was found between them, into which a probe easily passed. Near the aneurism the walls of the vein were thickened and rigid as those of an artery, a change not observed in the internal vein, while the coats of the artery were quite normal. Below the elbow the artery resumed its place between the two veins. Here also an arterial anomaly was observed: for the brachial artery, in place of dividing below the elbow into the radial and ulnar arteries, followed only the course of the latter, while the radial artery passed in the angle formed by the internal edge of the biceps, and the lower border of the expansion of its tendon. A premature bifurcation of the brachial had taken place at the upper part of the arm, and the arterio-venous communication took place only between the ulnar artery and its external collateral vein. This explains the strength of the radial pulse and the slight disturbance of the functions of the limb.

M. Broca, reporting upon this case, observes that the artery wounded was really the brachial, not the ulnar. All who have studied this part of practical anatomy (and it has particularly occupied the attention of Meckel, as also of the Reporter in the 'Bulletins de la Soc. Anat.,' t. xxiv. p. 49), must admit that in cases of this kind, however considerable may be the anomalies, there is always found, in the region of the arm, running from the axilla to the bend of the elbow, placed in the sheath of the median nerve, between the two deep brachial veins, an artery, variable in its calibre, but invariable in its relations, to which no other name than brachial can be given. The knowledge of this fact is of importance in surgery. Instances in which two arteries course along the arm are common enough to be taken into account in practice; and it is of use to know that this anomaly is submitted to certain rules, and that the situation of one of

* See Follin on Communication between the Brachial Artery and Deep-seated Veins British and Foreign Med.-Chirurgical Review, vol. v. p. 349.

these arteries is constantly normal. Want of precision here would lead to the admission of the existence of an aneurismal varix of the ulnar artery, an affection which, as far as the Reporter is aware, has never been met with. The satellite veins of this artery are probably too small to admit of the production of an aneurism; and the *phlebarterie* in this case was formed at a point where the satellite veins of the pretended ulnar were much larger than the ordinary deep ulnar veins. They were, in fact, the true brachial veins, perfectly regular in their origin, course, connexions, and calibre.

The high origin of the radial allowed the circulation to be carried on in a normal manner in one of the principal arteries of the fore-arm and hand; and by this circumstance M. Charnal endeavours to explain the benignity of the aneurism; but in point of fact, the production of serious accidents by varicose aneurism at the bend of the elbow is quite an exceptional occurrence. In most cases it only constitutes a slight infirmity, especially when, as in the present case, there is only a simple *phlebarterie*, without trace of aneurismal dilatation. Every surgeon must have seen patients who have lived for a great number of years, employing their arm freely, without any accident occurring calling for intervention, and even without the œdema being produced, which obliged this patient to suspend his labours from time to time. In fact, the disturbances which attend upon *phlebarterie* are manifested in the venous and capillary circulation much more than in the arterial—the latter pursuing its course scarcely affected by the small current flowing through a narrow orifice into the vein. It is different with the venous circulation. The current of arterial blood passing incessantly into the vein, and with a much greater impulse than that of the black blood, and directed towards the heart, opposes a serious obstacle to the centripetal movement of the liquid returning from the extremity of the limb. For this reason varicose dilatations are found not only opposite the *phlebarterie*, but below this point, and sometimes affecting the tributary veins of the injured vessel to a considerable distance. The physiological disturbance will therefore be found more proportionate to the size of the vein, and to that of the orifice of communication, than to the calibre of the adjacent artery.

This modification of the venous circulation may extend to the capillary circulation, and consequently exert some influence upon the phenomena of nutrition. Observing the patient whose case has led to these remarks during life, M. Broca found that while the pilous system was identical in the brachial region of both arms, the hairs of the fore-arm were larger, more deeply coloured, and longer on the affected than on the sound side. This is not the only instance of hypertrophy from the same cause of the hairs he is in possession of. Some short time since, the Reporter observed another fact of the same nature, and still more curious. The aneurismal varix occupied the thigh, the venous circulation being much impeded, so that large varices, complicated with œdema, affected the entire lower extremity. The patient limped, but he attributed this to weakness, and was far from believing it to be due to inequality in the length of the limbs.

Mensuration, however, proved that the affected limb was three centimetres longer than the sound one—viz., one centimetre for the femur, and two for the tibia, the foot itself being also elongated half a centimetre. The bones did not appear larger, although the varicose enlargement prevented this being accurately ascertained. Although these phenomena may possess only a secondary interest in their relation to pathology, properly so called, they are worthy of serious attention in their physiological relations. It need scarcely be observed how little they are in accordance with doctrines hitherto received; for it has generally been believed that nutrition, so far from being exaggerated, is impeded in parts under the influence of a *phlebarterie.* The derivation of a portion of the arterial blood, the enfeeblement of the pulse often observed in the arteries below, the sensation of cold expressed by most patients, and the diminution in the strength of the limb—a constant symptom, apparently due to muscular atrophy—are the circumstances which have led to the admission of insufficient nutrition, itself due to the insufficiency of the circulation, among the other effects of arterio-venous communication. However satisfactory this explanation may seem *à priori*, it is not in accordance with the facts mentioned, and it therefore calls for some examination. First, as to the state of the pulse; although this is sometimes manifestly diminished in power, in other cases it is not so, or even the reverse may be observed. In fact, soon after the establishment of the aneurism the artery implicated undergoes dilatation, which compensates for the loss of red blood that takes place by the arterio-venous orifice; and the arterial activity will depend upon the degree of such dilatation.

The sense of cold and numbness in the limb nowise proves the insufficiency of the circulation, for as in various other pathological circumstances, so in aneurismal varix, with a sensation of cold there may be a true elevation of temperature. The experiments of MM. Demarquay and Henry show that there may be an elevation of from $1\frac{1}{2}°$ to $3\frac{1}{2}°$ C. It is true that this elevation has not been observed in all cases, but in none has there been a diminution. It is now long since Hodgson and Forster observed the temperature of the lower extremity raised by from 2° to 4° C. some hours after the application of the ligature; and such increase usually persists during several days, or even for two or three weeks, the temperature becoming normal after the re-establishment of the collateral circulation. This increased temperature during obstructed arterial circulation is probably due to the greater temporary activity of the capillary circulation. In *phlebarterie* the capillaries become congested by another mechanism; but the congestion does exist, and it is permanent, the venous circulation being always much more impeded than the arterial.

Next, as to the muscular strength of the patient. There can be no doubt but that in most of the patients the limb is notably enfeebled, and it has been concluded from this that the muscles must have undergone atrophy. This is, however, a purely theoretical conclusion, for no one has verified it at an autopsy. In default of this, M. Henry has had recourse to circular mensuration, and he found that in a patient in whom the *phlebarterie* had lasted ten years, and had never been treated, while the circumference of the two arms was identical, that of the affected fore-arm, notwithstanding its feebleness, had increased by a centimetre. However, mensuration is an imperfect test, and it is to pathological anatomy we must appeal when the opportunity offers itself. The diminution in muscular power is probably due to disturbances of innervation analogous to those which impart to the limb sensations of numbness, illusory cold, pains and cramps. It is possible it may be due to congestion of the capillaries of the nervous cords themselves.

VI. *On Preternatural Anus.* By M. LEGENDRE.—In this paper, M. Legendre does not seek to give a complete history of preternatural anus, this having been already executed in the admirable works of Scarpa and Dupuytren, but merely to develope some points in relation to the pathological anatomy of the affection, the mechanism of its spontaneous cure, and a modification of the operative procedure.

Pathological Anatomy.—1. *Adhesions of the Intestine:* Adhesions are set up between the peritoneal surface of the intestine and that of the wall of the abdomen with great rapidity, and an examination of their progress is of importance, on account of the parts they take in the mechanism of the spontaneous cure of the affection. In a dog upon whom an artificial anus had been executed, adherent fibrils, capable of bearing a certain amount of traction, were found thirty-six hours afterward; and in a case of operation for gangrened hernia, Scarpa observed adhesions developed in twenty-four hours. The adhesions become more and more powerful and extensive, it being in the preternatural anus resulting from wounds of the abdomen that they acquire the greatest extent, offering a protection against effusions into the abdomen. In the case of the anus resulting from hernia, they are not so extensive, there being usually but one circle of adhesions on a level with the neck of the sac; while when the anus is the result of a wound, there is a circle of adhesions at the wound of the peritoneum, and another at the external wound, the parts, too, being united together between these two points. In some cases no adhesions take place with the hernial ring. When they do form at the ring, they afterwards extend to the neighbouring peritoneum, and in this way we may find one or more nooses of intestine adhering near to and supporting the aperture in the canal, two illustrations of which are given in this paper. The adhesions of the two ends of the intestine to the abdominal wall persist long after the preternatural anus has become cured, and then take on the appearance of a fibrous fold. It is difficult to admit, notwithstanding the great authority of Dupuytren, that the intestine can ever become free and floating in the abdomen. 2. *Adhesions of the Omentum:* Whenever omentum is present in a gangrened hernia, it forms adhesions with the circumference of the ring and the external tunic of the intestine; it frequently completely envelopes the intestine. So also with the *mesentery;* whenever it is in part destroyed by gangrene, it contracts adhesions with the intestine or the ring. 3. *Changes in the Intestinal Tunics:* When the two ends of the intestine have contracted adhesions to the ring, the inflammation propagated to its tissues determines changes in their nutrition and texture. Little notice has been taken of this, although it is of importance in regard to the mechanism of cure.

Mechanism of Spontaneous Cure.—After examining in detail the effects produced by the various pathological changes already described, M. Legendre furnishes the following general theory of the mechanism of cure:—

"We place in the first line the adhesions which unite the intestine to the walls of the abdomen and neighbouring parts. In certain cases they may suffice alone for the production of a complete cure, and their action is always necessary for this to be obtained. The phenomenon of the return of the intestine into the abdominal cavity, so important in the formation of the membranous infundibulum, is especially due to the contractions of the two ends of the intestine. But the cure of preternatural anus also exacts the concurrence of several phenomena, the contraction of the neck of the sac and of the cicatricial tissue which forms the membranous infundibulum gradually bringing about the cure, to which, through the retraction of their adhesions, the mesentery and omentum also contribute. All these phenomena occur in a certain order, which we have indicated in detail, and now we are about to group them theoretically, so as to represent their general operation in effecting a spontaneous cure.

"Immediately upon the occurrence of strangulation in a hernia, the inflammation which arises in the hernial sac and the intestine gives rise to adhesions between these two organs. When a portion of the intestine and its mesentery becomes gangrenous, the adhesions are limited to the circumference of the living portion whence the eschars have separated; and the same is observed with respect to a portion of intestine the protrusion of which from the abdomen has been caused by a wound. In both cases the adhesions between the serous tunic of the intestine and the adjoining parts, and with the mesentery and omentum in the vicinity, become multiplied These adhesions during their progress tend to raise up the intestine by reason of the facility with which agglutination between serous surfaces takes place, while the physiological contractions of the two ends of the intestine, feeble though they are, tend to the same end. This group of phenomena comes into action during a certain period, which we may designate as the *period of formation.*

"In a second period the adhesions have become quite organized, and then the more important phenomena take place which prepare the way for a spontaneous cure. These are but slightly marked in the preternatural anus which results from a wound of the intestine, in consequence of the too great extent of the adhesions. In that which results from a gangrened hernia, the two ends of the intestine are fixed to the abdominal parietes by adhesions which support the contractile efforts of the entire intestinal tube and of the two hypertrophied extremities. These movements, which tend to render the intestine free, act upon the newly-formed adhesions, or upon the upper part of the hernial sac when it is not adherent, inducing elongation and removal from the orifice, though in an irregular manner, the upper end being more active than the lower. A small cavity is formed, in which fæcal matters accumulate, leading to dilatation, and consequently aiding in the removal of the two ends of the intestine from the external orifice. A communication between the two intestinal orifices may already exist. This may be termed the *period of organization.*

"In the third group of phenomena, which may be denoted as the *period of retraction*, the preceding processes continue in operation, and a new action is added — the retraction of the newly-formed tissues. It operates on the adhesions uniting the intestine to the wall of the abdomen, and especially in the cicatricial tissue, which constitutes the membranous infundibulum." (pp. 258-260.)

Radical Cure.—After passing in review the various modes of treatment recommended by different surgeons, M. Legendre explains his own procedures.

"When the operator finds the intestine gangrened in a hernia, he must assure himself whether it has contracted adhesions to the circumference of the ring. If such adhesions do not exist, and the intestinal noose is a very small one, the best practice will be to fix the two ends of the intestine to the edges of the wound by some points of suture, according to Palfin's procedure, as the contractions of the gut will sometimes cause their re-entry into the abdomen. If the noose is of considerable size, a thread should not be passed through the mesentery, as recommended by Lapeyronne, but discountenanced by Scarpa, the rapidity with which adhesions form around the gangrened portion, the re-entry of which simple muscular action cannot effect, rendering this step useless. When the anus has become established for some time, and the adhesions are firm, we must commence by destroying the spur by means of the *enterotome*, in order to obtain a small intermediate cavity for the passage of the fæcal matters in the two ends of the intestine, this being an indispensable condition for the cure. The employment of the *enterotome* exacts the greatest precautions. It may be that the posterior portion of the spur not being adherent throughout its entire extent, and mortifying too rapidly, allows of the issue of fæcal matters into the cavity of the abdomen. This redoubtable accident has been observed several times, and in other cases, when the non-adherent base of the spur leaves a sufficient interval between its two walls, a noose of the neighbouring intestine may become interposed within this space. The danger in such case of embracing all the spur with the *enterotome* is obvious. These conditions, demonstrated by pathological anatomy, not being indicated by any sign on the living, one is obliged to operate in the dark. It would therefore be a more prudent course to divide only small portions of the spur at a time, carrying the *enterotome* gradually into the cavity of the intestine, or to employ a better-devised instrument." (pp. 268–69.)

The author gives a description of a modification of Blandin's instrument, which it would be difficult to render intelligible without the diagram. The effect of its operation is a more gradual division of the spur and a greater protection of surrounding parts. After the destruction of the spur, the intestine should be emptied by purgatives before proceeding further with the operation. The intestine must then be dissected away from the external aperture to a sufficient depth for the easy application of the sutures, the direction of the muscular fibres being carefully followed, their hypertrophy rendering this comparatively easy. By means of the sutures, applied as directed by Gély, of Nantes, the edges of the circular aperture of the intestine become inverted, and the bleeding external surfaces brought into contact rapidly adhere. Finally, the whole is covered in with a pediculated flap dissected from the skin above the preternatural anus.

REVIEW X.

Von dem Mangel, der Verkümmerung und Verdopplung der Gebärmutter; von der Nachempfängniss, und der Ueberwanderung des Eies. Von ADOLPH KUSSMAUL, Professor der Medicin in Heidelberg. Mit 58 Holzschnitten.—*Würzburg*, 1859. pp. 384.

On the Uterus, Absent, Malformed, and Double. Also on Superfœtation, and Migration of Ova. By Dr. KUSSMAUL.

DR. KUSSMAUL's attention having been directed to the subject of tubular pregnancy, he found certain points requiring more exact elucidation than had hitherto been bestowed upon them. In the attainment of this object, it became necessary that the whole range of the literature of extra-uterine pregnancy should be searched, and as many preparations of this accident, as possible, should be closely examined. One result of Dr. Kussmaul's labours has been the discovery that the majority of the recorded instances of pregnancy of the Fallopian tube have been erroneously so-called, having been, in truth, cases of pregnancy occurring in a rudimentary horn of the *uterus Unicornis.*

The mass of information collected by Dr. Kussmaul, upon this and the kindred subjects indicated on the title-page of the work, comprises facts and reasonings to which we would endeavour to do justice, both with reference to our readers and to the author, by submitting the following abstract, brief and incomplete as it must necessarily be, under our inevitable limitations of space.

As preliminary to a comprehension of the characters of uterine malformations, the author insists upon the study of the development of this organ in the human fœtus, and its condition in the lower mammalia. These steps or phases of development are traced fully in detail, from Rathkè and others, by the author, with copious woodcut illustrations. We cannot, however, better condense this part of the subject than has been done by Dr. Sharpey,* from the same sources and his own investigations:

"In the female embryo the common genito-urinary passage becomes divided below into an anterior part, *pars urinaria,* which receives the ureters, and ultimately forms the neck of the bladder and the *urethra;* and a posterior part, *pars genitalis,* or proper genital passage, which receives the Fallopian tubes, and represents the commencing *vagina.* The urethra and vagina both open into a still common part or *vestibule* of the genito-urinary passage. The Fallopian tubes coalesce at their lower ends so as to form a single median cavity, and thus give rise to the *uterus,* or at least to the upper part of that organ, for some observers describe the lower part and cervix as being formed by the upper end of the genital passage, or by a protrusion from it; the lower part of that passage, according to this view, becoming the vagina. For some time the uterus in the human subject continues to be bifid or two-horned, as in many quadrupeds, but after the end of the third month the angle between the orifices of the Fallopian tubes begins to be effaced, and the *fundus* is subsequently completed. Sometimes the bifid uterus continues through life."

* Quain's Anatomy, vol. iii. p. 411. 1856.

The non-absorption of the walls of the two primordial tubes in contact leaves a longitudinal septum, constituting separate uteri, with their vaginæ, a condition nearly parallel to the normal state in the *marsupialia*. In the placental mammalia there is often presented a trace of the lateral coalescence in the form of a narrow vertical separation in the virgin vagina. The division of the uterus into two lateral halves is seen in some rodentia. The increase of the fundus, by gradual obliteration of the cornua, may be followed through the herbivora, carnivora, and quadrumana, up to the human form :—

"But even in the lower quadrumana, the uterus is somewhat cleft at its summit, and the 'angles' into which the oviducts enter form a considerable part of the whole organ. As we ascend through the quadrumanous series towards man, we find the 'body' increasing, and the angles diminishing in proportion, until the original division is completely lost sight of, except in the slight dilatation of the cavity at the points at which the Fallopian tubes enter it."*

Having sketched the morphological history of the uterus, Dr. Kussmaul proceeds to its malformations, which he divides into eight groups.

1. A large group, in which the uterus is either wanting, or so stunted during the early months of fœtal life, that it is reduced to a mere trace of muscular fibre and areolar tissue. The rudimentary horns, extending towards the Fallopian tubes, give an outline figure of the letter Y. This arrest, occurring about the fourth or fifth month, may affect the growth of part only of the uterus, while the remainder of the organ may present either a single or double type ; thus the two horns, or the body alone, may be found, the cervix being absent, or inversely, the lower segment only existing, the organ reverts to the type of tardigrade animals.

The *uterus unicornis* is another form of the first group, one only of the primordial canals being developed in various degrees of perfection, while the other is dwarfed or stunted, giving the variety designated *uterus unicornis cum rudimento alterius*—a variety on which we shall have occasion more particularly to dwell in a later page of the present article.

The independence of growth of the two halves derived from the primary canals is strikingly exhibited in the combination of the imperfect and stunted rudiment of a horn, with the completely formed *uterus unicornis* capable of pregnancy, as well as in the double uterus resulting from the failure in fusion of the genito-urinary canals. Many degrees of these two conditions are met with.

In the second group the author places complete duplication of the uterus—i.e., its development as a double organ without any diminution of bulk on either side ; the circumference and actual substance of both uteri being equal to, or exceeding that of, one normal single uterus. The vagina sometimes single, sometimes double. The arrest of growth has simply occurred at the point of union of the primordial canals—representing the condition in mammalia. Dr. Kussmaul

* Carpenter's Principles of Comparative Physiology, fourth edition, p. 617.

describes three modifications of this doubling of the uterus:—*a.* The uteri entirely distinct, and so far apart as to allow folds of intestine to fall between them—as in monotremata and marsupialia. *b.* The uteri more or less near to a common cervix, divided from each other below by a septum, which also may or may not extend downwards, so as to form a single or a double vagina—the type of ruminantia, pachydermata, and rodentia. *c.* The uteri not separated as in the preceding variety, but united throughout their length, and having their cavities divided by a longitudinal septum—the *uterus bilocularis* of Rokitansky.* In this variety the outer form of the organs does not differ from that of a single uterus, except in being rather broader and shorter.

All conceivable modifications of these varieties may be met with, giving rise, according as the fundus appears more or less divided, to the denominations, "*uterus duplex bicornis*," "*uterus septus*," "*uterus subseptus*." These malformations probably date from the second month, as the genito-urinary canals have been found disunited before that period.

The third group embraces those deviations of form which have originated in disturbance of the process of development during the latest months of fœtal life or infancy, or even towards the period of puberty, whence results the "*uterus fœtalis*," or "*infantalis*."

Dr. Kussmaul constitutes a fourth group of uteri, in which the cavity is totally or partially wanting, "*atresia totalis* and *partialis*." This defect is generally associated with other malformations dating backward to the early months of pregnancy. With the body of the uterus otherwise well formed, there is sometimes found congenital deficiency or occlusion of the cervix and os uteri. *Atresia uteri* is most frequently associated with more or less complete *atresia vaginæ*, the latter is even in some instances replaced by an imperforate rounded cord or perforate to a small extent at its upper or uterine end.

In his fifth group, Dr. Kussmaul arranges the oblique form of uterine deviation originating in inequality of development, or from the agency of adhesions caused by peritonitis.

In the remaining groups are placed the instances of congenital malposition of the uterus; of fusion of the uterus with adjacent organs—e.g., the intestines or urinary organs; and of premature or precocious growth of the uterus.

The third chapter of this work treats of the entire want of the uterus in woman, a deficiency rarely met with, and not certainly discoverable in the living woman by any means of investigation which we possess. Many of the reputed cases of absent uterus, Dr. Kussmaul considers to have been instances of rudimentary conditions of the organs, and points out that the place of insertion of the round ligament serves to distinguish the enlarged Fallopian tube from a rudimentary or malformed uterine horn. Absence of the uterus does not necessarily exclude existence of the ovaries, which may be regularly

* See Pathological Anatomy, vol. ii., translated by Dr. Sieveking for the Sydenham Society.

developed and enclose Graafian vesicles, although it is more frequently observed that in such cases these organs have been also wanting. The Fallopian tubes in like manner are sometimes wanting, sometimes present. In all undoubted cases hitherto recorded, the vagina has either been found entirely wanting, or ending in a blind sac. The external organs may be either normal or malformed. The breasts have been seen fully formed when the uterus has been absent, and the general conformation of the body of a distinctly feminine type. Menstruation and the menstrual *molimina* have expressly been stated to have been absent even where the ovaries have contained Graafian vesicles.

The next rudimentary form of uterus, of which Dr. Kussmaul treats, is that designated by Rokitansky and other German pathologists, the *uterus bipartitus*, a defect originating during the first four or five months of fœtal life, and presenting, like other malformations, various degrees, from the mere solid muscular substance, to the thin membranous cavity.

The author in the two following chapters notices the infantile or fœtal uterus in which it is found that arrest of development has taken place during the later months of fœtal life; and the *uterus duplex* or *diadelphys*, which generally occurs with other congenital malformations of the body. As already remarked, the most important portion of this work is that which is devoted to the exposition of the characters and relations of the *uterus unicornis*. In the next ten chapters we have an elaborate array of cases and woodcut illustrations; it is here that Dr. Kussmaul has discovered the errors above alluded to with reference to the supposed cases of tubular pregnancy, and has hence been led to attempt an entire revision of the literature of this and allied topics.

The position of the one-horned uterus in the pelvis is always oblique, directed towards that side whence it springs, its degree of obliquity depending, however, on the presence of a rudimentary horn, and the height at which the latter is attached; the higher this arises from the more developed horn, the less the latter deviates from the middle line. The obliquity of direction necessarily gives the body of the uterus a convex and a concave side, the former gives attachment to the "super-horn." The body, or more perfectly developed horn, approaches in size to the virgin uterus; it tapers to a point, its walls gradually thinning to their termination in the Fallopian tube. The stunted half uterus, or "super-horn," presents various degrees of development; thus, this may exist as a thin band of muscular fibre, extending outwards and upwards, and merging in a round ligament; or it may be a smooth, rounded, muscular, imperforate cord; or it may, having the same form, present a cavity prolonged into a Fallopian tube. The unicorn uterus may be an independent malformation, or it may be conjoined with malformation of the urinary apparatus, and of other organs. As a general rule, the rudimentary horn opens by a canal into the lower or cervical portion of the developed horn. In one case it opened into a dilatation of the vagina.

The uterus unicornis, with rudimentary horn, is equally adapted for, and prone to pregnancy, as the same variety without the rudimentary horn. The most important practical fact is, that the rudimentary horn may contain the fœtus. Dr. Kussmaul has collected the histories of thirteen cases of pregnancy in this form of double uterus, constituting of themselves, apart from all other matter in this work, a most interesting body of pathological information. Respecting one case, he tells us that Dionis has recorded of Maria Theresa, the queen of Louis XIV. :—"Jam curiosa autem ipsa fuit regina ut satis diu consideraret istum uterum. Post meridiem Reginæ nomine mihi mandatum fuit, ut uterum istum denuo ipsi adferrem." We leave it to dealers in psychological curiosities to speculate upon the effect of Queen Victoria sending a like message to Sir Charles Locock.

Menstruation in the majority of these cases was normally present. No impediment to pregnancy seems to be offered by this one-sided development of the uterus. Pregnancy has been found to have existed in a rudimentary horn, without canal of communication with the fully-developed horn. It is improbable that such canal should not have been present ; the canal had doubtless become obliterated as a consequence of the changes in the deciduous lining of the canal, the seminal fluid having in the first place found its way from the developed horn to the ovum discharged from the ovary in connexion with the rudimentary horn.

The *uterus unicornis*, simply, is not specially prone to abort, while with the rudimentary horn it is precisely the reverse. The fœtus has most commonly been expelled into the abdominal cavity by a fissure of the horn, and death has ensued from hæmorrhage or peritonitis; or in some more fortunate cases the fœtus has become indurated or even ossified, until, from the accession of inflammation and suppuration, a fatal result has ensued. The fissure through which the ovum has been expelled has always been found near the origin of the Fallopian tube, or that point at which the walls have been thinned. In all these cases the developed horn has presented thickening of its walls and a deciduous lining of its inner surface.

The unicorn uterus presents no essential difficulty in labour, nor danger subsequently ; indeed, it is known that the subjects thereof have been the mothers of many children. Twin and multiple births from an organ the subject of this malformation, have been recorded; the possibility even of simultaneous pregnancy in the two uteri cannot be disputed. Since pregnancy can take place in the rudimentary horn, there is still less reason why it should not simultaneously occur in the more fully developed half of the organ.

The difficult physiological and medico-legal problem of superfœtation is brought under notice in the second division of the work. In order to attain clear views on this subject, Dr. Kussmaul, with obvious correctness, observes, that it is primarily requisite to distinguish between superfecundation and superfœtation. At the same time the author expresses a very decided preference for the "good old term," super-pregnancy (*nachempfangniss*), as embracing both ideas. By superfœtation is understood a subsequent conception during the existence of a previous pregnancy, or within the first menstrual period of pregnancy. The author gives the following illustration :—A woman in whom conception has taken place is simply pregnant, but she may never bring forth a fœtus, inasmuch as a fecundated ovum is not a fœtus; supposing that she should conceive again within a short period, she is superfecundated or superpregnant, but this does not constitute superfœtation. More than one ovum may be detached by the same fruitful coitus. "It is self-evident," says Dr. Kussmaul, "that ova may be successively disengaged within the same ovulation or menstrual period; yet," he adds, "we do not speak of this occurrence as an instance of superfœtation, but of simple multiple conception." Superfœtation, then, the author holds, must be the result of the fecundation of separate ova within different menstrual periods.

The question, however, to which, for practical purposes, it is important to obtain an answer, is—Is superfœtation, in the strict meaning of the word, possible? The fact has usually been denied, as for the single uterus, and admitted for the double organ. The turning point of the inquiry is, whether ova may be ripened and detached during a second or later menstrual period of pregnancy, and whether the condition of pregnancy offers an absolute hindrance to the fecundation of such ova. Dr. Kussmaul urges, that the supposed proofs of such fecundation are fallacious—viz., the presence of Graafian vesicles filled with coagula, as observed in some post-mortem examinations; the recurrence of menstruation during pregnancy; the disposition to abortion at menstrual periods. Little reliance, observes the author, is to be placed upon such evidence, since in one case referred to in an earlier page of his work, the menstrual "molimina" had occurred, although the ovaries were found in their fœtal condition, and in another instance wherein the same phenomena were alleged to have existed, yet after death testicles instead of ovaries were found. With reference to the proclivity towards abortion at menstrual periods, Dr. Kussmaul observes, that until it has been proved that the number of miscarriages is actually greater at those epochs than at any other during pregnancy, no legitimate conclusion can thence be drawn. Sanguineous discharges from the vagina, moreover, are not always menstrual. Still less proof is derivable from the presence of several corpora lutea; not only may two or three be formed when the ova are simultaneously matured in several vesicles, but they may also exhibit various appearances of age, since, while their development is the more active at the time of pregnancy, so is their retrogression the more tardy. The discovery of freshly ruptured Graafian vesicles in the ovaries of women pregnant, or recently delivered, would furnish strong proof to the doctrines of superfœtation; but the observations of the most trustworthy and distinguished anatomists are in direct contradiction thereof. Thus, Kiwisch, out of several hundred post-mortem examinations of the bodies of women recently delivered, found no recent cicatrices in the ovaries. Virchow arrived at a similar result from abundant opportunities during the fatal epidemic at the Berlin Lying-in Hospital. Hecker also, without denying the possibility of the occurrence, emphatically asserts his belief that the doctrine is founded in error, and that the reverse of a rule is established. Dr. Kussmaul's own investigations upon forty cases of extra-uterine pregnancy lead to the opposite conclusion. Superfœtation of a false or spurious character has occurred in cases where a previous extra-uterine pregnancy has taken place, and the results of conception have remained as a morbid product within the abdomen.

The physical condition of the uterus during pregnancy presents no absolute bar to superfecundation. It is now no longer held that the uterus is hermetically sealed by a mucous plug immediately upon the existence of pregnancy. Dr. Kussmaul contends that the presence of the decidua offers no insuperable hindrance to the possibility of superfecundation; the only effectual obstruction to the admission of semen into an impregnated uterus is the ovum itself, when it fills the uterine cavity, and causes occlusion of the orifices of the Fallopian tubes. The author concludes that the condition of pregnancy offers no real hindrance to a second conception within the first two or three months. Dr. Kussmaul holds, however, that in all cases of recorded superfœtation there has been a simultaneous multiple conception, followed by the death, or arrested growth, of one or more—while the other fœtus has progressed in development—and have been expelled at different periods; and in support of this view has quoted a long list of cases.

Migration of Ova.—Under this title (*Ueberwanderung des Eies*) the author, in his concluding section of his work, has collected examples in the human being of a curious phenomenon observed by Bischoff in lower animals. In December, 1857, Dr. Kussmaul, examining the body of a female who had died from the consequences of rupture of a tubular pregnancy, found that the corpus luteum was situated in the left ovary, while the ovum was contained in the tube of the right side. This occurrence induced more extended research, and the author found numerous instances of this transfer of the ovum from the one to the opposite side, due doubtless to the muscular contractions of the Fallopian tube, and the ciliary movements of the uterine mucous surface. A hitherto unnoticed dynamical cause of tubular pregnancy was thus demonstrated by dissection, and an additional light thrown upon an obscure phenomenon.

To say that the author of this work has exhibited therein an untiring industry is merely to repeat the compliment which has been so often paid to German authors as to have become threadbare. We cannot, however, dismiss the volume without urging upon those for whom the topics it discusses may possess special interest, not to remain content with the brief and imperfect abstract here laid before them, but to consult the volume for themselves; they will find therein a great mass of well-digested and valuable information, for which otherwise they must search, as the author has himself done, through an almost unlimited extent of periodical and other literature.

Review XI.

On Dislocations and Fractures. By Joseph Maclise, F.R.C.S. Fasciculi V. to IX.—*London,* 1859. Imperial Folio.

Impressed with the all-importance of a sound practical knowledge of the anatomy of particular regions of the body in the discrimination and treatment of dislocated and of fractured bones, we have deemed it expedient to present our readers with a notice of the concluding half of this work, the first half of which we reviewed on a former occasion.* We do so firmly persuaded that from a careful perusal of its pages, illustrated as they are with excellent lithographic illustrations, much useful information will be gained by the practitioner respecting a class of cases of frequent occurrence, and occasionally obscure and perplexing in their nature.

In Fasciculus V. we find an elaborate account of the structural relations of the elbow-joint. The author observes, that the supinators are but comparatively weak opponents, and hence it is that by their mere tonicity they render pronation the natural position at ease—a circumstance which should be borne in mind when setting fractures of the radius.

> "When the radius is fractured in its shaft, it is the pronator teres muscle which, acting on the lower or upper fragment, retracts this, and obliterates the natural and mechanically necessary curve of the bone. The other muscles, which arise from the inner tubercle, and are inserted into various parts of the hand, are flexors of this member especially, but they can also act as flexors of the fore-arm, and those which approach more closely to its radial side may aid the pronator teres; in all these capacities they require to be opposed by artificial extending means when the bones are fractured, else they will cause deformity as to the set of the fragments, and also shortening."

The author points out clearly the significance of the pronator quadratus muscle. He justly combats the view taken by Cruveilhier, of regarding the inter-osseous ligament as a mere aponeurosis, inasmuch as the radio-ulnar layer of inter-osseous fibres supports the radius by the ulna; while the ulno-radial layer of fibres, with the annular ligament, supports the ulna by the radius. The radius and ulna, when not disunited from each other by fracture, or by rupture of their connecting ligaments, can only be luxated, the author states, from the humerus in three principal ways—viz., either backwards, outwards, or inwards. The olecranon projects so far behind the joint, as effectually to prevent the forward dislocation while the fore-arm is extended, or whatever be its position. Now, we would remind the author, that the olecranon becomes removed from the posterior depression of the humerus the more the elbow is bent. Hence, dislocation of the ulna forwards may occur without fracture of the olecranon, when the forcibly bent fore-arm is impelled upwards and inwards by a blow or a fall on the elbow. There are certainly but

* British and Foreign Medico-Chirurgical Review, vol. xxii. p. 113.

few instances of this luxation occurring independently of fracture on record. In the case described by Colson (cited by Hyrtl, vol. ii. p. 296), the determining cause was a fall upon the half-bent elbow.

The anatomical relations of the dislocations of both bones are carefully given. It deserves notice, that in each simple luxation of the radius, whether forwards or backwards, the fore-arm is in a state of pronation, because, owing to the displacement of the radius on the undisturbed ulna, the pronator quadratus, which goes across from one bone to the other, is put on the stretch.

After the reduction of either of the simple dislocations of the radius and ulna, Mr. Maclise advises that care should be taken to maintain the bones in position, according to the particular ligaments which have been ruptured.

"In those luxations in which the annular ligament is not broken, the ulna, when reduced, will serve to retain the radius in its place. But if that ligament has been broken, then the head of the radius has no other stay, for the external lateral ligament attached to the annular is useless, and the slightest motion is enough to displace the shallow cotyloid facet of the radius from the condyle backwards or forwards, besides which the radius becomes wholly submitted to the traction of the biceps muscle. As the semi-flexed position of the fore-arm relaxes the biceps, and as the semi-prone position relaxes the pronators, and is the natural one assumed by the limb when at ease, the part should be set accordingly."

The author alludes to fracture when accompanied with dislocation of the bones of the fore-arm. He then proceeds to fracture of the olecranon, and recommends the treatment generally resorted to. "The fore-arm is to be fully extended and kept in that position by a splint applied in front of the joint, and so secured that the bandage will at the same time serve as much as possible to draw the olecranon to the shaft of the ulna." It is an interesting fact that the olecranon has given way through vehement contraction of the triceps muscle.

Fracture of the coronoid process of the ulna, when accompanying a dislocation of that bone backwards—an accident of serious consequence in regard to the mechanism of the joint—is next noticed. The treatment suggested is an apparatus which will maintain extension of the fore-arm from a fixed part of the arm at the axilla. It is somewhat singular that M. Malgaigne, in his elaborate volume on fractures, has overlooked this accident.

The author, in reference to fracture of the condyle alone, states that it is an accident of much more frequent occurrence than the fracture of the trochlea alone, and in all the cases which he has examined or read of, he has not found one of the latter kind. We are of the same opinion as to the rarity of this variety of fracture, notwithstanding the conjoint authority of Desault, Sir C. Bell, and Sir A. Cooper. M. Malgaigne never witnessed an instance of it. Under the head of treatment our author appropriately enjoins the semi-flexed position of the fore-arm, immobility being observed for twenty-five or thirty days—allowing some passive motion, however, after the twentieth day, so as to prevent trouble from articular stiffening. Fracture of one or other of the tubercles—an accident which, though not affecting the mechanism of the joint, yet renders a certain class of muscles incapable of performing the motions of the fore-arm—is treated by adopting a semi-prone and semi-flexed position of the fore-arm, which relaxes those muscles that arise from the inner tubercle, and allows of its being kept in its proper relative situation.

The form and mechanism of the wrist-joint and the hand, fractures of the radius and ulna, dislocations and fractures of the hand, next engage attention. With regard to the vexed question, as to whether the hand can be dislocated without fracture of the bones, the author asserts that to the anatomist either kind of accident may as readily occur in respect to the wrist-joint as in respect to any other. He then proceeds to show how the hand may suffer actual dislocation without the radio-carpal joint being at all implicated. In such case the luxation must pertain to the radio-ulnar joint alone, examples of which have been noticed, although misnamed dislocation of the ulna.

"The styloid end of this bone, naturally more or less prominent, projects to a remarkable degree in such an accident, but this appearance must be owing to a dislocation of the radius and hand from connexion with the ulna, and as a consequence of a rupture of the triangular fibro-cartilage, which, owing to its stronger attachments to the radius, follows that bone. The hand joined to the radius, which is its stock, is now evidently to be regarded as having suffered lateral dislocation from the ulna in respect to the radio-ulnar articulation."

We consider the above reasoning both ingenious and conclusive. It follows of course that the opposite form of this injury—namely, a dislocation of the hand with the ulna from the radius—cannot be deemed possible to occur.

With respect to the differential diagnosis between fracture and dislocation, which is often puzzling in this situation, much information may be gained by carefully studying the position of the styloid process of the radius in reference to the root of the hand. In the instance of fracture of the radius, the styloid process does not alter its position referably to the root of the hand, but it does so unavoidably in the instance of dislocation.

The author maintains, and we believe with reason,—

"That the true dislocation—that is the *disarticulation*—seldom or never co-exists with the fracture of a principal member of any joint, and for the reason that the fracture, when present, is in all instances the first effect of the violence, and then the force, wholly influencing the fragments, is incapable of disuniting the joint, of which the broken bone forms a part."

We concur with the author as to the principle on which the mechanical treatment of fracture of the bones of the fore-arm may be best conducted, and which reqnires that the appliances should serve the ends of extension in opposition to both the flexor and extensor muscles, so as to counteract shortening, and at the same time to sunder the bones at the place of fracture. The author reprobates the practice of binding the limb tightly by a bandage, a proceeding which can only promote the effect which should be avoided. Much mischief is often produced by this procedure. We recollect a case where gangrene supervened, and where amputation was performed, in which the result was fatal, caused by tight bandaging of a broken fore-arm.

"In order to relax the muscles" (directs the author), "set the limb first in a semi-flexed and semi-prone position, and then applying a pad (formed to correspond with the inter-osseous space) to the front and another to the back of the fore-arm, lay on each of these a splint of the suitable form—namely, an anterior one, bent at the upper end so as to be made to bear against the humerus and to reach to the palm, and a posterior one reaching from the olecranon to the back of the hand. Both these splints should project their margins so as to be half an inch at least beyond the outline of the bones, and for this reason—viz., that the bandage may bear against those margins instead of against the bones. When those splints are adjusted, extension should next be made, and while that effort is maintained, the bandage should be applied so as (first) to fix the hand as the point of extension, and (last) the brachial part of the anterior splint as the point of counter-extension."

An ingenious apparatus for the above purpose was lately submitted to the Academy of Medicine of Paris. It is composed of two splints connected together by means of screw-bolts furnished with nuts, which may be tightened at discretion. It possesses the advantage of preserving the two splints firmly on the same plane, and maintaining a compression perpendicular to the surface of the fore-arm.

It is a curious circumstance that the radius has been broken by simple muscular action, independently of external violence. A case is mentioned by Van Niérop,* of a woman, aged thirty years, who, while wringing out some wet clothes, sustained a fracture of the lower end of the radius.

In the next section are discussed the form and mechanism of the pelvic apparatus and of the coxo-femoral joint, with dislocations and fractures of the femur. After some interesting details on the homologies of the pelvis, the author points out the relative position and special action of each of the muscles in connexion with the hip-joint. In describing the capsule, he says we find it thrown into bands, especially at the inner side of the neck of the femur, and these directly unite this part of the bone to the fibrous membrane.

"I note these bands especially, for in the cases of fracture of the neck of the femur *within* the capsule, they are not generally ruptured, and when they are not, they serve still to attach the head of the bone vascularly to the trochanteric base, and thereby render it not altogether impossible for osseous union to take place."

A full account is given of the different dislocations of the femur. With regard to reduction, we find Mr. Maclise saying that the direction in which it is to be accomplished is in all cases indicated by the attitude assumed by the thigh.

"This attitude declares the nature of the accident, the relative position of the parts concerned, and the direction of the lacerated track in which the bone has been displaced, and consequently that in no other direction than backwards through that track can it be made to resume its natural position."

In respect to intra-capsular fractures of the neck of the femur, the author seems disposed to adhere to the ancient school, still repre-

* Gazette des Hôpitaux, No. 224.

sented by Boyer, which holds that the want of union in these fractures depends on want of contact and upon insufficient apparatus.

The form and mechanism of the femoro-tibial articulation, with fractures of the femur, dislocations and fractures of the tibia, the fibula, and the patella, follow in succession. In the concluding fasciculus the ankle-joint and foot pass under review, together with fractures of the tibia and fibula, dislocations and fractures; and some closing remarks on the law of articular development, false joint, and anchylosis.

The brief analysis which we have presented to our readers of Mr. Maclise's valuable and elaborate work is scarcely commensurate with its importance. In taking leave of him, we would thank the author, who here again is his own most correct and artistic illustrator, for a contribution to surgery which cannot fail to enhance and secure his reputation. Nor would it be right to say as much as we have done concerning the work before us, without adding that the moderate price at which it is published places it within the reach of even slender means.

Review XII.

The Causation and Prevention of Disease. By John Parkin, M.D., late Medical Inspector for Cholera in the West Indies.—*London,* 1859. 8vo, pp. 197.

In this treatise Dr. Parkin assails, with great vehemence, the principles and practice of the Board of Health, and ridicules the notion that animal or vegetable putrefaction, or the crowding of human beings in too confined a space, can be the cause of epidemic or endemic diseases. In opposition to these prevalent opinions, he advocates that which regards a miasm, or malaria, arising from causes operating below the surface of the earth, as the true source of the evils in question.

Dr. Parkin has, we think, made a strong case against his adversaries as far as epidemics are concerned; but he seems to us to have run into utter extravagance in the opposite direction, by maintaining that the presence of putrefying matters, and the crowding together of human beings, are preventive of epidemics, and, if we understand him aright, actually conducive to health.

That accumulations of filth, and emanations arising from decomposing animal or vegetable substances, are the ordinary causes of epidemic disease, whether *per se*, or as affording a medium for the development of pestilential germs, is, we conceive, disproved by all impartial observation; at the same time it would be rash, in the present very imperfect state of our knowledge, to deny that they may occasionally operate in this manner; and there are indeed many cases on record which can scarcely be reconciled with any other supposition; but however this may be, there is clearly no ground for the assumption that such causes are not detrimental to health, or that they may not increase the severity or promote the extension of epidemic diseases when once formed. Under ordinary circumstances, it is, we think, probable that the amount of evil arising from such causes depends very much on the constitution and habits of those exposed to their influence. Persons of what may be called a coarse organization, and little nervous susceptibility, may become so habituated to such influences as to be little injured and even scarcely annoyed by them, while others of a more sensitive constitution will suffer more or less in their general health from a state of things highly disgusting to the senses, and suggestive of all sorts of depressing mental associations. For example: many nightmen enjoy very good health; but if a person of refined mind and habits were compelled to follow this vocation, his health would probably suffer severely, though there would be little likelihood of his becoming the subject of typhus or malignant cholera, unless those diseases were already prevalent from other causes than filth and abominable odours. It should be remembered also that, apart from the more formidable and widely-spread epidemics, many instances have been recorded in which morbific poisons, peculiar in their effects and more limited in their range of operation, have appeared to arise from the effluvia of decomposing organic matter; indeed, there are probably few practitioners of much experience who could not adduce some such cases from their own observation.

Dr. Parkin thinks it

"Easy to show, by direct proof, that the products of putrefaction, whether animal or vegetable, do not and cannot cause either fever, or cholera, or any other general disease." (p. 26.)

"These products are carbonic acid, nitrogen, sulphuretted hydrogen, carburetted hydrogen, phosphuretted hydrogen, and ammonia. With respect to carbonic acid, nitrogen, and carburetted hydrogen, they are not deleterious but simply irrespirable; and as regards sulphuretted hydrogen, though it be, no doubt, a direct poison both with men and animals, and more particularly with the lower classes of animals, there is abundant evidence to prove that it is not productive of disease in the ordinary signification of the term. Neither can the morbid effects be due to the joint operation of two or more of these gases, for, as the nature of the organic matter varies greatly in different localities, the products of its decomposition must also vary, one gas being present in one case and absent in another, the proportion of those present differing also under different circumstances; no compound therefore could be formed sufficiently uniform to account for the phenomena of epidemic and endemic diseases, which present the same pathognomonic characters at all times and in all localities." (pp. 26-31.)

But we submit, that although this reasoning may be sufficient to prove that none of the sensible or known products of putrefaction are productive of disease, it by no means proves that there may not be other products which elude our senses, and of which we have no cognizance, but which may nevertheless be productive of disease. Organic poisons defy analytical chemistry. What can chemistry make of the poisons of the most venomous serpents? Or what can it detect in them but the ordinary constituents of animal matter? Can chemistry tell us why the poison of a body recently dead is more dangerous, when introduced into the circulation, than that of one in an advanced stage of putrefaction? If Dr. Parkin were asked to point out what gas or other substance it is that communicates the deadly power to his favourite volcanic miasm, would he be able to do so? Is he not obliged to confess that his malaria is an agent of the chemical composition and properties of which he knows nothing? If so, why should he deny that putrefaction may be attended with the evolution of a morbific agent equally inscrutable? We do not urge this as advocating the "organic" against the "volcanic" hypothesis; our object is merely to show that Dr. Parkin's reasoning is inconclusive, and weighs as much against his own view of the subject as against that which he is endeavouring to overthrow.

With respect to overcrowding, we can go so far with Dr. Parkin as to admit that there is no evidence of any pestilential disease having been traced originally to this sole cause; but when he affirms that it has no influence on the type or on the propagation of such diseases, we differ from him as widely as possible. Dr. Parkin endeavours to show that densely-peopled districts, so far from being more obnoxious to epidemics, are less so than those which have fewer inhabitants. To prove this position would, we think, require more evidence than our author has adduced; but supposing the case to be as he imagines, the real point at issue remains untouched. A place may, indeed, be densely peopled, yet not to a degree incompatible with a good state of the public health—look at London, for instance. But what should be understood by over-crowding, in a hygienic sense, is the huddling together of a number of human beings in a very confined space, so that the air shall be more or less perceptibly vitiated by their respiration and excretions; and all experience goes to prove that such circumstances, though they may not be instrumental in generating any particular morbific poison, are highly favourable to the spread of epidemic diseases when once introduced, and have a marked influence on their phenomena, almost invariably inclining them to an adynamic type, and tending remarkably to the development and activity of the contagious principle, wherever such principle is in any degree present.

In his views of the origin and properties of malaria, Dr. Parkin follows those who regard it as originating beneath the surface of the earth, from causes which he designates as *volcanic*. He lays down certain laws, six in number, which regulate the extrication of malaria from the surface, and its diffusion through the atmosphere. These laws are very nearly the same with those enforced by Dr. Aiton and other writers, and agree very much with the inferences of M. Regaud de Lisle. With his third law, that "Malaria is never extricated from the surface of water, under any condition whatever, as long as the particles of the latter fluid hold together," Dr. Parkin connects a train of observation and reasoning which is well worthy of attention. It has hitherto been a universal opinion that malaria is absorbed by water, and is lost by passing over even a small body of that fluid. This opinion he considers as entirely erroneous; holding, on the contrary, that malaria is not absorbed by water, except in the smallest possible quantity. His reasons for this conclusion are, in brief, the following. Water can only absorb a given quantity of any substance, and if the substance absorbed be gaseous, the excess is extricated as soon as the point of saturation is attained; and this effect is hastened by an increase of temperature. But malaria is not extricated from small bodies of water more than from large ones, either in temperate or in the hottest climates. If the most pestiferous marsh in the torrid zone be covered with water only an inch deep, the extrication of malaria will be as effectually prevented as if there were a dozen feet or fathoms of water. This could not be the case if the poison were absorbed by the water, which, when saturated, would give out the excess. On the other hand, malaria is extricated from the margins of large lakes and rivers, as well as from those of rivulets and stagnant pools. Let but the smallest portion of the muddy bank of a lake or river become exposed, and if the atmospheric temperature be sufficiently high, malaria will be evolved, however great the body of water with which it was previously in contact. But this could not be if the operation of Law 3 depended on the attraction of water for malaria, as the latter, instead of remaining in the soil, to be subsequently let loose into the air, would have been absorbed by the water, especially where the volume of water is great, and when, as in temperate climates, the quantity of the poison evolved is comparatively small. (pp. 89–90.)

Dr. Parkin explains in another way the apparent absorption of malaria by water; but for this explanation we must refer to pp. 91–2 of his work.

The great practical question of the prevention of epidemics will necessarily be viewed in a very different light by the advocates of the organic and volcanic hypotheses. If the former be true, good sewerage and free ventilation are the indispensable requisites; if the latter, sewerage, though it may be useful and desirable in some points of view, can never be a means of preventing epidemics; and in many cases may have an opposite tendency, by affording channels through which the morbific miasm may escape into the atmosphere. Supposing the volcanic hypothesis to be the true one, the great means of prevention will obviously consist in covering the surface of the earth with some impermeable substance in all localities where pernicious exhalations are believed to arise. In towns this is effected chiefly by the paving of streets, courts, alleys, and underground apartments—such as kitchens and cellars; and paving with flag-stones will evidently be more effectual than with loose paving-stones, because the former oppose a more continuous surface to the escape of terrestrial exhalations. Ordinary roads, and still more, macadamised roads, whether in town or country, will to a certain extent be productive of similar results. When a town is situated near the embouchure, or on the bank, of a river, particularly where the river flows through the heart of the town, the embankment of the river will be attended with the most beneficial results; but to render this efficacious, the surface must be covered to low-water mark, since it is during the receding of the tide, and from the sides and bottom of the river, that the miasmata arise. In swampy districts in the country the most effective method of preventing the extrication of malaria is to cover the surface with a sheet of water;

the quantity of water being immaterial, provided only that the surface be entirely covered. The plan of filling up a swamp, or covering it with earth and stones, will be dependent for its degree of success on the nature of the materials used, their solidity, and the greater or less porosity of the soil; but this plan can never be so efficacious as the former.

Among the conclusions arrived at by Dr. Parkin, there is one which, if proved, would be of infinite importance—namely, that relating to an agent possessed not merely of an antiseptic or deodorizing, but of real disinfectant, properties, capable of neutralizing the poison called malaria, and acting as an antidote and a remedy for its effects—viz., carbon, or its compounds, carbonic acid and charcoal. Dr. Parkin's views on this subject have been repeatedly brought before the profession, we need not now therefore dwell upon them.

Much further inquiry, and inquiry conducted in a much more impartial spirit than hitherto, will be required before any valid conclusions can be arrived at respecting the origin and prevention of epidemic and endemic disease.

Dr. Parkin's book is not written in a manner altogether suited to a scientific subject. He is very angry with the Board of Health, because they recommended the non-publication of a report on epidemic cholera presented by him to the Secretary of State for the Colonies, on his return from the West Indies in 1855. This seems to have engendered on the part of Dr. Parkin a degree of petulance and flippancy of style, and a disposition to make out his opponents not only wrong, but ridiculous. No scientific work should be written in such a spirit.

Notwithstanding all its faults, we can recommend Dr. Parkin's treatise as containing a vast deal of interesting information on the important subjects of which it treats, much acute reasoning and able illustration, especially in regard to the supposed origin of malaria beneath the surface of the earth, and some useful suggestions respecting the means of promoting the public health.

Review XIII.

1. *Traité Spécial d'Hygiène des Familles, particulièrement dans ses rapports avec le Mariage, au physique et au moral, et les Maladies Héréditaires.* Par le Docteur Francis Devay, Professeur de Clinique, Interne à l'École de Médecine de Lyon, &c. Deuxième Édition.—*Paris*, 1858. pp. 765.

A Treatise on Domestic Hygiene, especially in its relations to Marriage, Physical and Moral Development, and Hereditary Diseases. By Dr. Francis Devay.

2. *L'Amour.* Par J. Michelet. Quatrième Édition.—*Paris*, 1859. pp. 464.

Love. By J. Michelet.

Within a few months of the meeting of an Association for the Promotion of Social Science, it seems almost unnecessary to insist on the fact, that there is a large class of questions interesting as objects of purely scientific inquiry, important for their close relation to the wants of daily life, and of fair promise as methods of ameliorating the condition of our fellow men, which ordinarily receive less than their proper degree of attention. Some such conviction as this was in the minds of the eminent men who organized the assembly to which we have alluded. And although strictures more or less just have emanated from many quarters against the formal discussion of such topics in a national congress, they cannot apply to every method for bringing about the same end. There is undoubtedly much to be gained at the present day, and in the present state of science, by an attempt to reduce to rule and law the heterogeneous facts, the uncertain generalizations, and the somewhat technical divisions which now form the bulk of our information on many points of social physics. Much has been done during the last half century by the erection of political economy into a special study, and the still more recent tendency to investigate what may be called social physiology will probably be productive of no less valuable results.

For the increase of population and the multiplying of the diverse units constituting the great total of a State has so far confused the speculation even of the acutest observers, and so far isolated classes and orders of society one from another, that the conception of man himself has become more individual, more self-contained, and less consciously bound up in the various relations which compose his ordinary intercourse with his fellows, than it was in earlier and simpler times. By the very multiplicity of the phases of modern life, by the minute subdivision of labour and of rank, by the close adjustment necessary for mutual support and subsistence, the existence of a great mutual interdependence has got to be somewhat lost sight of, and the man is brought under our notice either as the animal of the physiologist, the intellectualism of the metaphysician, or the volitional atom of the statist, obeying general laws in spite of personal freedom of action. We hear little of man in the present day as the citizen, looking upward with the vivid realism of Plato and Aristotle to his mother-state; or of an omnipresent ideal commonwealth, endowed with active and living existence, which was so clearly obvious to the Greek mind. Indeed, this self-forgetting tendency of all largely-developed States seems to have been foreseen by Aristotle, when in his 'Politics' he says, "*οὔτε ἐκ δέκα ἀνδρων γένοιτο ἂν πόλις, οὔτε ἐκ δέκα μυριάδων.*" As ten individuals would only insufficiently represent the subordinated discipline of a Greek State, so one hundred thousand would be too numerous for its practical working. Strange as such an assertion appears in these days of myriads, and of single towns with a population exceeding the largest ancient polity, still, it cannot be denied that the difficulty really has occurred, as prophesied by Aristotle—the first and the greatest of political economists. For such bodies are, in his words, not *ἐυσυνόπτοι*, and the characteristic relations of men's personalities are lost in the complex mechanism of a highly artificial intercourse. There has been a similar and perhaps consequent forgetfulness manifested latterly of some of those relations by which the family is built up and elevated to its proper rank as the lesser emblem and type, of which, as of many factors, the ideal commonwealth is composed. There is no branch of inquirers to whom a return towards older and more philosophical views in this matter may be more valuable than to the thoughtful and scientific medical practitioner: compelled as he is to hold an intermediate station between physiological dogmas, on the one side, and the needs of daily life on the other; to interpret the axioms of abstract thought and observation into simple precepts of recommendation and avoidance, he must be perpetually met by the necessity for contemplating his patients as links in a social chain, held fast in either direction by bonds of attachment to the various sections of which human society is composed.

Hence we cannot hesitate to receive with satisfaction works like that of M. Devay, of which a second edition has recently made its appearance. For in it there is a full and fair discussion of that long series of problems which occupy the midway position between the facts of physiology and the most sacred mysteries of social life. Belonging to medicine by one of its closest affinities, it reaches far and wide into the examination of topics under the domain of hygiene, morals, and religion. Indeed, the consciousness of this fact, and the earnest desire of the author to extend the bounds of medical science into those border-grounds and "debateable lands" of investigation, forms one of the great merits of the book. The expression of this sentiment in the preface deserves quotation. Speaking of the little faith manifested in medicine by the majority of persons, the author proceeds to say,

"We have already stated elsewhere what we now repeat, there is a fault which causes as much serious injury in our profession as ignorance itself—namely, scepticism and systematic doubt of the value and bearing of the art of medicine. We allude here especially to the weakening of belief in medicine in the opinion of the public, that public which reverences no deities, gives way to no illusions, and watches with malicious interest the conflicts of our vain theories, oscillating with equal indifference in either direction. This evil is among us, and is at its height; it makes us regret a time now far distant, when the physician was encompassed with dignity; a time when the family welcomed him with pleasure, tempered with deference, and when his presence alone offered the firstfruits of recovery. The medical man was then accepted, not indeed as an oracle, but as a man marked out by a special character, which there was no gainsaying. And does not the defect arise, in great measure, from the little influence which medical men exercise on the family? Does not their mission almost terminate in our times with the superintendence of accidents, and of illness actually in progress? The power of physic is brought to bear after the evil has burst forth, never as a means of prevention. The medical man, summoned, like the handicraftsman or the public official, at the moment of a catastrophe, treats more patients than he in reality cures; because to cure the evil he must fathom it, and he requires to fathom it a confidence which is usually refused him. Generally he is ignorant of the pathological history of the family to which he is sent for, and lacks that guiding clue without which it is impossible to bring the treatment of chronic disease to a good result. On one side and the other medicine is thus too lightly treated. In return, the man of science, considered in some sort as a stipendiary, soon contracts a corresponding tone of feeling—namely, indifference and coolness.

When his business is done he departs, without giving the family the advantage of useful advice, arising out of the emergency, and on matters which he has observed perfectly, but on which he has not been questioned. Thus, in presence of mutual suspicion, the internal hygiene of the family is neglected, the most serious diseases silently spring up, and the medical man, who should be a daily adviser, has only a limited and occasional function."

It is undeniable that there is much force and cogency in these observations, and that the position of the medical attendant in some well-ordered households, even of our country, is anomalous, not in every way parallel to the correlative functions of the legal and spiritual advisers. Both of these stand often on a greater footing of intimacy and equality, carry a greater social weight, and exercise a more generally protective influence over the affairs of their several departments. And we agree with M. Devay in thinking that the remedy for a state of things unsatisfactory to both parties involved in it lies partly with ourselves; "le médecin doit s'elever aussi haut que son sacerdoce, et mieux prouver encore ce qu'il sait et ce qu'il peut."

The way in which he proceeds to carry out this view is best told in his own words—

"In pursuance of these convictions, my object has been to penetrate into some delicate questions which general treatises on hygiene have committed the error of omitting, and which by an imperceptible though constant action tell on the organism, as does dropping water on the rock. In studying hereditary disease, that never-ceasing source of misery in families, I have induced some to regard marriage more seriously in its sanitary point of view; this is one of the novel features of my work, and I venture to say, one of the highest importance."

It is not our intention to give any detailed analysis of this work, or indeed to do much more than call attention to it as a valuable essay on a class of subjects comparatively unhandled among ourselves, and deserving of fuller consideration. The second division of the book, which treats of what is termed l'hygiène de l'espèce, and principally of marriage, exhibits this in the highest degree. It commences by noticing the almost total neglect and forgetfulness shown at the present day in this matter, to all considerations but those of a social and pecuniary nature; then follow a series of chapters deserving attention as an abstract both of the historical and practical aspects of the question, establishing its principles on a sound physiological basis. Commencing by the discussion of divorce, precocious and tardy unions, special causes of incompatibility, the influence of celibacy and marriage on the length of life, and many other important though delicate inquiries, the author proceeds to consider the great topic of hereditary disease and congenital peculiarities. In this portion of the work, though we are not perhaps prepared to go to the full extent of his advice, or to give so authoritative and judicial a weight in our social life to deductions from dogmas of hygiene, as would be borne and appreciated under other governments and different organizations; still, much may be gained from a perusal of this essay, and more occasion given for individual reflection. While, on the one hand, we fully estimate the importance of the subjects themselves, and the necessity for their being brought under the notice of our profession; on the other, we cannot but commend the healthy and frequently religious tone, as well as the philosophic spirit, in which the discussion is maintained.

In turning from the consideration of a book conceived in a commendable spirit, and containing the results of much thought and knowledge on these obscure but important subjects, to another of very different character, and of which no such favourable opinion can be given, we are aware of a probable accusation of bigotry and narrow-mindedness. M. Michelet's book, "L'Amour," has, we understand, been well received in many quarters; is looked upon as inculcating valuable principles, and has been even held up as an efficient antagonist to the revolting tendencies of the modern school of sentimental romance, more especially as it is represented in France. If we are led to give a very different verdict, there are doubtless some who will find no difficulty in referring this opinion to a spirit of clique and professional jealousy. It may be well, therefore, to put in the very head and front of our accusations, the one which is most assailable on these grounds. M. Michelet's book is defective in our eyes because written by one who, although standing high in the general literary world, is in no sense a medical man or a physiologist. We have already shown our appreciation of the class of subjects to which this belongs, and which lie on the border-ground of science. These, we have already stated, are apt to be neglected by those to whom they more properly belong. There is no distinct reason why they should not be equally well undertaken, and with as satisfactory results, by men not formally bred up or devoted to the service of physic. But the present attempt to invade these domains is marked by the very faults and inconsistencies which are to be feared in such cases.

And first, there runs through the whole book a suppressed and semi-conscious vein of pruriency, which, though it never reaches any distinct expression approaching to obscenity, savours all the remarks with an insufferably nauseous effect. It is probable that this will not strike non-professional readers, for the intention of the work is moral throughout; but it is all the more dangerous for this very reason, and it reflects very accurately the excitable half-frightened attention which the mass of mankind gives to details of the grosser and more recondite parts of our bodily organization, unless previously disciplined to that habitual coldness and familiarity which is the characteristic of the experienced medical practitioner. There is, moreover, constantly a consciousness in the tone of the narrative, that it is touching on delicate subjects, and, as it were, treading on forbidden ground. Contrast this with the fearless outspoken philosophic ἦθος of any good medical book, even where the points treated of are of the most disgusting nature, and the difference will be instantly visible.

Indeed, carefully as it is concealed under bold figures of language, the ground taken up in Chap. iii., p. 24, is little more than a laboured excuse for writing such a book at all; and at the end of the same section we learn, *è converso*, from the denial of the author, that it has struck him in the middle of his attempt that all is wanting which constitutes the vital reality of a scientific work. "*Ce n'est pas un roman*" amounts to little more than an admission of the possibility of mistake, and an implicit confession of resemblance to such a production. On this point we must venture to reverse the author's own judgment of his production. The work is a romance, dull indeed, and abstract in character, but the legitimate offshoot and caricature of the idealizing tendency of modern prose fiction. The decrease of *dramatis personæ* in such works has been steady, the depiction of feelings and of hidden mental states more minute and accurate in every successive effort; it was reserved for M. Michelet to produce the last success of art by altogether cancelling *dramatis personæ* in their individual form, and concentrating the reader's interest on a minute realistic study of the erotic passion in a phase which the presupposition of marriage may indeed make civilly decorous, but fails utterly to render dignified or intellectually elevating.

A more serious deficiency of the production lies in the want of real physiological knowledge for such an undertaking. Of this the writer seems to be himself in a partial manner conscious, for he devotes a special paragraph to a statement of his personal fitness for the self-imposed task, and after describing how he became at one period of his life a public confidante, to whom strangers of all sorts entrusted their desires, griefs, aspirations, and disappointments, particularly such as were of a delicate character, he adds:

"These youthful spirits, which laid themselves open in full confidence and transparency, revealed much to me, and furnished unconsciously a considerable part of the immense store of facts from which this book, little by little, originated. But nothing assisted me more than the friendship of those to whom there are no secrets — I mean medical men. I have enjoyed the intimacy of several among the most illustrious of this century; for ten years I was the friend—nay, I may venture to say the brother—of an eminent physiologist, who retained in natural science an exquisite appreciation of moral truths. Much I learned in his company on many subjects, but in especial on that of love."

Accepting the author's own exposition of the means at his disposal for acquiring information beyond such as can be obtained by reading books of an unfamiliar class, we can scarcely avoid concluding them to be insufficient. It would seem that the temper of mind necessary to the philosophical consideration of subjects which do not strictly belong to the high road of physiological research, but which involve a mixed estimation of theoretical and practical truth, is the very last and most difficult to be reached. It needs that settled intimacy with the scientific bearings of questions which can come only with deep study and long experience; it needs compulsion by circumstances to contemplate the facts from many diverse points of view, and a keen appreciation of the exact height to which social and practical considerations may be allowed to rise without trenching on the province of more essential principles. Such a supposition of insufficient knowledge is confirmed by internal evidence; not only are physiological facts scattered very sparingly through a great mass of imaginative writing, but they are in some cases actually mis-stated. An instance of this occurs very early in the work, in the following passage of the introduction:—

> "Every century has its prominent and characteristic disease. The thirteenth was the age of the leprosy, the fourteenth of the 'black death,' the sixteenth of syphilis, the nineteenth is smitten at the two poles of nervous life in imagination and in love—in the man, of enervated brain, vacillating and paralytic; in the woman, with torturing ulceration of the womb; this century will be named the age of uterine diseases—in other words, of misery, abandonment, and despair among women."

Now, it may be there is some truth in the statement that the present age will rank in the after history of medicine as the period of uterine disease, but it will instantly occur to every man conversant with the medical facts that this is far more owing to the greater investigation and more accurate means of diagnosis brought to bear on the subject, than to any demonstrable increase in the amount of uterine disease. Indeed, we trace in the Greek writers distinct proof to the contrary, for there are few, if any, of the modern ailments which have not their counterpart—sometimes, it is true, very roughly sketched, but still capable of recognition. If we add to this fact the remarkable decrease of population now going on in France, which is a different medical point, and has other bearings, we have probably fathomed the whole depth of a loose and erroneous generalization, which would never have received the sanction of one who was really reproducing the results of mature study and defensible views of uterine pathology.

A still more glaring instance of this perversion of facts to suit the purpose of the writer, occurs somewhat further in the book, commencing by an excited statement of a recent embryologic discovery.

> "Facts derived from another source are beginning to indicate that the relation of love which man treats so lightly is for the woman in a far higher degree profound and critical. She gives herself away in her whole nature, and without redress. The phenomenon observed in females of the lower orders of animals, reappears less regularly, but still does reappear in woman. Fecundation transforms her in a permanent manner, and the widow often bears to her second husband children resembling the first. This is grand and terrible. The conclusion is overpowering for the heart of man."

From this he proceeds to weave a tissue of imaginative rhetoric which totally alters and perverts the bearing of the original text, and although no specific error can easily be fixed upon, still the unprejudiced reader can hardly fail to see that the result is erroneous, and that the premises are far exceeded by the conclusion.

But perhaps the most amusing specimen of this confusion occurs in the pages immediately preceding that from which we have just quoted. In them there is a kind of parody of a pathological examination of the subject. We are told with solemnity of the value of death, as clearing up these questions, especially in the form of suicide. The number of females exposed at the Morgue is stated for one year at fifty; this number is multiplied by ten to produce a more startling result; the time of the year is given in which such suicides are most common, and we are led to expect some practical deductions from the figures; but nothing of the sort follows: a loose contrast between summer weather and a deserted mistress winds up the paragraph agreeably, and so the section terminates. The real end has probably been attained, in creating a general impression that statistics of mortality have been contributing their share to the train of reasoning, that something has been proved as by pathological induction; and so we pass on to more of the same strain of vague declamatory composition.

It would, however, be unfair to insist long on faults of detail in a work of this character, if the general conception were healthy and the tone commendable, but this we fear is not the case. Springing out of a state of civilization full of prejudices, and accepting as facts all the erroneous views which fashion and sentimentalism dominant for many years have elevated into laws, it works out an artificial system of conjugal life, which has all the sensualism of the lowest libertinage, heightened by the deliberate prostitution of the imaginative faculties; for even if it were possible to grant that a view of love so materialist and physical as runs through the book were the highest ideal which can be reached, we should none the less complain of the want of simplicity and the artificial character of the enjoyment which it proposes. In this it is essentially the work of a Parisian mind, and does not rise one jot above the flux of novels perpetually streaming from that fountain of spurious sentiment and over-excited passions.*

From the description of the model wife in the first chapter, which is headed "La Femme est Malade," and which goes on to chronicle and deify with a nauseous admiration the whole course of the phases of illness in a dysmenorrhœic woman, we are called on to look upon the pale, nervous, over-stimulated Parisian beauty as the type of woman in her highest sphere and conception. Feeble as is the author's pathological acumen when he deliberately attempts its cultivation, he becomes closely and painfully descriptive when he only wishes to be pathetic. Page after page might be transcribed into any treatise specially devoted to the "morborum omne genus" which attend on the susceptible female frame when exposed to every influence, mental and bodily, which can injure its constitution. So far from being what the author pretends, a regeneration of the legitimate sexual relations which have confessedly fallen into such disrepute throughout France, and a rehabilitation of marriage on a sounder and more healthy footing, we believe this to be the very worst, because the most cool and philosophical, instance of demoralization which has issued from that country; it dilates with piquancy of language and a brilliancy of style almost pyrotechnic (for which M. Michelet must undoubtedly have credit), on the most dangerous symptoms of the malady—the poison is subtlest where least

* A singular confirmation of the views here expressed has been furnished by M. Michelet himself. Encouraged doubtless by the success of his former production in the field of sentimental physiology, he has just followed it up by a further work, under the similar title of 'La Femme.' We are not yet in a position to make any detailed criticism of its merits and blemishes, but we find it described as full of "obscene propriety," and as "too disgusting both in matter and treatment for us to do more than allude to it."—Saturday Review, Dec. 3rd, 1859.

detected, and most carefully concealed; in a word, it belongs to that moral order in which evil and falsehood is perversely elevated as an object of worship—a state of mind which Bacon has well named "the last of evils, the apotheosis of error."

But much as there is in the book of M. Michelet from which we are obliged entirely to dissent, and objectionable as we cannot but consider the tone in which it is conceived, there is no need to shut our eyes to a real and important problem which underlies it. It is beyond doubt that the social condition of woman, and the healthy adjustment of the various complications which spring out of the matrimonial relation, though a simple matter in a primitive society, becomes in a larger and more developed commonwealth in the highest degree intricate. It is difficult if regarded in the abstract form of the metaphysician, as one of the points at which mind and body begin, as it were, to flow in the same channel. Man has from the animal side of his nature sexual inclinations as purely physical as digestion or muscular action; but he has also, from the intellectual side, the power of elevating and refining these propensities until the product is far different and more noble. Either of these two opposed forces may preponderate, and at different epochs in history have swayed the balance. Any student can point to periods in which a license and immodesty of manners utterly inconceivable to minds of the present day, was accepted as the usual order of things; we may instance the time of Louis XV. in France, or Charles II. in England. And it needs no more search to discover periods like the later days of Louis XIV., or the time of the Puritans among ourselves, when formal profession of strictness and prudery, elevated into a fashion, have been the outward marks of a reaction from sensualism, and have attempted to refine our compound nature into a purism too unconditional for the needs of ordinary life. A like unhealthy horror of realities, often unpleasant, indeed, but which form an essential part of our individuality, seems chronically epidemic in America, and is far from being absent even in the easier and more natural tone of our present English society.

Not less difficult is the question if viewed with the comprehensive glance of the political economist. We may pass over the extreme cases and the most perplexing form of the problem, which exhibits a class of females, unsexed as it were, for the benefit of society at large, despised and held at arm's length by their own equals, disowned even by their relatives, hardly appreciably benefited by philanthropy, and yet as permanent and necessary an element of the safety of society as pestilences or standing armies.* Short of this, and within the bounds

* We are probably by no means the only persons to regret the cessation from premature death of the excellent series of inquiries on this most perplexing question, whose results appeared some years back in this periodical. They showed an amount of patient investigation, and a steady purpose of obtaining true results amid the innumerable errors besetting the subject, which are absent to our judgment in most similar researches, with the exception perhaps of the work of Parent-Duchâtelet. The loose and sentimental newspaper discussions of the past year are but a sorry alternative for Dr. Holland's labours. We would instance the whole topic as one of those in which the enlightened medical man who is anxious to do something beyond the routine of his profession, and to vindicate the social value of his art, might find a congenial field of occupation. It would come well within the province of one of the newly-created officers of health.

of social life, there remains great inequality, much room for difference of opinion. On one side, the followers of Malthus would regard as a crime what religion holds faultless; would make the citizen responsible to the State, not for the greatness of his family, as he was in olden times, but for its due suppression to the capacities of the period, to his own powers of sustaining it, and to the relative density of population. We feel that such artificial limits are of man's making, and have no warrant either in physiology or revelation; and yet the opposite alternative is worse, and must have often painfully affected the medical man whose duties have led him into the poorer districts of great towns, where early marriages, contracted almost between children, coexist with a variable demand for labour, and a trade whose products depend for their sale on fashion, or some other temporary and evanescent exigency.

And again, we are bound to give the writer credit for protesting against some of the older and more ingrained fallacies on this subject. He inveighs with much vehemence against the monastic doctrine of the impurity innate in woman, and of the consequent virtue of celibacy. In England this seems of little force, and a struggle with a shadow; but probably in Roman Catholic countries the case is different, where a party will always be found who will attempt to enforce this view, and in some cases with more or less success. Whatever, therefore, without immorality, tends to enfeeble a dangerous perversion of truth, in so far deserves our approbation.

But besides this negative and otiose merit, the book inculcates a domesticity and satisfaction in the pleasures of family intercourse, which is less common on the Continent than with us. In this there is a tacit protest against the practical polygamy and the need of esoteric excitement which make some foreign nations recal so irresistibly the society of Ancient Athens or Rome. Whether we visit some of the great European cities or follow the vivid pictures of a Platonic dialogue, nothing strikes the observer more painfully than the absence of that humanized tone which is the proper product of the blended sexes. In the latter instance we are able to watch the interlocutors through their day's work, from the morning gymnasium to the political intrigue, or philosophical discussion of the afternoon, and to the evening meal, followed by light games of dexterity and chance, or by the looser recreations of the wine-cup, the song, and the recitation. We hear of games and theatres, of battles and military services, which, not unlike those of the present day, passed with the bulk of the citizens rather for recreation than the severer labours of men in earnest; everywhere we find this unsettled kind of life, with its hatred of retirement and its ignorance of the more recondite pleasures of the family. And yet we have evidence that these men could rise to speculations of nobler import. In the 'Symposium' of Plato, to which our allusions more especially refer, there is a remarkable passage which deserves notice, not merely for the doctrines which it holds out, and which are very apposite to the present subject, but also for the fact that the main interlocutor is a physician. A double interest results from this; for if we suppose, as seems the most reasonable conjecture, that these narrative dialogues are not pure fictions, but rather events substantially authentic, adorned and illustrated by the genius and fancy of the narrator, we have not only the opinions of a cultivated Athenian on topics the especial study of classical times, but we also gain some insight into the social position of our brethren in the Greek metropolis, and the kind of scientific conversation which would pass current as a fair representation of their skill and doctrines. At the banquet, of which we have a detailed account, Socrates and his friends start the subject of Love for discussion. After some preliminary matter, Pausanias draws the distinction which has since become so famous, and from which, probably, our modern phrase of "Platonic affection" originally springs. He says, it is impossible to discuss love without separating the two kinds—the Uranian and Pandemic, the heavenly or intellectual, and the vulgar or bodily love. In the nature of things neither of these can be called good, neither bad; for both are implanted in us, and exist as physical facts; but the one leads to honourable, the other to base results; the latter is common to all animals, the former exercises the mind and sets in motion the finer portions of our nature. Here Pausanias ends, and Aristophanes should by turn take up the conversation, but from some accidental haste in drinking, he is seized with a violent suffocating cough; upon this he turns to Eryximachus the physician, who is next him at the table, and challenges him either to stop his fit of coughing, or to speak in his stead. Eryximachus replies he will do both, and before commencing to speak, prescribes for his suffering neighbour. First he tells him to hold his breath as long as he can, which will very likely stop the paroxysm, if it should not, he is to gargle his throat with water; lastly, if this should prove ineffectual, he is to irritate the nostrils with some small body till he sneezes once or twice, and however violent the cough may have been, this will put a stop to it.

During the carrying out of these instructions, which, by the way, have lost none of their value at the present day, and which we strongly recommend to any of our readers who may be similarly afflicted, Eryximachus follows in the line which the preceding speaker has begun, and says, that this double nature of love is not only discoverable in men's minds and bodies, but in all created nature. He then enters into a somewhat imaginative illustration from the medical fact of morbid longings after unwholesome articles of food, giving, in the course of it, a definition of medicine, which is not only original but also of some value: *ἔστι γὰρ ἰατρική, ὡς ἐν κεφαλαίῳ εἰπεῖν, ἐπιστήμη τῶν τôυ σώματος ἐρωτικῶν πρὸς πλησμονὴν καὶ κένωσιν.* If this be rendered to define medicine as "the science of the longings of our bodies towards support and evacuation," it is not easy to deny its truth or competence. Allusion is then made to gymnastics, diet, and the admixture of heat and cold in the various seasons of the year, and their effect on the bodily frame. Through these, which would now be termed questions of hygiene, he passes on to sum up the medical view of love in very remarkable words, which are not only far in advance of the conceptions of M. Michelet, but really present a fair statement of the philosophical question in its widest bearings:

"So great and powerful, and indeed so omnipotent, is love in all its forms; but that which aims at a noble end, subject to the restraints of justice and temperance, has the greatest power of all. It furnishes us with every form of happiness by binding us together in the closest relations of social union among ourselves, and in proper dependence on the gods, our rightful superiors."

The contrast between this view and that of the modern French writer is remarkable. Open as Plato's dialogue is to serious objections, we cannot help being of opinion that its tone is healthier and its tendency more elevating than anything in M. Michelet's work. For recognising, as both do, the innate and physical character of the basis on which this and our other emotions are ultimately founded, the heathen writer can see through this and other obstacles an upward and ennobling path to the society of our loftier fellow-creatures, and to communion with the gods; while an author in the nineteenth century of Christianity, not only never seems conscious of the religious aspect of the question, but is perpetually engaged about the most servile ideas, and is constantly falling to the very level which Plato looks down upon—namely, that of the Pandemic, or vulgar corporeal love. Nay, so great is this contrast, that we cannot refrain from quoting, in conclusion, a farther passage, in which Socrates takes up and follows out the previous idea:

"When," he says, "a man, through the exercise of an honourable love, rises to the perception of what is intrinsically good, he is near his goal. For the right course in pursuit of the pleasures of love is to start from visible beauty and its possessors, in a frame of mind constantly ascending step by step, as though with help of ladders, from one fair person to many, and from many to all that exist; and from fair features to noble acts, and from noble acts to honourable studies, until we reach that first of studies—the knowledge of the real and abstract good. If we thus nurture true virtue in our minds, it is in our power to become adorers of the gods, and above all other men to aim at immortality."

REVIEW XIV.

1. *Notes on the Wounded from the Mutiny in India: with a Description of the Preparations of Gun-shot Injuries contained in the Museum at Fort Pitt.* By GEORGE WILLIAMSON, M.D., Staff-Surgeon.—*London*, 1859. pp. 124.

2. *A Treatise upon Penetrating Wounds of the Chest.* By PATRICK FRASER, M.D., &c. &c. &c.—*London*, 1859. pp. 140.

DR. WILLIAMSON's handsome little volume, which is illustrated by some excellent representations of the preparations described in it, is a reprint from the 'Dublin Quarterly Journal.' It is an excellent example of a class of books which is increasing at the present day with a rapidity which, while it promises the greatest advantages to science, must increase most materially the labours of those by whom that science is to be promoted. This class consists of books which are mainly intended to be referred to by writers who may have in hand a special subject, and comprises catalogues of museums, collections of cases, statistics of practice, and all such-like things.

These useful books are, unhappily for their authors, quite unreadable, notwithstanding the interest of much of their contents. Yet what stores of facts are comprised in them, and what an advantage the present generation of medical writers ought to have over our ancestors, whose crude judgments were necessarily founded on individual experience and on vague impressions, which the more extended records of after times have shown to be often baseless. In order to call the attention of our readers to one of the most interesting collections of material for the description and treatment of gun-shot injuries in existence, we will select a few of the miscellaneous items of which Dr. Williamson's book consists.

To commence with the first page—here is a very important suggestion for remedying one of the most universally admitted defects in our army—the means of transporting the wounded. Speaking of the

> "Large number of cases of gun-shot compound fracture of the femur, in the Indian mutiny, where the patients recovered with good useful limbs, as compared with the number of thigh-stump cases, and the total by all wounds;" Dr. Williamson says, "this very satisfactory feature in the classified return of invalided wounded by the mutiny, appears to me perhaps not uncommon for Indian wars, but certainly very much so for European wars, as far as records enable us to make the comparison. This difference in favour of results by Indian wars I believe to be mainly due to the facilities afforded by the dooley for the successful treatment of this severest of all forms of compound fracture."—*Preface.*

Suggestions are given as to the means necessary for securing an adequate supply of doolies, and their bearers, from India; and a plate (p. 123) representing a dooley (a portable chamber, enclosed in tarpaulin curtains, large enough to carry a man reclining at full length, furnished with cushions and mattrass, and carried on a pole), accompanied by the necessary details as to number of bearers, &c.

There can be no doubt of the immense advantages in the treatment of certain cases from such a contrivance. The obvious objection to it is the great number of men required, "eight bearers to each dooley, with the usual number of sirdars, mate bearers, and mussalchees," says our author, in all between a dozen and a score, we suppose, for the service of each dooley, accommodating, if we understand the description, at most two patients. Still they would, of course, be reserved for special cases, and if the bearers could be procured, would, we doubt not, amply repay their expenses. The expense, indeed, would be a mere nothing, amidst all the lavish waste of a time of war, and as the scheme has answered in India, according to the concurrent testimony of many competent authorities, there is no reason why it should not be tried when next we are so unfortunate as to require it in Europe.

We cannot follow Dr. Williamson through all the subjects which his collection of cases and specimens illustrates; and we may remark in passing, that he has thrown unnecessary difficulties in the way of persons wishing to do so, by omitting index, table of contents, and headings to pages; but we will select two classes of cases—viz., resections of joints and bones, and gun-shot wounds of the thorax, on the latter of which his work may be compared with that of Dr. Fraser.

The following passages contain the principal information which Dr. Williamson's work conveys on the subject of resection of each of the larger joints.

"*Hip.*—Excision of the hip-joint for gun-shot injury has been performed eleven times. Of these, but one recovered, that of a soldier wounded by a shell at Sebastopol, and operated upon by Dr. O'Leary. The patient was twenty-five years of age; the head, neck, and trochanter of the femur were removed.

"Of the 11 cases recorded, 6 occurred in the Crimean war, 1 occurred in the Schleswig-Holstein war, 1 by Dr. Ross, 1 by Oppenheim, 1 by M. Seutin, and 1 by Schwartz. In the Crimean war, excision of the head of the femur was performed six times, and all but one were primary operations. One of the patients survived the operation and recovered — viz., Private Thomas M'Kenna, 68th Regiment. On his arrival at Chatham, the limb is reported to have been about two and a-half inches shorter than the other, and capable of bearing some considerable portion of the weight of the body. He could swing it and advance it, but the knee could not be bent. Rotation was admitted to a very limited extent, but performed with considerable pain. The wound was soundly healed.

"So far as the Crimean war goes, it clearly proves the superiority of excision of the head of the femur over amputation at the hip-joint. . . . No doubt in any future campaigns, excision of the hip-joint will be much more frequently employed, and great attention paid to the selection of cases." (p. 83.)

"*Knee.*—As yet our experience of excision of the knee-joint in cases of gun-shot fracture is not extensive, and the means necessary for after treatment in military practice are not encouraging; but the success which has followed it in cases of disease of the joint, makes military surgeons also wish to extend it to the field. The absolute rest and quiet after the operation, which are so difficult to obtain with an army in the field, is the chief and only objection to its adoption.

"There are two cases recorded of excision of this joint for gun-shot injuries, one in the Schleswig-Holstein war, and the other in the Crimean war. Both died." (p. 85.)

On this head we will merely observe, that the results of more extensive experience seem likely to diminish our estimate of the comparative success of this operation over amputation of the thigh; and that it seems very doubtful whether it is worth while to endeavour to practice it in the field itself. Exceptional cases may of course occur where the patient is wounded in the immediate neighbourhood of a stationary hospital, provided with the necessary appliances and situated in good air, but without such essential requisites we should shrink from an experiment in which, we fear, lives would be uselessly sacrificed.

When we turn to the resections on the upper extremity, there is no longer any doubt of the propriety of the operation, and the success of our military surgeons in this, as in every other department of operative surgery, seems to have been unexampled during the Crimean and Indian campaigns, especially the former, when their services were requited by such a storm of unmerited and ignorant obloquy. On the subject of resection of the shoulder, Dr. Williamson says—

"Only one case of resection of this joint was admitted from India; but there is another case where a secondary operation was performed at Fort Pitt, in a patient who is returned as a wound of the joint.

"In the Crimean war the head of the humerus was removed twice as a primary operation during the first period of the war, or that ending March, 1855, and eight times during the second. One of the two first-mentioned ended in death, and of the 8 subsequent operations only 1 proved fatal. The head of the bone was five times removed as a secondary operation, without a single casualty. In addition to these, there was a case in which the head of the bone, and a large portion of the scapula, broken into fragments, were removed.

"Out of the total number, then, of 16 cases, 3 deaths took place, or 18·9 per cent. Had this operation not been resorted to, amputation at the shoulder-joint, it is believed, would have become necessary in all." (p. 102.)

We have quoted Dr. Williamson's statement as it stands, but the reader will observe that he does not account for more than two deaths; and if this is the real number, it would make the return even more favourable to the operation.

Of resection of the elbow, the return stands thus:

"In the Crimean war, 22 operations in all were done on the elbow-joint, of which 3 ended fatally, and 2 more deaths took place after secondary amputation—in all a total of 5 deaths, or 22 per cent. of the cases treated. This percentage slightly exceeds that of resection of the shoulder-joint, but in both instances resection afforded a much more favourable result as to the mortality than amputation." (p. 108.)

The subject of resection is further illustrated by the recital of a most extraordinary and most gratifying case under Dr. Williamson's own care, in which he removed the *whole* of the ulna, the head and neck of the radius, and two inches of the end of the humerus, on account of disease. The wound united by the first intention, and at the time at which the account was written (four months after the operation), the patient "could bend his fore-arm, raise his hand behind his head, and lift a twenty-eight pound weight from the ground; he could also pronate and supinate the hand; there was no anchylosis of the wrist-joint, and he could use his fingers well." (p. 108.)

Let us now turn to gun-shot wounds of the chest; and as on this subject Dr. Fraser's work claims precedence as a regular "treatise," we will endeavour to lay its arguments before our readers, and see how far his conclusions bear the test of the facts which Dr. Williamson has produced.

Dr. Fraser's treatise was originally written, as he informs us in the preface, for the 'Transactions of the Medico-Chirurgical Society,' and its publication furnishes a sufficient justification to the Council for its non-appearance in their volume. Without denying the merits which portions of it possess, and wishing to give all due credit to Dr. Fraser as a zealous and honest inquirer after truth, we cannot see how it would have been possible for such a society as the Medico-Chirurgical to have sanctioned by their authority a treatise so hastily written that the author himself refutes his own principal conclusion, and which is besides rather controversial than demonstrative.

Dr. Fraser's object in this treatise (and it is a very rational and worthy object) is to show that the diagnosis of a wound of the lung in gun-shot injuries to the chest is frequently arrived at rather hastily, and that no one of the ordinary signs can be exclusively relied on as proving that diagnosis. The signs enumerated by Dr. Fraser as having been relied on to prove that the lung is implicated in a wound of the chest are dyspnœa, hæmoptysis, emphysema, pneumonia, pleuritis, and tromatopnœa (or the passage of air by the wound); and the method which he adopts is to review each of these symptoms in succession, with a view of proving that it may be absent when the lung is wounded and present when it is not, and that therefore it is of little value as a diagnostic sign. What, we would ask, is the inference? That there are other symptoms of wound of the lung? If so, why are they not set forth in this treatise? That wounds of the lung cannot be diagnosed? That we trust is not Dr. Fraser's meaning; yet it is the conclusion to which a stranger to the subject would most likely be brought on reading the book. Take any ordinary disease, collect its symptoms out of one of the text-books. Is it not usually true of each of the symptoms, that the disease may occur without them, and that they may occur without the disease? Does this destroy their worth as symptoms of the disease? Not to any one who knows that a disease is judged of from the assemblage of its symptoms, not from the individual signs taken separately. We object, therefore, *in limine*, to this method of Dr. Fraser of discussing the individual signs separately, as being a waste of space; and a much more powerful objection, to our mind, to his treatise, is that it is based upon cases either so imperfectly observed or so badly reported, that no conclusion can possibly be drawn from them. Thus, at p. 52 a table is given which is described by the author as "a highly-interesting and valuable table" (*vide* p. 47), but in which the same strange negligence in language and matter prevails which disfigure other parts of this treatise. The collection is headed "A tabular statement of symptoms in thirty cases of lung-wound," yet in many of the cases it is expressly stated that the lungs were not wounded, so that we presume *lung*-wound must be a misprint. In no less than eleven of the cases, no symptoms whatever are mentioned as having occurred, and if we turn to the notes of the same cases which are scattered through the book, we find that many are so loosely reported, that no conclusions can be drawn from them. Thus the first case in the table is quoted on p. 79 as follows:—"John Maher, aged twenty-five, 57th Regiment, was wounded at the unsuccessful attack on the Redan on the 18th of June; ball passed through both lungs; died on the 21st of June." Surely, before conclusions can be safely founded on published cases, we must have them somewhat more carefully reported than this. Several experiments on animals also are recorded, but they do not seem to have been sufficiently carefully devised or sufficiently connected with each other to prove anything except that severe wounds may be inflicted on these poor beasts without any very marked symptoms being observed, and therefore rather savour, to our mind, of cruelty. Nevertheless, some very striking and well-observed cases may be found scattered through the volume, which prove at any rate the danger of trusting too much to any one or two symptoms of lung-wound as pathognomonic, and go far to justify Dr. Fraser's general conclusion. "Although I would not place implicit reliance on any one of the heretofore-accepted signs of lung-wound, if there were three or more of them present, I should consider their concurrence as strong presumptive proofs." (p. 87.) But the author mars the force of his reasoning by curious inconsistencies, which are either due to extreme negligence in writing or to confusion of ideas; thus, in speaking of tromatopnœa, or the passage of air through the wound, he says:

"I am, indeed, of opinion that when the lung is really wounded this tromatopnœa must cease; thus, when a small opening is made into the thoracic cavity without wounding the lung, air will pass freely out and in during respiration; but if the opening be enlarged, and the lung be so wounded that there is a direct communication formed with the opened pleural cavity, the entrance and exit of air will cease, from the simple physical fact that all opposition is removed, and no confined body of air is subjected to the alternate movements of the thorax." (p. 86.)

Yet the author goes on to say, in the next sentence but one, "out of nine fatal cases noticed by the author, in which the lungs *were* wounded, it (viz., tromatopnœa) was present in two." So that it is quite clear that the statement made above is too general, and that if the author wished to give definite value to his researches on this point, he should have endeavoured to appreciate the influence of the size of the external wound and of the rent in the lung, the conditions of the lung in respect to adhesions, &c., upon this symptom, and so to have shown in what circumstances tromatopnœa may be taken as decisive of a wound in the lung, and when, on the other hand, we may hope that the pleura only is opened.

The strangest statement, however, is one which is made in summing up the result of the whole book (a "physiological summary," as Dr. Fraser calls it). In this summary it is broadly laid down:

"That in the human subject, as well as in animals, an actual wound in the substance of the lung is always, sooner or later, mortal; not from the effects of inflammatory action, but, in recent cases, from a sudden cessation of proper aeration in either the whole or portions of one or two lungs, or sudden hæmorrhage." (p. 140.)

In opposition to this, Dr. Fraser himself says, "There are cases in which recovery has taken place when the substance of the lung was wounded" (p. 8); and refers to a case related by Larrey, "in which recovery took place, and there was no doubt that the lung was wounded." (p. 11.) A very interesting case,* and one which would have been very valuable to Dr. Fraser had he met with it before the publication of his treatise, is found at p. 31 of Dr. Williamson's work, with a beautiful representation of the parts, in which the lung had been wounded, but the patient survived eleven months, and then died

* [See also a remarkable case given by Mr. Forde in the Medico-Chirurgical Transactions, vol. xx. p. 378.—Ed.]

of gangrene of the opposite lung, from some unexplained cause, but influenced, as Dr. Williamson supposes, in some manner, by the old wound, which had never soundly healed. The track of the ball was found still open and lined by a membrane, on which the bronchial tubes opened; the exit of the ball had been kept open by necrosed bone. We may notice that here, if anywhere, according to Dr. Fraser's reasoning, tromatopnœa should have been absent, since the wound communicated directly with the air tubes, and consequently there was no resistance to the passage of air by the natural way; yet it is stated distinctly that "air escaped on expiration and coughing."

On a review, then, of the diagnostic portion of Dr. Fraser's work, we would say that he appears to have succeeded in showing that the symptoms usually relied on are more frequently present when the lungs are not wounded, and more frequently absent when they are, than has been generally supposed; but that his treatise adds little to our knowledge of the positive diagnosis of these cases, and that he has been led by haste and argumentative zeal to put his conclusions far more absolutely than his facts warrant.

An interesting point in these injuries of the thorax is the question of the treatment proposed by Mr. Guthrie in cases of wounds of the diaphragm, where that injury can be diagnosed, and the patient is in danger of dying from phrenic hernia—viz., to cut into the belly and draw the viscera out of the thorax, and so by reducing the hernia save the patient's life. Interesting cases may be found on this head in Dr. Fraser (pp. 94 *et seq.*), and in Dr. Williamson's book there is a beautiful engraving (p. 36) of the parts in a case of this kind. We think a study of these cases will convince any dispassionate reader that the suggestion is in the highest degree unpractical and dangerous. Unpractical, because the diagnosis can never be made with certainty, unless in very recent cases, when the accompanying lesions must forbid the thought of any such operation, and when, indeed, Mr. Guthrie himself (see Fraser, p. 98) afterwards allowed that it could not be put in practice; reserving it for secondary cases, where the patient has recovered, with a hole in the diaphragm, through which the abdominal viscera afterwards protrude and become strangulated. Dr. Williamson's cases will show, if any cases were required for the purpose, that this condition can hardly ever be diagnosed with accuracy enough to warrant an operation; and if Mr. Guthrie's idea is correct, that a wound in the diaphragm never heals, to what purpose would it be to submit the patient to so frightful an operation when, if the viscera could be drawn out of the thorax, no means exist for closing the hernial opening, or keeping the viscera from immediately passing through it again?

A very important portion of Dr. Fraser's treatise remains to be noticed—viz., that in which he speaks of the treatment of wounds of the chest. Here he reasons strongly against the necessity of the copious and indiscriminate bloodletting which is, or was lately, prescribed for such injuries; and we must say that the cases he has adduced, together with the notes of those reported by Dr. Williamson (so far as the latter are fully reported), are enough to create some little doubt, at any rate, whether bleeding can be so necessary to the cure of these wounds as some would still have us believe. Many cases may be found here in which wounds of the chest, which would by most people be diagnosed as involving the lung, recovered without bleeding; and others in which the fatal progress and the distressing symptoms seemed little affected by copious bloodletting. Dr. Fraser seems opposed to bleeding altogether; but we have no doubt that his theoretical reason for it—viz., because pneumonia is, as he believes, a rare complication of wound of the lung—is erroneous. There is every reason, however, to believe that the pneumonia which follows a wound of the lung is often, perhaps usually, only as much as is sufficient and necessary for its closure, and that if bleeding would affect it at all, it would be injuriously. On this subject, as on so many others, the truth seems to lie between the extreme partisans on either side, and the symptoms of each case will doubtless furnish the best guide to practice. With the limited experience that we have in London of gun-shot wounds, we cannot affect to give any opinion on this subject; but we can entertain no doubt of the relief which we have seen from *small* and *cautious* venesection in cases of fractured ribs with wound of the lung, and should certainly expect similar results in gun-shot injuries.

Review XV.

A Manual of the Philosophy of Voice and Speech, especially in Relation to the English Language and the Art of Public Speaking. By James Hunt, Ph.D., &c.—*London*, 1859. 8vo. pp. 438.

This book treats of so many branches of knowledge, that a doubt naturally arises as to the competency of any one individual to deal with them all. Such diversified subjects as physiology and the cuneiform characters, music and pathology, typography and therapeutics, hieroglyphics and acoustics, anatomy and oratory, with others not more cognate than these, must be allowed to furnish a range of inquiry rather trying to the abilities and information of the writer who ventures upon it. Dr. Hunt does indeed request the reader to bear in mind,

> "That in a volume like the present, comprehending such a variety of topics which have individually been frequently discussed by the most eminent writers, the amount of original matter cannot be very large; but professing to be merely a manual, it is of much less consequence that the opinions advanced should be *new*, than that they should be *true*."—(Preface.)

But in order to compile with accuracy on any subject, it is indispensable that a writer should himself have a very good general acquaintance with that subject; and hence it happens that those works in which a single author ranges adventurously through too wide a field of research frequently exhibit gross ignorance on several points, and no very satisfactory information on any.

Whatever may be the qualifications of Dr. Hunt for the production of such a work as that before us, we must, on our own part, disclaim all capacity to criticise it in its alarming totality, and we cannot but think ourselves fortunate that a very large proportion of its contents do not fall within our province. We now proceed briefly to notice some of those which do so.

The first chapter contains a sketch of the anatomy of the lungs and the mechanism of respiration, which, though slight, is quite sufficient for the purposes of a work professing to treat of speech.

The second chapter, "On the Nervous System," is extremely defective. For example, in the description of the lateral ventricles of the brain, no mention is made of any of the objects there visible, except the corpora striata, and these are described in the manner of one who has never seen what he is describing. The account of the cerebral nerves is also such as might be expected from a writer who had no practical acquaintance with the subject. The origin of some of them is very imperfectly traced, and that of others altogether unnoticed. Dr. Hunt, it is to be observed, is not a Doctor of Medicine, but of Philosophy; we do not, therefore, blame him for not understanding anatomy, but we do blame him for writing on matters that he does not understand. If he thought these anatomical details necessary to the elucidation of his subject (which, however, we cannot perceive), he should have got some competent person to supply them.

The description of the "organ of hearing" and observations on the corresponding sense, in the fourth chapter, are very general, but embrace, perhaps, all that is required; and the writer, by avoiding unnecessary details, has probably escaped inaccuracies similar to those just noticed.

The fifth, sixth, and seventh chapters, "On the Vocal Apparatus," "The Organs of Articulation," and "The Production of the Voice," are, on the whole, very good. The larynx is well described, and the progress of opinion respecting the action of the vocal ligaments and the formation of the voice is accurately traced; the author finally acquiescing in the view now generally considered as established by the researches of Müller and others—viz., that

> "The human larynx is a kind of reed instrument with a double membranous tongue, and that the sound is the result of the vibration of the vocal ligaments, which are regulated by the same laws as other metallic or elastic tongues." (p. 97.)

The word "other" might as well have been omitted, as the vocal ligaments are not metallic.

The only vocal phenomena which are not yet fully reconciled with this hypothesis are those of the *falsetto*. On this Dr. Hunt has some observations which we believe represent pretty correctly the present state of the case. As all our readers may not be familiar with the subject, we give the passage in a somewhat condensed form.

In accounting for the falsetto, which differs from the chest voice not merely in pitch, but in quality, some have maintained that the vocal cords vibrate only half their length, and that the rima is partly closed. Lehfeldt first observed the fact, that in the production of the falsetto the three borders only of the glottis vibrate. Müller and most other physiologists are of the same opinion. Peterquin and Diday, however, contend that the falsetto notes are *flute* notes, not produced at all by the vibration of the vocal cords, but by the vibration of the air as it passes through the glottis, which thus changes from a reed to a flute-like instrument, while in the production of the chest-voice the whole breadth of the cords is thrown into vibration. Physiologists differ as to whether women possess the falsetto voice, though the flute-like *timbre* of their highest notes evidently presents a similar character. Some consider the falsetto as a defect of voice; this may be the case where the chest-voice has less than the normal compass, but not otherwise. Ségond, who has made various observations by means of the speculum invented by Garcia, is now convinced that the superior ligaments, which he formerly considered as concerned in the falsetto, influence only the *timbre* of the voice, while the lower ligaments are its essential organ. During the transition from a chest-note into the falsetto, the ligaments approach each other, and vibrate in their whole depth; they become relaxed in the falsetto, and touch only at their edges, while the lateral parts are raised by the current of air.

Dr. Hunt's account of "The Voice of Animals," contains a good summary of what has been observed on the subject, and is well worthy of perusal.

We now come to a long succession of chapters in which there is so little belonging particularly to our department, that we need not enter upon them; we may, however, in passing, notice a just remark of the author respecting the formation of the vowel sounds:

"Purkinjé, Kempelen, and most writers, physiologists as well as grammarians, consider the action of the lips quite as essential to the formation of the vowels as that of the throat. Such is evidently not the case, for all the vowels can be pronounced distinctly, though perhaps not with the same precision, the oral opening remaining the same, and the lips unconcerned." (p. 150.)

Indeed it will be found that a, and e, and perhaps i, may be pronounced pretty distinctly, though with an effort, with the mouth opened to its utmost extent, so as to preclude all motion of the lips, and throw the task of articulation entirely on the throat.

There is another thing which we must notice in flying over these chapters—namely, that our psychological virtue is greatly outraged by the following sentence, which is to be found at p. 172:

"As Steinthal does not recognise in the primitive formation of language any act of reason, he appears to hover midway between the innate ideas of Locke, and the pre-established harmony of Leibnitz."

Innate ideas of *Locke*, who denies altogether the existence of innate ideas!

In Chapter XVIII., "On Disorders of the Voice," we meet with some therapeutical remarks which are worth about as much as lay-medicine generally is—how much that may be we will not specify, lest we should be thought illiberal and unpolite. In the same Chapter, Dr. Hunt expresses his opinion, that the peculiar affection known as *dysphonia clericorum*, or clergyman's sore-throat, is mainly attributable to three causes :—1st. The fact of the vocal organs being suddenly called into violent action on Sunday, after a week of comparative quiescence. 2ndly. The stooping position of the head in reading, and consequent compression of the larynx. 3rdly. The assumed tone adopted by preachers, in order to give greater solemnity to their sermons, and which has an injurious effect on the voice. The first of these assigned causes may have some little influence, though certainly not much, especially as many, if not most, of our clergy preach on other days besides Sunday. The others we do not regard as worthy of a moment's consideration ; for the second should evidently be still more operative in respect to professors at universities, and others who are in the *daily* habit of reading in public, than to clergymen ; while the third should be more influential in the case of actors, than of any other persons whatsoever.

Chapter XIX. is on Deaf-dumbness and Muteism—

"The acquisition of speech," says Dr. Hunt, "is no doubt the result of imitation, but of an imitation which is really marvellous; for the process of articulation is not merely very complex, but mostly hidden from sight. Hence the instinctive attempts to imitate articulate sounds are at first unsuccessful, until at length the infant hits, as it were by chance, upon the proper mode, and, by repeated efforts, gradually acquires the control over the requisite contraction of the muscles concerned in the production of speech sounds." (p. 317.)

Would it not be nearer the truth to say that there exists in man an instinct of speech, which acquires its full activity only under the influence of imitation? It is not merely a general principle of imitation that is at work, for in that case a monkey would learn to speak sooner than a child ; neither is it merely a special disposition to imitate articulate sounds, since a parrot possesses this in an eminent degree, without ever acquiring real language ; but it is a strong innate tendency to form articulate sounds, *and to connect them with definite ideas,* which tendency is developed and invested with its particular character, by imitation. The rudest and most stupid savages speak, while writing—a more exact though less ready means of communicating ideas—is adopted only in states of comparative civilization ; the reason of which appears to be that speech originates in an instinct, while writing is the gradual result of reflection and experience.

Dr. Hunt considers it remarkable "that Sir B. Brodie, though a stanch opponent of phrenology, appears inclined to assume the existence of a special organ of speech." (p. 320.)

Sir B. Brodie's remarks, however, have reference rather to the power of articulation, than to speech in the extended sense of the term. He argues that "if there be a part of the brain whose office it is to combine the action of muscles for the purpose of locomotion, it is a fair conclusion that there is some other part of it answering the same purpose as to the muscles of speech."*

We think there is force in this observation ; and it seems probable that those birds which have the power of articulation, possess some

* Psychological Inquiries, p. 49.

such special organism in common with man, which, however, fails to endow them with what is properly called speech; they have ideas, they have imitation, and they have the power of producing articulate sounds, but they lack the instinct of speech, which is given to man alone. Dr. Hunt states correctly, as the general result of observation, that

"while, excepting the cerebellum, the septum, and the fornix, most parts of the encephalon affect more or less the power of speech, the chief influence is exercised by the anterior lobes of the brain." (p. 323–4.)

He adds:

"And such a result might have been expected; for without adopting either the phrenological principles, and (or) the inferences drawn from them, it cannot easily be denied that the perceptive powers, and the reflective faculties in general, much depend on the development and the healthy state of the anterior lobes of the brain, and that such being the case, their influence on language must be paramount. We may also fairly assume that cases of muteism without deafness or idiotcy, are not the consequence of the absence of a special organ of speech in the brain, but result either from an abnormal condition of some part of the vocal apparatus, which may defy detection, or from a derangement either in the nervous centre, or of the conductor-nerve supplying the nerve-force to the organs of articulation." (p. 324.)

It is not probable that dumbness is often connected with the *absence* of a special organ of speech in the brain; for, if it were so, we should long ere this have found out the proper situation of such organ; but we think it highly probable that disorder of some particular portion of the brain which presides over speech may be the cause of dumbness in cases where this is not otherwise to be accounted for.

Under the head of "Education of Deaf Mutes," our author gives a good sketch of the literary history of the subject, and then alludes to the principal means at present employed in educating the deaf and dumb — namely, natural and methodical signs, written language, dactylology, and lip-reading and articulation; but his remarks on these topics are meagre in the extreme, and as we are not bound to supply his deficiencies, we will here bring our notice of his work to a conclusion. In doing so, we may say that, placing ourselves in the position of the general reader, which is the only one we are entitled to assume in respect to a considerable part of the matters treated of, Dr. Hunt's work contains a vast variety of information, which seems to us of a less inaccurate character than that usually to be found in books of such very comprehensive scope. Possibly the professed philologist, or the professors of many other departments of knowledge, might find, in the portions bearing upon their own particular subjects, defects as great as those which we discover in the parts relating to medical science. But that is no affair of ours; and altogether, we think that Dr. Hunt's book contains much that may be acceptable to a large class of readers.

REVIEW XVI.

Cholera-Epidemien i Kongeriget Danmark i Aaret 1853. Ved DR. TH. BRICKA.

The Cholera Epidemic in the Kingdom of Denmark in 1853. By DR. THEODORE BRICKA 8vo, pp. 288, with Five Plans. *Copenhagen*, 1855.

WE have in former numbers of this periodical called the attention of our readers to the details of the cholera epidemics in Sweden and in Norway, and we are now able to complete these reports by the statistics and history of the epidemic of cholera in Copenhagen and in Denmark in 1853. It was in that year that the last severe visitation of cholera occurred in England, when Newcastle-on-Tyne and many other towns suffered so much from the ravages of the disease. The admirable organization of the profession in Denmark has provided ample reports on the progress of the disorder, both in the city of Copenhagen and in the various localities of that kingdom where it manifested itself, and these reports have been very ably condensed by Dr. Bricka in the present volume.

Copenhagen had up to 1853 entirely escaped the ravages of cholera during the great epidemics that had spread over Northern Europe, devastating the countries on either side of the little kingdom of Denmark.

In 1848, when cholera prevailed extensively in Sweden, as well as in other parts of the Continent, a few isolated cases did indeed show themselves in Denmark, but the malady did not spread beyond the immediate vicinity of the spot where the cases first appeared. A well marked case of cholera certainly occurred on the 6th of November, 1848, at Dragoer, on the island of Amager, close to Copenhagen. The patient, a man of sixty, and an assistant pilot, recovered; but his wife, who attended upon him, took cholera on the 11th of November, and died on the 16th. A cousin of this woman's, a female, aged thirty-eight, nursed her in her illness, and then took the disease on the 13th, and died on the 15th of November. It was ascertained that the old man in whom the malady commenced, had been on board of a ship coming from St. Petersburg, where cholera then prevailed; but it could not be made out that any of the crew of that ship were then affected with the malady. A slight epidemic of cholera likewise showed itself at Bandholm, on the island of Lolland, in 1850, in the month of August, and this epidemic was peculiarly fatal among children; nor could it be traced to any positive importation. A schooner from Lubeck had indeed come into the harbour on the 28th of July, but none of the crew were sick, and Lubeck was not declared to be infected till the 31st of July. The disease, however, would probably be in the town for a few days before the fact of its presence there was declared.

In Corsoer, in Zealand, two or three cases of cholera were observed in October of the same year. The first was in the person of a sailor, who had been working on board of two vessels from suspected ports; and the second victim was a shoemaker, who inhabited the same chamber as the first. A number of minor cases afterwards occurred in the same house, and in others in the same street, but the malady did not spread further.

Copenhagen.—When cholera broke out in the capital of Denmark, in June, 1853, there was no cause for anticipating its appearance. At the time when the malady had prevailed in former years in all the neighbouring kingdoms, its vicinity to Denmark was indicated by an extraordinary prevalence of diarrhœa, both in country districts and in Copenhagen itself. In 1852, when no cholera existed in Sweden, Norway, or Germany, diarrhœa was extremely prevalent in Copenhagen; but in the spring of 1853, that capital was remarkably free from bowel complaints; so much so, indeed, that in April and May only four fatal cases are recorded (inclusive of infants); and in the first half of June not a single death occurred from the same cause. Cholera at that time prevailed at St. Petersburg and at Cronstadt, but this was its nearest approach to Copenhagen.

On the 11th of June, 1858, a carpenter, who worked on a dredging-lighter at Nyholm, was attacked with vomiting and diarrhœa, and on the following day he was placed in the Nyboder Hospital. He recovered, and went out on the 25th of June. On the 12th of June, a man of fifty-nine years of age was brought into this hospital from the same locality; he was placed alongside of the first-named patient, and died on the 15th of June of well marked cholera. The disease then spread through this hospital, and showed itself next in various parts of the city. It is certain that the epidemic first appeared in the Nyboder quarter, and then spread itself over the north-eastern portion of Copenhagen. The inmates of the prisons in Nyhavn entirely escaped. The small, narrow streets in the centre of the town were less severely visited than some of the larger thoroughfares; nor was the malady most intense in the dirtiest localities. In the district of the Vesterbro, where the drinking water is notoriously bad, where butchers'-shops and pigsties abound, and where drainage is scarcely possible from the want of fall, only a very few cases of cholera occurred.

The epidemic lasted from the 11th of June to the 13th of October.

It was a pretty general rule that the disease died out soonest in those parts of the town where it showed itself at first with most marked severity. Two streets (the Vester gaden and the Fridriksborg gaden) in both of which an epidemic of typhus had prevailed during the previous summer, remained singularly free from cholera.

The mortality was greatest in the back rooms of sunken dwellings, and next in the rooms immediately above those, and was least in those rooms that looked into the street. The relative condition of life of the persons inhabiting these respective dwellings must not be overlooked, as the dwellers in the back rooms were mostly individuals of the lowest class and in the most impoverished circumstances. Cholerine and diarrhœa were of course extremely prevalent during the whole course of the epidemic. The total number of cases was 7219, and the deaths 4737.

The aged were the readiest victims, not one individual above eighty years of age recovered when attacked, and children under a year old rarely survived. The mortality from other diseases remained during the epidemic at its usual average. In the garrison of Copenhagen, amounting to 6500 men, there occurred 116 cases, 68 of which proved fatal—a very small per-centage indeed of cases, but which has been observed to occur also among garrison soldiers in England.

Cholera at this time (June, 1853) prevailed only at St. Petersburg and Cronstadt, from which ports ships arrived almost daily at Copenhagen. Previous to its outbreak there, no cases had occurred in other parts of Denmark; it was precisely in the port at which the ships from St. Petersburg arrived that it made its first appearance. No positive evidence is, however, forthcoming of the importation of the disease; but once within the precincts of this large city, the proofs of direct infection were easily to be found. Thus, Dr. Bricka tells us of the great number of cases and deaths among the attendants in the cholera hospitals, though here, too, there occasionally were remarkable exceptions. None of the female attendants in the Hospital of Our Saviour fell victims to the disease; indeed, only one was attacked with cholera, though this building was ill placed in a sanitary point of view, and was constantly crowded with cholera patients. Dr. Bricka does not believe in the reputed danger of cholera hospitals as "foci" of infection. Some quarters of the town immediately in the vicinity of these establishments remained free from the disease. Nor does our author admit that the epidemic could be referred to a sudden miasm brought across the seas by the wind, or generated in the town itself. No important changes in the condition of the city had been made for many years. The malady, he observes, crept through the streets like a contagious disorder, and did not burst at once upon the whole city, as is the case with influenza and other true epidemics. But while he denies the production of the malady by local causes, he is fully prepared to admit their important influence on the progress of the disease. Uncleanliness, overcrowding, and bad food are all disposing causes of cholera; but in Copenhagen, as was also the case in Warsaw in 1832, and elsewhere, the malady was by no means the most severe in those localities where these causes were specially in operation. In some newly-built houses the malady was especially virulent, while in others of equally recent construction it made no progress at all. Among ten families living close to the water, and at the mouth of the open town sewer, on the Norrebro, there was not a single case of cholera.

The measures taken by the Government and by the medical authorities, when the cholera was declared to exist in the town (June 24th), deserve our attention. A sanitary commission was established, and five offices were opened directly in various quarters of the town to receive announcements of cases of cholera. Physicians and students were in constant attendance day and night at these offices, ready to visit the sick in the localities indicated, to provide them with nurses, or, if necessary, to procure their immediate transport to the cholera hospitals. The hospitals were likewise duly prepared for the reception of the sick, and early in July the regular house to house visitation was duly organized by a society of medical practitioners and students. This system was at first vigorously pursued, but it had not the wished-for effect of staying the disorder; on the contrary, the epidemic continued to increase, and reached its height about the third week after house to house visitation was commenced.

The disease then spread so rapidly that the system could no longer be carried out, and moreover it became evident that the premonitory diarrhœa so much insisted upon by English writers did not in many cases exist. We have ourselves observed this during the great epidemic at Newcastle-on-Tyne in August and September, 1853; many persons were then struck down at once, without any premonitory diarrhœa, and sank rapidly without rallying from the first shock. On the 24th of July, in the very height of the epidemic, the important measure of removing all the inhabitants from the infected houses and districts was commenced. Tents were pitched on open spaces within the city, and to these tents the healthy from the infected houses were removed. In this way 345 families were removed from the foci of infection, and placed in a purer atmosphere, and to all appearance with the happiest results. Very few cases of cholera occurred in the transplanted families, and a still smaller proportion of deaths.

Some of the dwellings from which these families were displaced at that time were not opened to them again, but were subsequently taken down and rebuilt. The greatest cleanliness, and the most efficient sanitary domestic precautions were enforced by the police; the theatres, and even the open-air tea-gardens, were closed (we doubt the prudence of the last-named measure), and an improved rate of diet was ordered for the workhouses and for the prisons. The military and naval establishments were likewise placed under strict superintendence. Soon after the emptying of the infected houses had been commenced, the epidemic began to abate, though the weather continued close and sultry.

It is more interesting, as bearing upon the great question of the contagion or non-contagion of cholera, to trace the progress of the malady in country districts, than to attempt to follow its development in large towns. In the latter it often spreads with terrible rapidity, and in a very few days the whole of a town may become so thoroughly infected, that it may be looked upon as one great focus of disease. In the country the malady is generally more slowly developed, and the mode in which it may be transported from place to place, or communicated from one individual to another, can be more easily followed. This was certainly the case in regard to the progress of cholera in Sweden and in Norway, and it also holds good in regard to Denmark. Copenhagen seems to have been here the focus from whence the malady radiated to different towns and villages, as no cases occurred in country districts prior to the outbreak in the capital, and in a great many instances the fact of the importation of the disease from Copenhagen seems to be distinctly proved. Dr. Bricka is not an ardent contagionist himself, but he does not, on the other hand, attempt to explain away the fact of the disease having been brought in many cases into country districts previously quite healthy, by individuals coming from infected localities. The districts immediately around Copenhagen, and nearest the great centre of infection, will first engage our attention.

To the south of the city, and joined to it by a narrow strip of land, lies the peninsula or Island of Amager, the kitchen-garden of the Danish metropolis. From the end of May till the beginning of July, 1853, cholerine prevailed extensively here amongst the agricultural population. For the last six years this malady, or its equivalent, diarrhœa, had shown itself in the northern parts of the island in the months of August and September, but it had seldom commenced before the end of July.

The first fatal case of cholera occurred in an aged widow, who daily visited Copenhagen to dispose of the vegetable produce of her garden. She sickened on the 8th of July, on her way back from the town, and died on the 10th inst. Her daughter, aged twenty-two, next took the disease (July 12th), and then her elder sister (aged sixty-seven), who sickened on the same day and died upon the 15th. On the 17th the nephew was attacked, and died on the same day. After this many cases occurred in the village (Sundbyvester), and on the 15th July the malady appeared in the extreme south of the island, at Dragoer.

A sailor was brought thither from his vessel, which lay in the Nyhavn at Copenhagen. He died on the 16th, and his wife, who was in perfect health when her husband was brought home, took ill and died on the following day. The malady thus showed itself in two quarters at once—viz., at the villages of Sundby, in Taarnby parish, in the north, and at Dragoer, in Hollœnderby parish, in the extreme south. In Taarnby parish the disease, when once introduced, spread with fearful rapidity, and continued its ravages there up to the 25th of August. In Dragoer, on the contrary, it showed no disposition to spread, but confined its attacks to two or three of those about the individual by whom it was first introduced, and then no further cases occurred till a fresh importation took place from Copenhagen. Dr. Feilberg, the reporter from this district, adds, that he has obtained almost certain proof that the two or three cases that occurred in Dragoer, in 1848, were really imported, and not of spontaneous origin.

It is probable that Dragoer presented sanitary conditions less favourable to the spread of cholera than the Sundby villages in Taarnby. Of the sanitary state of the latter Dr. Feildberg speaks in terms of unqualified condemnation. The farm-houses in these villages (Gaarder), which were placed on rather more elevated ground than the cottages, were hardly attacked at all. The cottages of the peasantry, however, were packed together on ground imperfectly drained, and their position did not allow of free access of air to the dwellings. One farm-house, however, which was placed down among the cottages, and in front of which was a filthy poof of stagnant water, did not escape the disease. The number of cases on the Island of Amager was

562, and of deaths 338; while the mortality in the affected villages was from 7 to 8 per cent. of the whole inhabitants. Of males 288, and of females 274 were attacked with the disorder; but the deaths were 162 males and 176 females. This excess of mortality in the latter case may be accounted for by the fact that a great number of aged women took the disease, and most of these speedily died. Almost all the habitual spirit-drinkers, and those who drank to excess during the epidemic, fell victims to its ravages.

"It is perfectly plain to me," observes Dr. Bricka, "that cholera did not arise spontaneously in this district. Not one of those who laboured under cholerine took cholera, till the latter malady was imported from Copenhagen by persons who had taken the infection there, and which they brought back with them to their own families." (p. 41.)

In the southern districts around Copenhagen, cholera first appeared on the 29th of June, in the parishes of Frederiksberg and Hvidovre. The first case was that of a man who had frequently visited Copenhagen, and especially had been at a house where a servant girl had died and a child lay ill of cholera. This man died at his own house on the 30th of June; his wife and children, who nursed him in his sickness, escaped, but the malady affected a family living in the same house, and from thence spread to a house over the way, and extended itself through the village. In Frederiksberg there were 48 cases and 30 deaths, and in Valby 40 cases and 27 deaths. To the latter village the malady was apparently brought by the widow T——, who had been engaged as a nurse in one of the cholera hospitals in Copenhagen. The day after her return from thence her daughter, seventeen years of age, was seized with cholera, and died on the following day. From thence the malady could be traced to the neighbouring house, where five persons out of seven were attacked. Removal of all the inmates from the infected houses was here practised with success. In Bröndshoi, a parish lying directly north of Copenhagen, a charcoal porter was first attacked on the 10th of July, after returning from his daily work in Copenhagen. He died on the day of his illness, and eight days after his wife was seized, and died in two days. On the 13th of July, a woman who lived in Utterslöv village was seized with cholera. She had worked daily with a gardener on the Norrebro, in whose house seven persons had died of cholera, and many still lay sick. She died on the 14th of July. After her death her husband came home from a hamlet six miles from Utterslöv, and where no cholera had been. He was, however, seized with the disease after having been a day or two in the house where his wife had died, and he followed her to the grave on the 17th. In all Utterslöv there were 90 cases and 27 deaths. The village lies in a hollow surrounded by hills, which impede the free circulation of air; the drinking water is bad, and in the middle of the street there is an open drain with a mossy bottom, and which had not been cleaned out for fourteen years.

The next parish affected was Bröndby öster, where a woman, who took children to nurse, was attacked on the 21st of July. She had been in Copenhagen the day before, in a house where a child had died of cholera, and she brought back to her own dwelling some of this child's clothes. One of the nurslings, a child, aged eighteen months, was likewise seized with cholera on the 21st, and died the next day. The disease then spread into the village; there were in all 36 cases, and 14 proved fatal.

In Hersted öster, a man from Frederiksberg was attacked on the 29th of July, and was immediately secluded by the authorities in an isolated house, and the disease progressed here no further.

In Hersted vester, a woman upwards of seventy years of age, and who had just visited a family suffering from cholera in Copenhagen, was attacked, and died on the 3rd of August. Her husband next sickened, and died on the 17th. His body was placed in the watchhouse of the village, from which there was a current of air up into the church tower and loft. The bell-ringer and his wife, who frequented the church tower daily for the purposes of their calling, sickened on the 23rd of August, and from thence the disease spread to the neighbouring houses. We do not pretend to say that these last two caught the disease from the emanations of the dead body placed in the watchhouse under the church tower; but it does not appear to us at all improbable.

In Skodsborg, in a large house called the "Lysestöberiet" (candlestick manufactory), 14 cases occurred in rapid succession. The house was then shut up, and the other inmates, 40 in number, were located in tents, and the malady was instantly arrested in its progress. This was on the 18th of July, and no more cases occurred till the 9th of August, when a servant-girl from Taarbæk returned sick to her father's house, and four other cases then followed in rapid succession in the same house. Another village not far distant, Överröd, suffered severely from cholera. It contained only 207 inhabitants, and of these 36 took cholera, and 18 died, or nearly 9 per cent. of the whole population. The sanitary condition of the village was in every respect extremely bad; but Tröröd, another village in the same parish, and if possible worse in a sanitary respect, entirely escaped. The introduction of the disease into Överröd could not be traced.

In Skovshoved, nearly due north of Copenhagen along the coast, the disease broke out on the 24th of July, in the person of a girl, a servant in a house in which a family from one of the most infected districts in Copenhagen had taken refuge. On the 27th of July the master of this girl fell sick and died, and on the 28th her mother, who had come over from Taarbæk to nurse her sick child. After this, the malady spread on all sides; but it could be traced for some time through various channels, affecting the relatives attending on those primarily attacked. Nine per cent. of the inhabitants of Skovshoved were carried off.

In Taarbæk, a bathing village a little to the north, there were 65 cases and 34 deaths. Most of these cases, according to Dr. Von Rosen's report, were traceable to infection.

The first case of cholera in Lyngby, a village a few miles north of Copenhagen, occurred in the month of July, in an aged female, and proved rapidly fatal. No importation of the disease could be traced;

but the woman who washed the body took cholera and died in a few hours, and then her husband was attacked with the disease. In all there were 67 cases and 35 deaths.

In Vangede, a small village, badly situated and ill-drained, there were 24 cases and 13 deaths. The epidemic began in a female who had been a servant in a house where cholera raged in Copenhagen, and she had nursed one of the inmates. She came back ill of cholera to her own house in Vangede, and soon after her husband and her mother were both attacked with the disease, which then spread rapidly through the village.

The town of Elsinore (Helsingör) was but little affected by the epidemic, notwithstanding its daily intercourse with Copenhagen. The town is healthily situated, there is good water, and much attention is paid to cleanliness. In the early part of July two or three isolated cases occurred; but were almost entirely confined to persons who had come direct from Copenhagen. On the 24th of July cholera appeared in a sewing girl, who had been employed for several days in the house of an individual just arrived from Copenhagen, and whose wife had died of cholera in that city. The man himself at the time laboured under diarrhœa, and made use of a night-table in the room next to that in which the girl sat at work. There is reason to believe, too, that the girl used the same convenience. Moreover, it must not be omitted that she was employed on the clothes of the deceased wife which had been brought from Copenhagen. In the house in which this first patient died, eight fresh cases followed in rapid succession, and all were fatal. The malady then spread to two other houses; but was arrested by the dispersion of the inhabitants. Altogether there were 34 cases and 27 deaths. Sixteen of the fatal cases were habitual drunkards.

About a dozen cases occurred in the village of Sörup, a few miles from Elsinore, and subsequent to the outbreak in that town. These cases could not be traced to importation; but the malady seemed to be decidedly infectious when once established in the village. The medical attendant ascribes the outbreak of the disease to the neglect of sanitary precautions; but while these would predispose the inhabitants to its influence, the fact of its existing only a few miles off at Elsinore would lead to the suspicion that it was imported from thence.

In Hilleröd, a village nearly west of Elsinore, there were 15 cases. The first was in the person of a female who arrived sick from Copenhagen, and in whom cholera quickly developed itself, proving fatal on the succeeding day. One of the corpse-bearers took ill on the day of the funeral and quickly died, and a servant in the house of the first-named patient died four days after. Out of the 15 cases of pure cholera, there were 14 deaths.

In Frederiksvaerk, on the Isefjord, there were 18 cases and 11 deaths. The first was in the person of a young widow who had arrived the day before from an infected house in Copenhagen. Her lover had accompanied her from Copenhagen and attended upon her all the time of her illness, from the 12th to the 17th of July, on which day he himself became affected. On the 19th, the widow's mother, who had likewise been in Copenhagen, and had arrived from thence on the 16th, took the disease. Next her husband was attacked, who had never been in Copenhagen at all, but had remained much in his daughter's room, and when the second patient was removed to the hospital he had laid down in his yet warm bed. The disease then spread irregularly through the village, and in many cases communication could not be traced. Some of the infected houses were occupied by persons in easy circumstances, while in others there was a total neglect of sanitary measures.

In the little village of Anderöd another small epidemic of cholera occurred; but it was confined to three houses. The first case was that of the husband of a woman who had the day before returned from Copenhagen with diarrhœa. The reporter of these cases is a strenuous anti-contagionist, and he honestly acknowledges that it is a great blow to his theory to find that the first case of cholera in Auderöd was so evidently imported from the metropolis, or otherwise he would have been able to have proved most satisfactorily the spontaneous origin of the disease. As it stands, however, there are at least 10 cases out of the 18 in which strong proofs of infection may be alleged.

In Roeskilde, a considerable town lying to the west of Copenhagen, and on the line of railway to Corsoer, there were 14 cases of cholera, all of which could be traced to importation or infection.

In St. Jörgensby, close to Roeskilde, a case occurred, on the 15th of August, in the person of a man who had recently had a visit from his brother, a convalescent from an attack of cholera in Copenhagen. On the 22nd of August, a railway labourer, who dwelt in the same row of cottages, was attacked, and died the next day. On the 23rd, a woman who lived in the next room was confined, and two females who assisted her, and the midwife who officiated at the birth, were carried off by cholera. On the 25th, one of the newly-born twins died; but the mother and remaining child escaped. These persons were all in comparatively easy circumstances, and the houses were of a better description than ordinary. In all, there were 39 cases and 25 deaths. St. Jörgensby is a village inhabited almost exclusively by day-labourers, residing in houses which are sub-let to the utmost, and are consequently crowded to excess. Intemperance prevails much in the village, but chiefly among the other class of inhabitants, the fishing population, but amongst these, only one individual was attacked. It is remarked by the medical reporter, that the close packing of many individuals in one room seemed to have a far more deleterious predisposing influence than habits of intemperance. In the village in question, there were three dwelling-houses particularly overcrowded, and these three dwellings constituted the strongholds of the disease. The women and children suffered the most, because they were constantly within doors: the men were less affected, as their occupations took them into the open air.

Holbæk, a small town on the Isefjord, lying west of Roeskilde, was twice invaded by cholera in the summer of 1853. On the first occasion, the disease lasted from the 21st of July to the 22nd of August;

the second was from the 28th of September to the 13th of October. Diarrhœa and cholera had prevailed for some time previous to the first-named period, but was very amenable to treatment. The first case was an imported one from Copenhagen; it was mild in character, and the spread of the malady was limited. After a lapse of five weeks the disease was imported afresh by a vessel from Aarhuus, where cholera then prevailed. A sailor who had suffered from diarrhœa on his voyage from that town, arrived at Holbæk on the 28th September, and on that day was admitted into the Lazaretto, with well-developed cholera. An intemperate old woman, who attended him in the Lazaretto, took cholera on the 4th of October, and died on the 6th, and then her daughter became affected with the same malady. In the whole North Zealand medical district there were rather more than 1400 cases, and 825 deaths.

In the medical district of South Zealand cholera numbered but few victims; there were only 45 cases and 25 deaths.

The first case occurred at Karrebaksminde, on the west side of Zealand, on the 9th of July, in the person of a cook on board of a vessel which had left Copenhagen on the 7th instant. He was attended by his father, a man of fifty, stout, but intemperate, and he took the disease from his son, and died at Appernaes.

A remarkable case of suspected contagion occurred at Præstö. A captain of a vessel from Copenhagen was taken ill at Præstö, and died of cholera at Vaagoe. This man, on the day that he was taken ill (July 23rd), had been several times in a baker's shop in Præstö, and on the 27th a weakly servant girl in the baker's house died of cholera. On the same day an apprentice of the baker's was attacked, and also the wife and two children, one of which children and the wife were carried off by the disease. The malady did not spread further.

On the 23rd of July, a man from Christianshavn came to Alsted, near Sorö. Next day he took cholera, and died on the 25th. Five days after his father died of the same complaint.

On the 25th of August a patient, ill of cholera, was landed from a ship at Vordingborg, and placed in the Lazaretto on Masnedoe, where he died on the 28th. The day after his death his two attendants were attacked with diarrhœa at Nyraad, and on the 3rd of September the owner of the house where they lodged had a most severe attack of cholera. Two days after an old man died of the disease at Masnedoe.

Another slight outbreak of cholera occurred at Beenlöse, near Ringstedt, where a man and his wife, without apparent infection, were suddenly attacked with cholera, and died. A neighbour who had waited on them took ill, and died two days after. Next the mother of the last-named patient, who had attended on her daughter, and had washed the soiled linen after her death, was seized with cholera, and she communicated the disorder to two of her other children. These parties all lived in separate and distinct parts of a healthy village.

On the Island of Bornholm, separated by a wide expanse of sea from Denmark, and lying much further to the east, several cases of cholera occurred, but all at a period subsequent to the appearance of the disease in Copenhagen. It was not till the 21st of July that cholera was seen in Bornholm, the first case being that of a sailor who arrived sick from Copenhagen on that day. He was placed in a building contiguous to a hospital for aged and infirm men; and on the 30th of July, the very day on which the first-named patient went out cured from the adjoining house, five out of the eight inmates of the hospital were attacked, and all fell victims to the disease. The remaining three were then removed to another part of the town, and remained healthy. In all there were 24 cases in Bornholm, 13 of which proved fatal. In the islands of Laaland and Falster the disease first showed itself on the 8th of July, in the persons of two sailors, who had left Copenhagen in a sloop on the 2nd July, and had landed at Nykjobing, in Falster. Subsequent to their arrival, cases appeared among the inhabitants, and the epidemic continued till the 1st of September, numbering 87 victims out of 174 sick. The dispersion of the inmates of the infected houses was here also put in practice, and with the best apparent results.

On the Island of Fyen there occurred in all 210 cases and 97 deaths. The district physician, Mende, believes that the malady was first introduced into Svendborg, a town in the south of the island, by a family from Copenhagen. In the town of Odensee, on the north side of the island, there were four distinct outbreaks of the malady. At first 2 cases were imported from Aarhuus on the 29th July; then for four weeks no fresh cases occurred, and after that the disease broke out afresh, in a boy, who died on the 30th of August. No direct communication could be traced in this instance, but a journeyman blacksmith, who had just arrived from Copenhagen, was attacked on the same day with diarrhœa, which speedily ended in cholera. The malady next attacked the mother of the boy, and from thence spread to five or six of the neighbours. The journeyman blacksmith had been meanwhile conveyed to the Grey Brothers' Hospital, in the opposite part of the town; and here the next case occurred in the person of one of the aged inmates of the hospital, and several others of the Brethren afterwards fell victims to the malady. Out of 18 cases there were 12 deaths. That the disorder did not spread further is probably owing to the care that was taken to disperse the inmates of the houses in which cholera broke out. In twenty-two instances this was done, and only one individual of the whole of their inmates was subsequently attacked with cholera.

At Villestofte, near Odensee, a weaver, who, with his wife, had been to the infected town of Aarhuus two days previously, was attacked with cholera on his journey home, and died on the 30th of July. The family, four in number, was then moved to the house of the widow's parents, about a mile from Villestofte. The weaver's son, a boy of twelve, was there seized with the disease, as were likewise the grandfather and grandmother, and all three died on the 2nd of August. The widow now returned to her own house with her three remaining children, when all were prostrated by the malady, and another of the children died. In fourteen days 8 cases and 5 deaths occurred in two houses.

In Nyborg cholera appeared on the 18th of August, and continued for twelve weeks. There were in all 34 cases and 24 deaths. Importation could not be proved, but the locality in which these first cases occurred was eminently favourable to the spread of cholera. The disease broke out in a room over a cellar which had not been entered for twenty years, and was then full of water. When the water was pumped out, twenty wagon loads of accumulated filth had to be removed before the floor could be reached. The only possible way by which the disease might have been imported was, that a girl from one of the most infected houses in Copenhagen came to lodge in this house shortly before the 18th of August.

On the mainland of Denmark, the ravages of cholera were confined to only a few localities. A well-marked case of importation occurred in the Ljimfjord on the 27th of July. A young woman came on that day by steamboat from Copenhagen to the Island of Morsoe, and proceeded directly to her parents' house at Karby. On the 29th she was seized with cholera, and died on the 31st. On the 1st of August her brother was attacked, and died, and on the 3rd of August her sister and both her parents sickened of the malady; the father and the sister died, but the mother recovered. The house was then carefully secluded, and no more cases occurred.

In the town of Aarhuus the disease broke out on the 19th of July; no direct importation could be traced, but a student of medicine, who had been attending cholera patients in Copenhagen, arrived on that day in Aarhuus, suffering from diarrhœa. A servant girl was seized with cholera on the 22nd, but it was not ascertained that she had been in communication with the student before mentioned; she was placed in the hospital of the town, and the malady then spread in a day or two among the inmates. In Aarhuus there were 336 cases and 213 deaths. Infants and old people suffered most severely, and the mortality among females was in excess of that of males. The dirtiest, narrowest, and most populous streets, were most ravaged by the disease. The population of the town is about 9000, the mortality, therefore, was about 2 per cent. Here, as elsewhere, the dispersion of the inmates of an infected house seemed to be the only effectual means of arresting the progress of the disease.

In the extreme north of Denmark, on the great inlet of the Ljimfjord, stands the town of Aalborg.

In this town, and in the surrounding country, there were 986 cases of cholera and 544 deaths. The town has always been noted for its unhealthiness, its annual mortality being nearly as high as that of Copenhagen. The town lies low, and close to the water; indeed, it is partly built on land reclaimed from the sea. During the month of June, and the first half of July, the health of the town had been better than ordinary. On the 27th of July, the steamer *Cimbria* arrived from Copenhagen. The night before reaching Aalborg, two of the crew of the steamer had died of Asiatic cholera, and their bodies were carried on shore and buried at Aalborg. On the 31st July, cholera broke out in the town, and continued from that date to the 7th of

October, there being in that period 762 cases and 353 deaths. The population in 1850 was 7745, so that nearly 10 per cent. of the inhabitants were attacked.

The able author of the medical report on Aalborg, Dr. Speyer, is a strenuous anti-contagionist, and he believes the malady to have arisen spontaneously. There is, however, a most suspicious coincidence between the arrival of the infected steamer *Cimbria* and the outbreak of the disease a day or two after.

The intemperate here did not suffer more than those of sober habits. The dispersion of the inmates of the affected houses was commenced early in the epidemic; but ere long the number of those seeking a refuge in the temporary asylums became so great, that it was thought necessary to reopen some of the earlier infected dwellings. This was done; but the measure proved a very unfortunate one, for although the houses had been thoroughly cleansed and ventilated, cholera reappeared among the inmates after their return.

A few cases, and those chiefly imported ones, occurred further inland on the banks of the Ljimfjord. In Sebbersund, the captain of a ship, who had returned from Aalborg on the 9th of August, was attacked with cholera on the 11th; on the 19th his wife took the disease, and died on the 21st. A sister who had waited on her took cholera, and died on the 22nd. Another family resided in the same house, and the disease next appeared amongst these. Next, the malady broke out in a house at the other end of the village. A labourer who dwelt there had been appointed a guard over the first affected dwelling; and it was proved that during the night he had gone to the house and had stayed there for some time; he took cholera on the 14th September, and died on the same day; his two sons and his wife were next attacked. The disease did not appear in any other house in the town; but a woman belonging to Vaar helped to put the above-named guard to bed when he was seized with the disorder, and then went home herself, took cholera, and died the next day.

On the 17th of August, a fugitive from Aalborg arrived at Gadenlund, a village several miles north of that town; no cholera had been previously observed there. On the 22nd August, a peasant who had been stacking peats on the 18th and 19th, in company with the Aalborger fugitive, took the disease, and died on the 24th. On that day the woman of the house took ill, and died on the 25th. The widow of the second patient was next attacked, and also an old pensioner, who had been stationed as a guard at the door of the infected house. In all there were 7 cases and 4 deaths.

The town of Fredrikshavn, near the extreme north point of Scaw, or Skagen, was perfectly healthy till a cholera patient from Copenhagen was landed there on the 20th July, from the steamboat *Waldemar.* The two succeeding cases were in persons who had been in communication with the first attacked. The first eight cholera nurses fell victims to the disease. In all there were 100 cases and 67 deaths. Of 11 cases above seventy years of age, all died. In one house there were not less than thirteen persons attacked.

The total number of persons who were attacked with cholera in Denmark in 1853 was 10,598, and of these 6688 died. Out of the former number, 9037 were inhabitants of the capital or of market towns, and of these 5785 died; while in the country parishes there were only 1561 cases and 903 deaths. It was in the immediate vicinity of Copenhagen, the great focus of the epidemic, that the country parishes suffered most severely. Those parishes in conjunction with Copenhagen and the Island of Amager afforded not less than 10,293 cases, and 6500 deaths; while the more distant country parishes only give a sum total of 305 cases and 188 deaths.

With regard to the means adopted to stay the progress of the epidemic, the reporter tells us, that on the 1st of July the Medical Society addressed the Health Commission in Copenhagen, and laid before it the detail of three measures which they considered available for this purpose. These measures were:—

1. House to house visitation.

2. Evacuation of the infected houses, by removing all the healthy inhabitants to other localities.

3. The providing of eating-houses (*Bespiishings ansfalten*) for the dispossessed poor, and for those unable to procure a proper daily supply of food.

In regard to the first-named measure, Dr. Bricka informs us that it was far from being followed by the beneficial results ascribed to its operation in other countries, and especially in the English reports on cholera. The visiting staff organized in Copenhagen was perhaps more complete than has ever before been attempted. It consisted of 86 physicians and 42 senior students, who devoted all their energies to the accomplishment of their task. House to house visitation was commenced on the 10th of July, at the period when the epidemic suddenly increased in virulence, and rose at once from 305 to 1074 cases in the week. The next week, however, the cases rose to nearly 2000 (1907), and the week after the number was hardly diminished, for it was 1796.

Dr. Bricka illustrates the inefficacy of this measure by tabular statements, and then sums up his opinion of house to house visitation in the following words:—

"Although this measure (house to house visitation) was here practised earlier, and pursued more energetically and fully, than was ever done in other countries, we are bound to confess that the results were far from being so favourable as they are stated to have been elsewhere. Perhaps the success attributed to this measure in England and in other lands arose from its having been had recourse to at a later period of the epidemic, at a period when it had reached its culminating point, and the coincident cessation of the disorder was merely its natural diminution occurring at the period when house to house visitation was commenced. But if this measure failed in exercising any marked influence on the progress of the epidemic, it might at least be expected that the daily or weekly visiting lists would indicate the approach or receding of the malady, either in regard to the whole town or in particular districts. This, indeed, was partially but not invariably the case, for there were several instances where the malady invaded certain localities without any indications of its approach having been obtained by the visitors."

Dr. Bricka, however, by no means denies the utility of the system of house to house visitation, for he observes—

"I believe that the tranquillity of the town and the admirable order preserved by the people amid the horrors of the epidemic, were mainly due to the confidence inspired by these daily visits, and to the good counsel and moral influence afforded by the visiting physicians. Numberless sanitary deficiencies were likewise brought to light in the daily rounds, and most speedily remedied; and the visits went hand in hand with the measures for supplying a sufficient amount of food, the visitors pointing out the localities where these supplies were to be obtained; and lastly, they prepared the way for the evacuation of the dwellings, by making known the houses from which the inmates ought at once to be removed. I am therefore of opinion that house to house visitation, if cholera should again break out amongst us, must not be omitted; but I believe it may be safely practised on a less extensive and less costly scale." (p. 245.)

The testimony of Dr. Bricka as to the efficiency of the second measure —viz., the evacuation of the infected houses—is too valuable to be omitted. This measure was likewise proposed on the grounds of its being recommended by the English Board of Health; but all are aware how feebly it has in any case been carried out in this country. There is a difficulty in turning an Englishman out of his home, however poor that home may be, which is not experienced in more despotic continental cities.

"Although," says Dr. Bricka, "this measure was not put in force until the epidemic had reached its culminating point, and although it was not so energetically and completely carried out as might have been desired, its results were very different from those obtained by the former method. The whole arrangements for the evacuation of the infected houses was confided to one man, Mr. Thomsen, of whose devotion and intelligence it is impossible to speak too highly. Four large asylums were opened for the dispossessed inmates; they were partially lodged in tents and partly in some of the empty barracks of the garrison. The number of healthy persons removed from the infected houses amounted to fully 2000; and the removal was immediately followed by a most marked abatement in the epidemic. . . . . .

"It is well known that very few cases of cholera occurred among the healthy inmates removed from the infected houses. In the tents on Christianshavn, whither 376 individuals were brought, there were 30 cases of cholera and 15 deaths; but eleven of these cases occurred on the day after their removal, so that in all probability they had brought the disease with them from their own homes. Many of these people, too, although they slept in the tents, yet wandered back again during the day to their old homes, or to other places where cholera was raging. In the other three asylums, wherein upwards of 1100 people found refuge, there *were only two or three cases of cholera and not a single death*."

In all cases, too, where the same measure was put in practice in the provincial towns, it appears to have been followed with the like gratifying results.

We regard Dr. Bricka's report as a most valuable contribution to the history and pathology of cholera. Though himself evidently not a fervent contagonist, he has faithfully reported and commented upon the great array of facts bearing upon this question, and certainly favourable to the doctrine alluded to, which are contained in these pages.

From a perusal of the more recent cholera literature, we are led to believe that there are few parties at present who are disposed absolutely to deny all contagion in cholera, as was done in a more or less complete form in certain of the Reports of the English Boards of Health. The facts, however, that are laid before us in this present report of Dr. Bricka's amply confirm the opinion we on two former occasions expressed in this journal, that in outlying and thinly populated districts the contagious character of cholera is more apparent, and its progress from house to house can be more easily traced, than amid the concentrated infection of large towns. The unsatisfactory results of the system of house to house visitation as compared with the other measure, that of separating the healthy from the sick by the speedy removal of the former from the infected houses, is only what was to be expected if cholera be really propagated by contagion. Should the disease ever revisit our shores—and there is no reason to suppose that it will not return—we would urge at once upon the authorities the evacuation of the infected houses by the healthy inhabitants as the great means of safety, though we would not altogether neglect the moral influence on the people, and the local knowledge of the habitats of the disease, to be obtained by regular house to house visitation.

PART SECOND.

Bibliographical Record.

ART. I.—*The Work and the Counter-work, or the Religious Revival in Belfast.* By EDWARD A. STOPFORD, Archdeacon of Meath. Third Edition.—*Dublin,* 1859. pp. 104.

IT is very palpable that, however much of genuine religion may find a place in the revivals that are now going on in the north of Ireland, there is a very great alloy of the meretricious and profane. This admixture is the more painful where it is promoted and fostered by those whose duty should especially lead them to guard the holiest aspirations and emotions from being sullied by carnal and morbid passions. The Archdeacon of Meath, in addressing his clerical brethren and the public, writes with a very good knowledge of the vagaries to which ill-fed bodies and ill-stored minds lay open the nervous system; and asserts roundly, that much of what is creating so great an interest in Belfast, is nothing more than hysteria, male and female assuming for the nonce the garb of spiritual conversion. To medical men this is nothing new, and we should merely regard as truisms such sentences as—"I cannot recognise as the act of God, hysteria thus produced and forced, neither can I so recognise hysteria which is produced by preaching." But it is not necessary to converse with many clergymen on these topics in order to discover that, as a body, they are ignorant of all means by which they may distinguish the vigorous manifestations of a healthy mind from the vicious exhibitions resulting from morbid excitement. Let them read the Archdeacon's pamphlet, which will enable them to estimate correctly much of what the daily papers report, concerning the particular phenomena in question, while it will prove a guide to them in their daily walk of life, and render their ministrations more useful, by enforcing the soberness and earnestness of true Christian regeneration. Well does our author illustrate this. Having maintained that numerous cases which he saw at Belfast were unmistakeably hysterical, he continues—

"It becomes us to inquire whether the preaching of Christ and his Apostles did ever produce hysteria; I have read of Him that He went about 'healing *all manner* of sickness and *all manner* of disease among the people' (Matt. iv. 23); but I have never read that his preaching did ever create anything of the nature of disease. I have read that our Lord did give power and commission to His twelve disciples 'to heal all manner of sickness and all manner of disease' (Matt. x. 1), but I never read that any sermon preached by them, or any word spoken by them, except in the execution of miraculous judgment, did ever produce anything of the nature of disease. On the day of Pentecost itself, to which the prevalence of hysteria under preaching is now compared, when three thousand were pricked in their hearts on the awful charge that *they had crucified* Him whom God had made both Lord and Christ, I find no trace of hysteria, but on the contrary, in the very period of conviction, the best and highest exercise of their moral and intellectual faculties."

The Archdeacon goes on to demonstrate how great the moral power is which may be exerted over these morbid manifestations, and shows by his illustrations how, on the one hand, they may be checked by firmness and decision; how, on the other, by yielding and vacillation feeble symptoms may be nursed into uncontrollable paroxysms. Owing to peculiar circumstances in his ministerial career, our author has had extensive experience in the treatment of hysterical phenomena, and he has been led to a strong conviction that—

"Apart from all Divine or miraculous power, and assuming only the facts of our constitution and the influences which we know to act on it, hysteria could neither have arisen nor existed in the presence of Christ; that it must have stood mute and have ceased in the presence of the calm power of his perfect manhood. The woman taken in adultery could not become hysterical, she stood quietly in the midst, and spoke calmly. Neither could that sinner, who poured forth all the emotional feeling of her soul in washing his feet with her tears and wiping them with the hair of her head."

There are two main elements in the production of the dangerous and painful phase of these revivals which we are considering; the one active, the other passive; the one the mistaken, and it may be culpable, interpreter of the word of God; the other the feeble, excitable, and erring listener in the congregation. Let us see how, when these two elements come into contact, the result cannot fail to be such as delineated—

"I was myself present in a Presbyterian meeting-house at a prayer, offered with the most frenzied excitement and gesticulations, that God would, then and there, descend and strike all the unconverted to the earth. That prayer was accompanied throughout by a storm of cries, and groans, and exclamations, and amens, all having the true hysteric sound. This was the most frightful scene I have witnessed in life; at the moment of the awful COMMAND to the Almighty to come down and strike, it was perfectly terrific. No such scene would be permitted in any Bedlam upon earth. Presence at such a prayer could be redeemed from guilt only by the purpose of warning. I have many terrible recollections of life, but this prayer is the most frightful of them all. I have been used to be calm in the presence of hysteria; I was calm then; but the physical effect upon myself was as if I had been drinking plain brandy."

Can we be surprised that girls fed upon bread and tea, worn out by work, and subjected in crowded and ill-ventilated assemblies to such influences, should yield to their emotions, especially when they are anticipating some peculiar manifestation of spiritual influence?

In another instance, the preacher, taking the parable of Dives and Lazarus for his text, neglected all other topics which it might suggest, to the exclusive consideration of hell; hell, h-e-ll, h—e—ll, was the one cry; and the "sole object aimed at was to produce a sensation of intensified torture of physical self-feeling." The sermon was remarkable for the paucity of ideas, some passages were devoid of them. One part in which, by a constant repetition and transposition of "the existence of Dives," and "endless duration," no impression remained upon the hearer but the prolongation of agony, struck Archdeacon Stopford as remarkable for its skill in wording; but the whole object of the speaker seemed to be the elimination of every idea or thought, "and it was precisely here, where every idea had disappeared, that the preacher bestowed the whole force of voice, and tone, and gesture," a fact which the narrator had observed in other sermons before.

The effects were such as any one remotely conversant with mental operations must have anticipated—

"Precisely as I expected, when all sense and meaning was gone, the preacher had his base and unmanly triumph in evoking a wild and long-continued scream of hysteric agony, which as it rose more loud, and thrilled more wild, did effectually silence the preacher, and left him standing in his pulpit with a most self-satisfied air, until her tardy removal enabled him to proceed. My horror was not lessened, as I watched the effect on a well-dressed and nice-looking girl near me. Her countenance did change at that fearful cry, but it changed into an expression of steeled indifference hateful to be seen in woman."

We have no doubt that Archdeacon Stopford's pamphlet, which we need scarcely state is written in a truly Christian and manly spirit, will be largely read. We feel assured that it will prove of special use to the clergy, by initiating them into mental and physical phenomena with which few of them have any acquaintance, but which they must learn to appreciate, if they are to know the full scope of their ministrations. The author lays much stress upon the information to be derived from medical men by the clergy upon distressing and perplexing parts of their duty, and advises them to have recourse to the advice and guidance of Christian physicians. It is only exceptionally that now-a-days the functions of the clergyman and the physician can be suitably united in one person, but this renders a good mutual understanding and co-operation between the two professions the more necessary, and we trust that the suggestions on this point will not be thrown away upon the clergy. Nowhere is this co-operation more wanted, more imperative, than in large towns, and we feel assured that we shall never see the work of the clergy crowned with all the success we desire for them, until they have practically and universally acknowledged the necessity of this union. The abuses that have been manifested at Belfast prove this in a palpable way, but in a minor degree circumstances are daily occurring throughout the country which remind us of the necessity of a more cordial interchange of good offices between the clergyman and the physician. Let it be our part to do all that lies in our power to promote this consummation.

In taking leave of Archdeacon Stopford, we would venture to suggest a doubt as to his view being correct, that all the cases he witnessed were solely hysterical. We are almost disposed, from his descriptions, to infer, that some of them must have been epileptic. This, however, in no way affects his general argument, but rather strengthens it; inasmuch as we are thus required to admit the production of even a more serious form of disease than the one to which alone the author adverts, as resulting from the prevailing excitement.

ART. II.—*The Surgeon's Vade-Mecum: A Manual of Modern Surgery.* By ROBERT DRUITT, Licentiate of the Royal College of Physicians, London; Fellow of the Royal Medical and Chirurgical Society of London, &c. &c. Eighth Edition. Much improved, and illustrated by three hundred and twenty-eight highly finished wood engravings.—*London,* 1859. pp. 791.

THIS popular volume, now a most comprehensive work on surgery, has undergone many corrections, improvements, and additions, and the principles and the practice of the art have been brought down to the latest record and observation. In a short preface the author thus points out the novel features of the edition:

"In the chapter on Inflammation, which is entirely new, I have endeavoured to present the facts in a modern, practical guise, stripped of the formal old Hunterian phraseology. Pyæmia and phlegmasia dolens are removed from the chapter on the Veins, and are treated of in their natural alliance with erysipelas and diffused inflammation. Due notice has been taken of the use and abuse of caustics in the treatment of cancer. The arrangement of the chapters on Injuries has been altered, so as to give due prominence to the comparative safety of subcutaneous injuries. The whole chapter on Gun-shot Wounds has been written afresh and very much enlarged, from materials kindly placed at my disposal by Mr. George Lawson. The chapter on the Eye has again been most kindly revised by Mr. Haynes Walton, whom I have, besides, to thank for the materials for a section on the Ophthalmoscope. The treatment of anchylosis by forcible extension, and of syphilis by fumigation, the recent improvements in ovariotomy, and in the treatment of vesico-vaginal fistula, the radical cure of hernia, and the subject of chloroform and the too frequent deadly results of its administration, may be mentioned as having received special addition or improvement; whilst in the last chapter I have taken pains to bring into small compass the latest and best information on Excision of the Knee-joint. It will, indeed, be a reproach to surgeons, if this humane and rational operation shall be discontinued on the plea of want of success or large mortality resulting."

In this warlike age, much surgical interest necessarily attaches to all that relates to the treatment of wounds from projectiles, and it is an imperative duty of our young surgeons who join the public services, to be acquainted with the best writings on the subject, and we have pleasure in pointing here to the pith of what has been published; besides this, there are many new facts, useful rules, and valuable hints.

Of the operations in surgery it is impossible to speak too highly. The descriptions are so clear and concise, and the illustrations so accurate and numerous, that the student can have no difficulty, with instrument in hand, and book by his side, over the dead body, in obtaining a proper knowledge and sufficient tact in this much neglected department of the medical education.

Some knowledge of the diseases of the eye is now required by all students, not only for their examinations, but in after life in the exercise of their profession; for affections of this organ are no longer entrusted to the hands of the specialist alone, although here, as in general practical surgery, the more delicate operations, those requiring for their best execution much practical skill, will ever be confined in each community to comparatively few men.

There is no modern treatment at all accredited, or any method of operating that has stood the test of practice, that has not been considered and dwelt on. Among others, we may allude to the section on the ophthalmoscope, a simple but wonderful appliance, that promises to render obsolete all that has been written on deep-seated diseases of the eye. Of course there is yet much to be made out in this department, but the rules for the use of the instrument, in ascertaining the healthy appearance of the interior of the eye, together with the chief morbid changes, may be specially recommended to the student.

ART. III.—*On the Nature, Causes, Statistics, and Treatment of Erysipelas.* By PETER HINCKES BIRD, F.R.C.S., &c. &c., Author of the Jacksonian Prize Essay on Erysipelas.—*London*, 1858. 8vo, pp. 60.

THE exposition Mr. Bird has given of his subject is exceedingly elaborate and complete, though, by a singular anomaly of arrangement, he throws the most important part of his materials into an appendix which is nearly three times as long as the little essay to which it is added. In this appendix he condenses much information, statistical, etiological, and pathological, contained in the work of Fenger, 'De Erysipelate Ambulanti Disquisitio,' which he states, we believe correctly, to be little known in England, interspersing the results of his own experience, and various matters derived from other sources. It must not be inferred from the small dimensions of Mr. Bird's work that the information it contains is scanty or restricted to a few points. On the contrary, it brings together an immense accumulation of well-digested facts, and might, in truth, be expanded into a large volume without subjecting the writer to the charge of too great diffuseness. The highly condensed and almost tabular character of the work precludes any attempt at analysis on our part; we need only say, therefore, that it records briefly, but distinctly, the observations of all the best writers on the subject, whether old or recent, British or foreign. Mr. Bird's own remarks are chiefly corroborative of or dissentient from these authorities, and are derived from 260 cases which have fallen under his notice.

With regard to the immediate seat of erysipelas, as developed in the common integument—a point on which difference of opinion exists —Mr. Bird has been led by his own post-mortem inquiries to the conclusion that,

"In the most superficial form it is seated in the papillary layer of the dermis, and that as the disease becomes more complicated, the more deeply-situated parts are involved; thus, beginning at the papillary layer of the dermis, it extends into the deep stratum or corium, then to the adipose or cellular tissue; it may thence involve fascia, muscles, and inter-muscular cellular tissue. In simple erysipelas there is merely serous effusion; in complicated erysipelas, destruction of the capillaries and all the tissues it invades." (p. 12.)

The therapeutical portion of the work might perhaps have been somewhat extended with advantage, but the writer's remarks are judicious, and to the purpose. Of the tincture of sesquichloride of iron, extolled of late in the treatment of erysipelas, Mr. Bird says that his experience "is not at all in its favour, when compared with alcoholic stimulants, to which it may, in some cases, be a useful adjunct." He adds: "It is especially indicated in albuminuria coincident with, or consequent on, general erysipelas, of which I have met with three cases." (p. 14.)

On the whole, we can recommend Mr. Bird's work, diminutive as it is in bulk, as affording the greatest amount of accurate information on the subject of erysipelas that has ever been brought together in one view; and the very limited compass within which this laborious task has been accomplished, should be considered, in such a book-making age as the present, as enhancing the merit of the writer.

ART. IV.—*The Treatment of Obstinate Ulcers and Cutaneous Eruptions on the Leg without confinement.* By HENRY T. CHAPMAN, F.R.C.S. Third Edition.—*London*, 1859. pp. 161.

THE fact of this little work having reached a third edition is sufficient evidence that it has supplied a want felt by the profession. The objects of the treatise are thus stated by the author:

"*Firstly*, to place in as strong a light as possible the greater efficiency and expediency of the treatment by bandaging; *secondly*, by the suggestion of a substitute for Baynton's strapping, exempt from its inconveniences, the extension of the principle of support to cases in which it had hitherto been considered as totally inadmissible; and *thirdly*, the advocacy of aqueous dressings, that is to say, watery solutions and preparations, in preference to topical applications of an unctuous kind." (p. 1.)

The following is Mr. Chapman's description of the peculiar method which he recommends for general use:

"*Construction of the Bandage.*—The sore being dressed with a piece of lint or soft linen, cut to its shape and dimensions, and dipped in cold water, or in one of the lotions just specified, the limb is to be strapped with wet bands of linen or calico, precisely in the same manner as Messrs. Baynton and Scott applied adhesive plaster."

The method of applying the bandage is then more particularly described:

"That part of the bandage over the ulcer should be moistened from time to time with cold water containing glycerine, or with Goulard's lotion. . . . . No one who has not tried this mode of bandaging can form an estimate of the powers of adhesion possessed by the wet strapping, or the amount of equable support it is capable of affording." (pp. 80, 81, 83.)

We cordially commend Mr. Chapman's little book to the attention of surgeons of dispensaries and hospitals, being satisfied that the easy and apparently efficient plan of treatment which he recommends will be found well worthy of a more extended trial than it has yet received.

Several other ingenious suggestions for the amended treatment of ulcers and eruptions on the legs will be found in it, which our space will not allow of our noticing more particularly.

ART. V.—*Phthisis and the Stethoscope, or the Physical Signs of Consumption.* By RICHARD PAYNE COTTON, M.D., Fellow of the Royal College of Physicians of London, Physician to the Hospital for Consumption and Diseases of the Chest, Brompton. Second Edition.—*London*, 1859. pp. 80.

WE have pleasure in again recommending this practical little volume to young stethoscopists. It contains all that is essential for the physical diagnosis of pulmonary consumption, while its statements are clear and to the point. When a third edition appears, we would suggest the addition of a ninth chapter, devoted to the physical symptoms, indicating the arrest and involution—not to say the cure—of tuberculosis; a point equally interesting to the patient and to his medical adviser.

ART. VI.—*Die Elephantiasis oder Lepra Arabica.* Von CARL FR. HECKER, Professor der Chirurgie und Augenheilkunde, und Director der Chirurgisch-Ophthalmologischen Klinik an der Universität Freiburg im Breisgau. Mit fünf lithographirten Tafeln. —*Lahr*, 1858.

Elephantiasis or Lepra Arabica. By CARL HECKER, Professor of Surgery, &c., in the University of Freiburg. With Five lithographed Plates. Fol. pp. 15.

THIS monograph is, by virtue of its excellent illustrations, large type, and river of margin, what the Germans would term a *Prachtausgabe.* It owes its production to the rare occurrence of an indigenous case of German elephantiasis, which came under the author's treatment, and proving fatal, enabled him to make an accurate inquiry into the morbid anatomy of the disease. The patient was a young woman, aged thirty-two, a native of the Black Forest, who was admitted into one of the surgical wards of the Freiburg Hospital on the 15th Jan., 1852, on account of an enormous tumour occupying her back, besides which there were sixty smaller tumours scattered over the body, not generally exceeding the size of a cherry. The tumour existed in early childhood, but it was only during the last six to eight years that the great increase had been observed in the largest one. This extended from the seventh cervical vertebra downwards, occupying the whole back, covering the nates, and being attached by a peduncle, which was sixteen inches broad and extended from the last cervical to the first lumbar vertebra. The grandfather, the father, and the child of the patient were affected with similar but smaller tumours.

It is unnecessary for us to go into the argument, in favour of or against the operation. It was urgently desired by the patient, and the surgeons of the hospital thought it called for. An attempt at passing a ligature through the peduncle failed, owing to an artery being wounded; so simple amputation was had recourse to, which exposed a bleeding surface of two feet in circumference. A large number of gaping arteries and veins had to be tied, which was done with all expedition, still the patient lost a large quantity of blood; and though she rallied from the immediate effects of the operation, she sank in the evening of the same day.

The enlargement of the vessels, as seen on the surface of the wound and within the thoracic cavity, is admirably represented, the size of life; besides these views, there are microscopic representations of the intimate structure and drawings of a few cases observed by other authors. The work contains nothing that is novel, but it will be acceptable to those who wish to have the most recent pathological investigation of the parts involved in the disease, together with illustrations that, besides having considerable artistic merit, possess the best quality that can characterize a scientific work—truthfulness.

Art. VII.—*Annales de la Société Anatomo-Pathologique de Bruxelles.* Bulletin Nos. 1 and 2.—*Bruxelles,* 1859.

Annals of the Pathological Society of Brussels. Nos. 1 and 2.

Under the presidency of the well-known morbid anatomist, Professor Gluge, a Pathological Society was formed in Brussels in 1857, destined to promote the study of pathological anatomy by an examination of specimens exhibited by the members, and by the formation of collections. The Society meets weekly, under the auspices of the University of Brussels; and it has been endowed with special privileges with regard to the pathological specimens that may occur in the different hospitals of the town. The reports are published in the form of pamphlets, as often as sufficient material has accumulated. In the two that are before us we meet with some interesting cases, accompanied in part by scientific disquisitions, which show that the writers are fully alive to the progress of science in other countries besides their own. We notice especially a carefully detailed instance of rupture of the chordæ tendineæ of the mitral valve in a young woman, aged twenty-five, occurring under excitement, and resulting from old endocardial inflammation and degeneration of the tendons, by M. Allix; an article on acute fatty degeneration of the liver, by the Secretary of the Society, M. Marcq; and another, by M. Roger, on malformations of the pelvis.

We should add that the 'Bulletins' are illustrated; nor may we deny ourselves the pleasure of offering to this young Society our cordial good wishes that it may prosper and extend its researches, to the benefit of science and humanity. Considering the constant, the daily intercourse between this country and Belgium, we would also express a hope that a definite relation and active interchange of good offices may be soon established between the Pathological Society of London and the one which we now have introduced to the notice of our readers.

ART. VIII.—*The Climate of Brighton.* By WILLIAM KEBBELL, M.D., Physician to the County Hospital.—*London*, 1859. pp. 187.

THE days are fast disappearing in the mist of antiquity when to sit behind four spanking bays was one of the great delights of life, and when even baronets did not think it beneath their dignity to handle the ribands and to ask for the coachman's fee. The stage-coach still exists in certain districts into which a traveller but rarely penetrates, as we can testify from recent experience; but the celebrated whips and teams of old have disappeared from the Brighton road, and the only choice now is between an express or a slow train. We will not enter upon the inquiry as to the relative gain and loss accruing from the victory obtained by steam over horseflesh, but one thing is certain, that if Brighton has been brought nearer to London by the enormously increased facility of communication, this change has not been accompanied by a corresponding improvement in the sanitary condition of Brighton itself.

The gradual extension of the town, and the great increase of permanent residents, together with the means of rapid transit, make it appear, as indeed it is, a suburb of London. The health-giving seabreezes that tempt the weary Londoner to Brighton, or that induce him to send his family there during the autumn, remain the same as ever, but the necessary evils accompanying the growth of the population have not been met in that spirit of advancement which belongs to the present age of sanitary measures. Much as Nature has done for Brighton in respect of soil, of elevation, of air, of sea, competitors for public favour will start up and distance it, unless the authorities recognise the claims which may be duly made upon them, for securing a uniform and complete system of sewerage. Let them follow the excellent example set them by their neighbour, Worthing, which, after a lingering and sickly existence of many years, has started up with new vigour; and having called in a Hercules to cleanse its Augean stable, now offers to the phthisical, and strumous, and cachectic individual a place of recovery and of solace that may soon rival Ventnor and Bournemouth in one sense, as it has already eclipsed Brighton in another.

Dr. Kebbell, in the useful little book before us, admits the defects of Brighton candidly; but of course he dwells chiefly, and justly so, on the grounds that have given to the locality so high a place in public estimation; he shows us the natural advantages which it possesses, and proves them by statistics, as far as statistics can serve that purpose. The book is divided into three parts or chapters. The first treats of the circumstances determining and affecting climate generally; and after a brief summary of the climates of Great Britain, conducts the reader to the second chapter, in which the climates of Brighton in the four seasons are successively treated of. The third part is devoted chiefly to the consideration of the morbid conditions in which the Brighton climate is found beneficial. It contains much that will prove useful to the practitioner, and gives, on the whole, a correct picture of the sanatory and sanitary aspects of Brighton. London physicians would probably ask for more detailed information as to the different localities of Brighton, for there is probably no town of the same area which presents so many variations of climate, each possessing well-marked characters and influences. Local partialities and jealousies may perhaps interfere with the complete execution of this part of the undertaking. Still, as the book will doubtless reach a second edition, we would suggest the propriety of extending this part of the work; nor can we admit that the range of legitimate subjects is exhausted while ozonometry is excluded. The author is evidently himself a careful meteorologist, and would find no difficulty in adding observations on this point to those already given, and by this means increase our means of judging of the properties of the Brighton atmosphere in different localities. When the second edition appears, the author will doubtless carefully revise the proofs, and see that the proper names are always correctly spelt; we make this remark, because Sir James Clark's name, which occurs more frequently than any other, and is quoted as an authority, is invariably misspelt.

We venture to offer these suggestions because we have perused the book with profit and satisfaction, and think so well of it that we wish to see its value still further increased. In its present form it fully deserves the careful attention of the English practitioner, for it is painstaking and truthful, and does not possess a greater bias than is perfectly excusable in a denizen of a place endowed with such restorative powers as we must fain accord to the breezes that play on the chalk cliffs of Brighthelmstone.

ART. IX.—*Geschichte christlicher Krankenpflege und Pflegerschaften.* Von Dr. HEINRICH HAESER, Professor zu Greifswald.—*Berlin,* 1857. pp. 126.

History of Institutions and Associations for the Care of the Sick during the Christian Era. By Dr. H. HAESER, Professor at Greifswald.

ON the occasion of the fourth centenary jubilee of the University of Greifswald, Professor Haeser wrote, as is customary in Germany on such occasions, the official *programm*, a sort of congratulatory thesis, in the name of the medical faculty. The orthodox language to be used at such times is Latin; but as the subject of Dr. Haeser's essay excited a wider interest, the author has re-issued it in a larger and more complete form in the German language. We cannot more briefly express our opinion of the character of the book than by saying that we would wish to see it again translated; it merits an English dress, not only because it treats of a subject now exciting a peculiar interest among our countrymen and countrywomen, but because it is handled in a way worthy of the occasion and of the university from which it has issued. The history of any phase of human development is worthy of study; the tendencies displayed in the various branches of the healing art not less instructive when regarded in the historical point of view, than are the political struggles of mankind.

In the work before us Dr. Haeser, whom we introduced to our readers some time ago as the author of an important work on the history of medicine, first briefly adverts to the scanty provisions made in the pre-Christian era for the care of the sick, and then proceeds to show how Christianity called into life the deaconate and the hospital, and infused the spirit of association for the purpose of taking charge of the sick. The account of the various orders that from the time of the feudal ages have devoted themselves to the protection and treatment of the sick, is fraught with interest of an enduring kind; and although the necessity that called most of those orders into existence and maintained them has passed away, we may, in some of the survivors, mirror ourselves, and seek for a renewal of that spirit of devotion which has achieved great things in this field of Christian philanthropy, and may yet be resuscitated to new life.

Whether we prefer the *Beghuines*, or the association of the *Frères Hospitaliers Pontifes*, or the *Sœurs de Charité* of Vincent de Paul, it is difficult not to look with envy to the Roman Catholic Church which could again and again foster such excellent associations; and while availing itself of the enthusiasm of the founder, enlarge its own sphere by employing those agents which have the most ready access to the human heart. In this the Roman Church has indeed ever known how to combine the wisdom of the serpent with the simplicity of the dove. Let us hope that Protestantism may in England, as it has already so successfully done in Germany, realize the spirit of some of those noble institutions of nursing sisterhoods, which are indeed of a catholic character, but which have hitherto been limited to Roman Catholicism.

At present we would only invite attention to the very careful and detailed account drawn up by Dr. Haeser, and based upon authentic records. Having performed the ceremony of introduction, we must leave him to make friends for himself, until we can find a more fitting opportunity of dilating further on his merits, and of entering more fully into some of the questions involved in the whole subject of nursing.

ART. X.—*The Speaker at Home. Chapters on Extempore and Memoriter Speaking, Lecturing, and Reading Aloud.* By the Rev. J. J. HALCOMBE, M.A., late Scholar of Magdalen College, Cambridge. *And on the Physiology of Speech.* By W. H. STONE, M.A., M.D., F.R.C.S., L.R.C.P., late Scholar of Balliol College, Oxford, Medical Registrar to St. Thomas's Hospital.—*London*, 1859. pp. 184.

THERE is scarcely a single agent at our command by which, for good or for evil, we may more readily influence the society in which we live, than the power of speech; there is scarcely a feature in our education which occupies a less prominent position than the development of this power. We are taught to spell, and perhaps to read articulately, to mind our stops, and not to drop our h's, but how many schoolmasters attend to the paramount duty of instructing their pupils in the higher uses, not to say ornaments of language, and enable them at all times and under all circumstances to control, modify, and guide the organs of speech? Mr. Halcombe addresses himself chiefly to the clergy, with whom elocution is a part of their professional ministration, but his remarks are applicable to all, and we cannot but think that in our profession much good might be effected by inducing greater attention to the subjects discussed by Mr. Halcombe. In order to speak well it is necessary to have clear conceptions of what ought to be said; the matter being prepared, it by no means follows that the proper manner is a necessary sequel; it is well to know that even our greatest orators have not acquired their proficiency without much and protracted labour. We may say of the orator, *fit, non nascitur*. How much less, then, can those whose daily avocations do not force them to cultivate the power of speech, expect to possess that facility of diction which all covet, and yet so few take the pains to acquire? There is much declamation just now about the absence of political influence on the part of the medical profession. One element in securing power which our profession has especially neglected, lies in the art of speaking; those of us whose ambition takes that direction, must not shrink from the difficulties to be overcome, if we wish to control and influence multitudes by the intonation of our voice or the inflexions of our tongue. But it is chiefly to the young generation that we must look. In the prosecution of professional duties, few can find the time, if they have the inclination, to cultivate the art of speaking; at school and college the foundation must be laid, or the superstructure is not likely to prove either secure or beautiful. As an illustration of these remarks, we may quote Pitt, whose powers as an orator are described by Lord Brougham as having been magical—

> "His father, Lord Chatham, is said not only to have caused him continually to translate aloud the ancient classics into English, but to have been in the habit of making him declaim upon a given topic about which he had previously given him full and accurate ideas. The powers thus acquired enabled him to bring at once to bear upon any question all the varied resources with which nature and education had gifted him."

The consequence of this training was, that being unexpectedly called upon in Parliament, his maiden speech, though entirely without preparation, was completely successful, and he was thus enabled to do at the age of twenty-two what much older statesmen have failed to accomplish, unless with the aid of careful preparation.

The advice given by Mr. Halcombe, and the rules he lays down, are applicable to any persons, young or old, who are desirous of speaking or reading well. The book is an unpretending, but satisfactory production, which conveys much excellent instruction in a very pleasant manner; it addresses itself to a much wider circle of readers than one might gather from the title, unless the word speaker be interpreted—as in this case it should be—everybody who speaks, or wishes to speak, good intelligible English. To that large class we recommend it.

ART. XI.—*An Expository Lexicon of the Terms, Ancient and Modern, in Medical and General Science, including a complete Medical and Medico-legal Vocabulary.* By R. G. MAYNE, M.D. Part IX.—*London*, 1859.

THIS valuable work is now drawing to a close, the ninth number bringing the main body of the Dictionary down to the last letter of the alphabet. A considerable number of addenda are given in this part, and as they only belong to letter A, it is to be feared that the appendix will almost rival the body of the work in size. We will hope, for the author's sake, that his first edition may prove so acceptable to medical men, that a second may soon enable him to place these waifs and strays in their proper places. In a work of this kind it is unavoidable that omissions should occur which are discovered after it is impossible to rectify them, except in the manner adopted by Dr. Mayne.

The Dictionary, as we have repeatedly pointed out, contains a large amount of information, well arranged, and very accessible; and we have little doubt that it will soon come to be regarded as a necessary appendage to the library table of every man of science and scientific dilettante.

ART. XII.—*Contributions to Midwifery, and Diseases of Women and Children; with a Report on the Progress of Obstetrics, and Uterine and Infantile Pathology in* 1858. By E. NOEGGERATH, M.D., and A. JACOBI, M.D.—*New York*, 1859. pp. 466.

THE Contributions in this volume consist of seven essays, which we notice seriatim—

1. "Three cases of induction of premature labour after Cohen's method." Dr. Noeggerath advocates the induction of premature labour by the method known as that of Dr. Cohen, of Hamburgh, and which consists in the injection of water into the uterine cavity by the means of a catheter inserted between the membranes and the inner surface of the uterus. At the same time Dr. Noeggerath discusses other methods, and by the comparison of the results, establishes to his own satisfaction the conclusion that Dr. Cohen's method is the safest and most efficient with reference to both mother and child. It is a method that has not been generally employed by English obstetricians, but has found great favour among our German brethren. A report of its successful employment in a case of distorted pelvis was given to the Obstetric Society of London by Dr. Trouncer, in October of the present year. We have referred to the monograph of Dr. Krause, of Dorpat ('Die Künstliche Frühgeburt'), in which, from the details of cases to be found scattered throughout British and Continental medical literature, Dr. Krause concludes, as does Dr. Noeggerath, in favour of Dr. Cohen's method.

2. "Four cases of injection of a caustic solution into the cavity of the womb, illustrative of the advantages and dangers connected with

this proceeding," are sufficiently conclusive in their results to deter any than a very rash man from adopting such measures after their perusal. Out of four cases, two exhibited alarming symptoms, and one resulted in death; and yet Dr. Noeggerath could pen such a sentence as the following, immediately after the statement of such results—"The question, whether caustic injections into the womb are connected with dangers or not, seems to me unsettled." (!) What amount of mortality would Dr. Noeggerath deem requisite to settle this question?

3. "On the employment of pessaries." This essay recommends Zwanck's pessary (or Eulenburg's Hysterophor), a modification of which by Dr. Savage, of London, presents improvements.

4. "On the parthogenesis of uterine polypi," in which, from the occurrence of a *placenta succenturiata*, rapidly developed into apolypoid tumour, the writer infers that light is thrown upon the origin of uterine polypi.

5. "A case of invagination of the colon descendens in an infant, with repeated hæmorrhages in the *colon transversum*," by Dr. Jacobi, is interesting from the rarity with which it is met with in medical literature.

6. In this essay, Dr. Jacobi very strongly recommends the employment of oxysulphuret of antimony in inflammatory diseases of the infantile respiratory organs—in "inflammations of the larynx, trachea, bronchi, bronchia (?) and lungs"—"after the inflammatory fever is removed, and the disease has reached its highest development." Its benefits, Dr. Jacobi states, are to be obtained by giving it in full doses—e.g., a grain and a half to three grains repeatedly. At a later period of the disease, the writer remarks that the oxysulphuret may be combined with iron, quinine, &c. It occurs to ourselves, that at the particular stage of the disease indicated by Dr. Jacobi as suitable for the administration of this remedy, the patient is in a fair way for recovery, and that the virtues of the medicine are somewhat masked by the advances of health.

7. "On the etiological and prognostic importance of the premature closure of the fontanels and sutures of the infantile cranium." Dr. Jacobi shows, from the researches of Eichmann and others, that in well-developed children the large fontanel is closed at or before thirteen months of age, and that it is open at the same period of life, or later, in a large majority of badly developed children; and that as the size and form of the cranium result from the process of ossification at the sutures, pathological conditions will follow on their premature closure. Dr. Jacobi, however, adds that this law is not without limitations, and quotes the statement of Huschke, that the cranium increases in size up to the sixtieth year. The brain, however, the writer repeats, does not increase in size after the completion of the ossification of the fontanel and sutures, and he is of opinion that the increase of the cranium is effected by the absorption of the osseous substance of the inner surface and its reproduction on the exterior. "The intellectual faculties, and those of locomotion and sensibility," are injured by premature closure of the sutures, and consequent diminution of the

cranial cavity. Idiocy if life extends beyond infancy, cerebral affections of a similar character to those of inflammatory diseases, and a condition that has often been spoken of as hypertrophy of the brain, are the forms in which the effects of this abnormal condition of the bones have been manifested. The condition of cranium here pointed out by Dr. Jacobi has been very little noticed by pathologists, but we have ourselves seen examples thereof.

The latter portion of this work, the "Report on Obstetrics," although comprising three-fourths of the entire volume, it will not be necessary that we dwell upon, beyond stating that it presents a very full summary of all that has lately appeared in various periodicals, and other recent works upon obstetrics, and the diseases of women and children. Dr. Barnes's periodical Reports upon the same subjects will not have left our readers uninformed of any important communications.

ART. XIII.—*A Digest of the Vital Statistics of the European and Native Armies of India.* By JOSEPH EWART, M.D., Bengal Medical Service.—*London*, 1859. 8vo. pp. 182.

WE are glad to find the medical officers of our Indian army availing themselves of the numerous opportunities within their reach for studying the causes of the diseases prevalent in the tropics and their mode of prevention. We had lately occasion to advert to the works of Dr. Jeffreys and Dr. Norman Chevers,* to which the volume before us constitutes a most important addition. Dr. Ewart goes over some of the ground already traversed by Dr. Chevers, and adds a vast amount of original and very valuable matter, which must have an important bearing upon the future management of our troops in India. The great mortality to which at present they are liable, is illustrated by the following startling announcement—

"The European army has hitherto *disappeared*

In Bengal in about every		10½	years.
In Bombay	„	13¼	„
In Madras	„	17	„
In all India	„	13½	„ "

Dr. Ewart agrees with Dr. Chevers in assigning to the Bengal Presidency the highest rate of mortality; but he accounts for this by the greater abundance and virulence of malarious exhalations in the Gangetic Presidency, rather than by the greater exposure of the troops to the dangers and vicissitudes of war, and by the frequent postings of corps in new stations. Probably all of these causes operate. It is gratifying to find that of late years a gradual and progressive diminution of the rate of mortality has been taking place, and we cordially agree with the author when he expresses his belief that a much greater diminution might be effected by an increased attention to sanitary measures.

After considering the general mortality among our European troops

* British and Foreign Medico-Chirurgical Review, Jan. 1859, p. 110.

in India, Dr. Ewart proceeds to take up the individual diseases by which this great mortality is occasioned. Fevers, diarrhœa and dysentery, hepatitis, cholera, phthisis, and "other diseases," are successively dwelt upon. According to the author, there is every reason to believe that the remarkable influence of malaria upon the mortality amongst Europeans is not confined simply to fevers and bowel complaints, and that the cachexia produced by its inroads upon the nutritive and blood-making functions is a most powerful predisponent cause to the majority of other diseases; it is his conviction that, "*provided the sources of malaria were dried up*," the average standard of health of the European race in India would bear comparison with that of any race upon the face of the civilized world. Until this can be effected, he advocates strongly the use among our troops at malarious stations, of small doses of quinine as a prophylactic, and endeavours to show that the expenditure in quinine would be far more than counterbalanced by the prevention of sickness and the saving of human life.

From among the many important facts elucidated by Dr. Ewart's inquiries, we select one which is well deserving of further investigation. While statistics reveal a remarkable diminution of late years in the mortality from fevers, bowel complaints, and hepatitis, a diminution which the author is inclined to attribute to the substitution in treatment of quinine and tonics for the lancet and mercurialization, a reverse picture is presented by cholera. Statistics demonstrate a decided decrement in the frequency of this disease amongst our European troops, but a very remarkable augmentation in the ratio of deaths to attacks in all the three Presidencies. Thus, in Bengal, from 1818 to 1835, the rate of mortality was 26·36 per cent., while from 1836 to 1853 it was 39·75 per cent.; in Bombay, from 1818 to 1835, it was 21·64 per cent., and from 1836 to 1853, 41·11 per cent.; and in Madras, from 1829 to 1838 it was 27·1 per cent., and from 1842 to 1851, 50·2 per cent.

ART. XIV.—*Summary of recent Publications.*

THE past quarter has not brought us as many new works of importance as we expect at this ordinarily most prolific period of authors. The great majority of the books before us are new editions of more or less known works. Among the new births, the one that will probably command most attention is Dr. Garrod's 'Nature and Treatment of Gout and Rheumatic Gout,' a volume replete with interest, full of valuable facts, and abounding in instructive argument and illustration. We hope soon to speak with equal confidence of the value of Dr. Austin Flint's work on 'Diseases of the Heart,' which, with the same author's volume on 'Diseases of the Respiratory Organs,' we shall, if the Fates are propitious, report upon more fully in our next. A work on 'General Paralysis,' by Mr. Austin, does not afford much light on this intricate subject; but why Mr. Charles Jones Humphreys should have published his lucubrations, entitled, 'Phthisis Pulmonalis, and Tubercular Phthisis,' except under the influence of the *cacoethes scribendi*, it were difficult to say. Our readers will be glad to receive a second edition of Dr. Hughes Bennett's 'Pathology and Treatment of Pulmonary Consumption;' and the profession will hail with pleasure the fourth edition of Dr. West's classical work on the 'Diseases of Infancy.' Dr. Edwin Lee's works on 'Homœopathy and Hydropathy,' and on 'The Watering-places of England,' each come before us in a fourth edition. A small volume, with the attractive title, 'Blood Disease,' by Dr. Vaughan Hughes, is before us, as well as a reprint of the Old Sydenham Society's 'Epidemics of the Middle Ages,' by Hecker, with the addition of a most interesting account of the strange phenomena manifested in the pilgrimages or crusades of children in the beginning of the thirteenth century. Mr. Lobb's little book on the 'Treatment of Diseases of the Nervous System by Galvanism,' has reached a second edition. Dr. Beale favours us with a series of plates intended to illustrate his work, entitled, 'How to Work with the Microscope;' and from the same laborious author we receive a continuation of the 'Archives of Medicine,' which completes the first volume of this serial. Papers, by Dr. William Moore, on 'Infantile Mortality,' and on 'Small-pox and Vaccination Statistics,' with a Swedish inaugural treatise on 'Uremia,' by Dr. Troilius, conclude our list of medical works, *sensu strictiore.*

In surgery, besides the works already discussed in the earlier pages of this number, we have to introduce to our readers a neat illustrated 'Manual of Operative Surgery on the Dead Body,' by Thomas Smith, F.R.C.S., to which we shall return at an early opportunity. Mr. Martin has issued 'Illustrations of the Use of the Ophthalmoscope,' in which he passes in review the various forms of eye disease the knowledge of which has been enlarged by the employment of this instrument. The advantages already gained by the ophthalmoscope are considerable, and Mr. Martin is of opinion that its careful and assiduous employment will yield as much success in the diagnosis and treatment of diseases of the eye as we have already obtained by the aid of auscultation and percussion in thoracic affections. It is not a year since we announced a third edition of Mr. Hunt's duodecimo, 'Guide to the Treatment of Diseases of the Skin;' we now have to introduce to the profession a fourth edition, containing two additional chapters on the vegetable parasites of the skin, and on metastasis in its practical bearings. Mr. Yearsley's book on 'Diseases of the Throat' is before us in the form of a seventh edition.

In anatomy and physiology we have received valuable contributions from Mr. Lockhart Clarke, Mr. Liston, and Dr. Davy. The first continues his researches into the minute structure of the brain, and forwards a first series, in which the microscopy of the medulla oblongata is investigated and illustrated. Mr. Liston's contributions to physiology and pathology (in 'Philosophical Transactions'), contain three papers: 1, "On the Parts of the Nervous System regulating the Contractions of the Arteries;" 2, "On the Cutaneous Pigmentary System of the Frog;" and 3, "On the Early Stages of Inflammation." Dr. Davy investigates by careful experiments the electric condition of the egg of the common fowl.

From America we have received the second edition of Parrish's 'Introduction to Practical Pharmacy,' largely illustrated by woodcuts of the apparatus used by the pharmaceutical chemist: in which, as far as we can judge from a cursory inspection, the preparation of the most recent additions to the materia medica receives attention, as well as that of the established drugs. With this work we may mention the appearance of a fifth edition of Mr. Bullock's translation of the 'System of Instruction in Qualitative Chemical Analysis,' by Dr. Fresenius; which, as we are informed by the editor, is much in advance of the latest German edition of the work, as it "contains a large amount of original matter, communicated by the author during the time it (the English translation) was passing through the press."

Not long since we introduced to the notice of our readers Mr. Grindon's 'Manchester Walks and Wildflowers;' we now have to notice a larger similar work by the same author, entitled, 'The Manchester Flora,' which will doubtless prove very acceptable to those denizens of the cotton metropolis who can find leisure to indulge in botanical recreations.

Mr. Kirwan embodies the results of his experience in nine voyages with troops, as to the means suitable for the promotion of the soldier's comfort and health on board ship, in 'Notes on the Dispatch of Troops by Sea.' As this is essentially a sanitary matter, we may couple with Mr. Kirwan's Notes Miss Baines' paper 'On the Practice of Hiring Wet-nurses,' which was read at the meeting of the National Association for the Promotion of Social Science, held at Bradford in October, 1859. We may allude at the same time to Dr. Tripe's 'Third Annual Report of the Board of Works for the Hackney District.'

We have to advert to the 'Medical Reports' for 1856 and 1857 of the Wieden Hospital of Vienna, which are almost entirely of a statistical character. We hope shortly to lay before our readers the points that may be of interest to them in Dr. Mouat's valuable 'Report on the Jails of the Lower Provinces of the Bengal Presidency in 1858-59,' and we conclude our summary by introducing to our readers two introductory addresses by Dr. Collingwood and Dr. Russell Reynolds, the former of whom discusses the influence of the microscope upon the progressive advance of medicine, while the latter presents us with a discourse on 'The Facts and Laws of Life.'

PART THIRD.

Original Communications.

Art. I.

Contributions to the Pathology of Diphtheritic Sore throat, and other kindred Affections. By John Burdon Sanderson, M.D., Member of the Royal College of Physicians, Assistant-Physician to the Hospital for Consumption at Brompton, Officer of Health for Paddington, &c.

M. Bretonneau in the first page of his treatise on 'Diphthérite,' in drawing a distinction between ordinary inflammation as affecting the mucous membrane, and the diphtheritic inflammation, is unable to account for or explain the existence of a membranous exudation in the one case, and its absence in the other, in any other way than by attributing it to what he calls a special "mode inflammatoire," or special modification of the inflammatory process. So long as pathologists were contented with looking at inflammation from the point of view of its absolute characters, without attempting to disclose the mystery of those vital processes of which they are the mere indices, a vague expression of this kind was the utmost that could be arrived at by way of explanation. Now, however, that we not only study the absolute characters of disease with much more exactitude than our fathers, but have learnt to regard all morbid processes as mere perversions of the functions of the affected parts, as taught us by an advanced anatomy and physiology, we are able to dispense with these makeshifts, and in most cases to connect observed differences in the signs of disease, with corresponding differences either in the seat or nature of the changes on which they depend. Thus, when we find that the mucous membrane of the pharynx and fauces is subject to several conditions of acute disease, to all of which the term inflammation in its ordinary signification is applicable, however different some of the phenomena which express them, we are entitled to assume that similar differences exist either in the structures affected, the order in which they are affected, or the nature of the change to which they are subject. Now, as it is evident that the word *inflammation* only includes in its meaning the four characters, pain, heat, redness, and swelling, and as structural differences cannot be accounted for by any possible variation in the relations between these elements, it is clear that in the question before us that word is meaningless, and may be entirely dispensed with. Modern pathology has taught us that the essential change which is common to all those processes which are accompanied with the signs of inflammation, as well as to others to which those signs are wanting (as, e.g., the deposition of tubercle), is *exudation*, which consists in a perversion of the function of the living part, so that the liquid plasma which the part in its natural condition uses for the purposes of its own life, is diverted into another channel.

In order to the existence of inflammation, a network of capillaries is necessary; for one of its elements—redness—is expressive of a change in the condition of those vessels; but exudation may have its seat in any living structure, for it is a perversion of a function essential to life. In whatever direction the stream of plasma normally passes from the blood to the living cell or fibre, it may, by perversion from that course, give rise to fibrinous or albuminoid concretions, or serve as the germ fluid of a morbid growth, so that although it is utterly meaningless to draw any distinction whatever between the inflammation of the epithelium and inflammation of the sub-mucous tissue, both being, in fact, unmeaning expressions, we can speak of either separately as being the seat of exudation. Between every cell, or series of cells, or other structural elements, and the blood, exchanges of material are in constant operation, which in each instance form a system as special and distinct as the structure itself; and each such system is capable of being independently perverted, and so giving rise to a separate morbid process. So that whenever we have to do with a plurality of structures distinct and unconvertible into each other, we may presume that we are likely to have as many varieties of exudation.

2. *Preliminary anatomical considerations.*—The mucous membrane, whether digestive or respiratory, consists of two distinct structures—viz., the *membrana propria*, or submucous tissue, and the *epithelium*. As these structures differ very materially in the mouth, pharynx, and air-passages, in their relations to each other, and as these differences are important pathologically, it becomes necessary to remind the reader of some of them. In the mouth, the membrana propria is from 0·01 to 0·015 inch in thickness, but at some parts it is much thicker, as in the gums, soft palate, and uvula. It consists of a dense felt of white fibres so close together that they can scarcely be distinguished as such. The density of the structure is greatest at its superficial aspect, where its fibrous constitution is so completely indistinguishable that it appears under the microscope perfectly homogeneous. But on examining the deeper layers, especially after the addition of acetic acid, the most marked characteristic of the structure attracts the attention of the observer—viz., the highly refractive minute nuclear fibres which form an intricate network throughout its substance. There is no difference whatever between the membrana propria of the pharynx and larynx and that of the mouth, excepting that both the former contain a larger proportion of the reticulated elastic fibres above described.

The epithelium of the mouth and that of the air-passages, on the other hand, differ widely; the one resembling more or less the structure of the skin, the other having a structure peculiar to itself. Immediately on the free surface of the membrana propria of the mouth and pharynx, rest several layers of minute vesicles about the size of the colourless blood-corpuscle. Other ranges of cells are superimposed upon these, which are larger, more or less compressed, and many-sided; and in pursuing the examination towards the surface, we arrive finally at the well-known epithelial scales, in which simultaneous attenuation and extension have reached their utmost limits, the whole forming a resistant structure of about the same thickness as the membrana propria. Each of the minute cells next the membrana propria is furnished with a vesicular nucleus, a little smaller than itself, which remains until the cell has attained its complete development, but gradually disappears during its transformation into the epithelial scale.

In comparing with this structure the epithelium of the larynx, the most marked difference which presents itself lies in the comparative tenuity of the latter, of which the thickness is not more than one-tenth of that of the former. It consists of four or five layers, which differ from each other in the form of the cells which compose them. Those next the membrana propria resemble exactly the similarly-placed cells in the epithelium of the mouth, but their transformation in the more superficial layers differs entirely. Instead of being compressed in a direction vertical to the mucous surface, they are extended, and finally transformed into the well-known club-shaped bodies covered with cilia on their truncated ends.

Immediately below the membrana propria, in both cavities, is the sub-mucous cellular tissue, varying extremely in quantity and thickness, and in some parts of the cavity of the mouth (gums and hard palate) entirely absent.

Having, then, to do with two distinct structures in the mucous membrane proper, and a third in intimate relation with it—viz., the sub-mucous cellular tissue, we should expect to be able to distinguish three corresponding series of pathological results of exudation. And to the fundamental distinctions thus derived from the seat of the morbid process, others subordinate would arise as to its nature. Thus, a process of exudation existing in any of the three structures might be albuminous merely, or albuminous and fibrinous; or in other words, the exuded liquid plasma might or might not have the property of spontaneous concretion, which is the sole fact by which fibrine is distinguishable from the other albuminous compounds.

Let us inquire whether these distinctions, assumed *à priori* from an anatomical point of view, correspond with observation. As regards the alterations of the cellular tissue, I shall only refer to them for the purpose of distinction, and in so far as they are associated with those which form the subject of this paper. The diseases which depend on them are well known and perfectly understood. The exudation of liquid plasma, first albuminous, ultimately fibrinous, in the cellular tissue of the fauces, constitutes the ordinary forms of tonsillitis and pharyngitis; a similar infiltration affecting the larynx gives rise to the more dangerous, though much less intense degree of swelling of the wall of the laryngeal cavity which constitutes the leading anatomical character of simple laryngitis.

3. *Preliminary pathological considerations.*—Physiological and pathological considerations alike point to the conclusion that every morbid process affecting the mucous membrane takes its origin at the free surface of the membrana propria—the surface which separates that structure from the epithelium. It is there that the organic life of the tissue is specially localized; for however little anatomical foundation there may be for asserting the existence of a germinal or basement membrane, it is not less certain that the development and growth of cells go on at that surface with extraordinary activity. It is in the deep layer of the epithelium, therefore, and in the membrana propria itself, that we should look for the earliest traces of exudation; the deeper as well as the more superficial structures being affected secondarily or consequently.

In the purely catarrhal or, as it is more properly called, erythematous inflammation, the nature of which may be said to be perfectly understood, the obvious phenomena are a tumid condition of the mucous membrane (as shown in the nares by the feeling of obstruction which precedes a common cold), combined with undue dryness of the epithelial surface. The first is produced directly by the infiltration of the affected tissue; the second indirectly by the consequent interference with its capability, whether physical or vital, of transmitting liquids from the capillaries to the surface. This early stage of the erythematous inflammation soon gives way to a second, in which an abundant liquid, wholly wanting in structural elements, exhales from the membrane, this being accompanied with diminution of the previously existing swelling. Throughout the whole process there is bright redness of the affected part, on which account the term erythematous is more applicable to it than any other. The whole is an example of a simple *albuminous* exudation, the effects of which are limited to the immediate neighbourhood of its surface of origin.

Between this and the more intense degrees of albuminous exudation the differences are dependent partly on the secondary affection of the subjacent tissues, and partly on an abnormal development of the epithelium. The first of these consists in the infiltration of the cellular tissue with liquid plasma, as already described; the second in the increased vegetative activity of the epithelial cell, which, instead of undergoing its usual transformation into a scale, retains its spheroidal form, and is thrown off as a so-called mucous corpuscle, the mucous surface, instead of exuding a clear, colourless fluid, becoming bathed with a whitish or whitish-yellow mucus, which, as the process advances, becomes more and more tenacious.

Of the concrete results of exudation affecting the mucous membrane, we have to distinguish two kinds: *a*. The true fibrinous *coagulum*, which results from the concretion of a liquid plasma containing fibrine: *b*. Granular or amorphous material deposited interstitially or on the surface of the epithelium. Both of these are commonly called fibrinous, but, strictly speaking, it is incorrect to apply the term fibrine to anything except the product of a coagulation analogous to that which takes place in the liquor sanguinis. According to many pathologists, the granular matter which is interstitially deposited in the meshes of the membrana propria and other similar structures (as, e.g., in the urinary tubules of the kidney), is to be regarded as a concrete *dêbris* arising from the disintegration of the tissue, indicative rather of deficient absorption than redundant deposition. On the other hand, as it is known to occur in membranes which are in the act of *transmitting* fibrinous plasma (and here we may again cite the example of the kidney), it is not unreasonable to suppose that it is deposited by a process resembling coagulation in the interstices of the transmitted membrane. In either case it is a consequence of abnormal nutrition having its seat at the surface of the membrana propria.

Concrete exudations, either on its surface or in its substance, form the essential lesions of the mucous membrane in the following pathological states—viz. (*a*) Superficial sloughing of the mucous membrane; (*b*) Superficial true croupous concretion; and (*c*) Superficial granular concretion.

4. *Superficial sloughing of the mucous membrane* (Diphthérite, according to Virchow and Rokitansky).—There are certain cases (apparently much more common in Germany than they are in this country) to which the term diphtheria is applied by Virchow and Rokitansky, but which differ entirely in their pathological results from that disease as defined by Bretonneau. Professor Friedreich, of Heidelberg, thus describes the lesions characteristic of diphtheritic laryngitis:*

"Gray-white or yellowish-white masses are formed in the superficial layers of the inflamed mucous membrane; these take the place of the structure of the mucous tissue in the affected parts, and consist of a granular amorphous substance resembling detritus. As, therefore, the whitish layers actually involve the mucous tissue, they cannot be removed from the latter without loss of substance, and if during life the diphtheritic deposit disappears, there is always loss of substance—that is, ulceration of the mucous membrane is established, which is rendered liable to increase in depth and circumference by new diphtheritic deposits. We have thus in diphtheritis an inflammation accompanied with partial destruction and necrosis of the mucous membrane, which has been rightly distinguished, on account of its malignant character, from fibrinous laryngitis."

Friedreich speaks of this affection as occurring for the most part secondarily, with or without croupous exudation on the surface, after measles, scarlatina, or small-pox.†

Rokitansky describes similar lesions. Diphthérite, according to him, is characterized by "an infiltration of the mucous membrane, with exudation and sloughing (*Verschorfung*) of the same; that is, it consists in a mortification of the membrane, which becomes a whitish, yellowish-brown, greenish-brown, friable, or tenacious sphacelus (*Brandschorfe*) suffused with blood."‡ It need scarcely be observed that the anatomical changes here described differ entirely from those

* Handbuch der speciellen Pathologie und Therapie. Red. von Rud. Virchow. Erlangen, 1858. Band v. L. 3, s. 429.

† Ibid., p. 432.

‡ Rokitansky: Lehrbuch der path. Anatomie, Band ii. s. 43.

which characterize diphtheria. The following case, which has recently come under my observation, exemplifies the distinction in a marked manner:

T. J., aged thirteen months, an ill-conditioned infant, of markedly strumous appearance, had an attack of measles about the 4th of November last. He was ill a week, but no medical aid was sought; he recovered, but continued to cough until November 19th, when he was first observed to have difficulty in swallowing and dyspnœa. These, however, attracted no attention until the day before I saw him. The child was placed under my care (November 25th) only eleven hours before his death. He then exhibited in an exquisite degree the phenomena of the croupal agony; the back was arched, the head thrown backwards, or tossed rapidly from side to side, and the limbs in violent movement; the laryngeal inspiration-sound was loud, sometimes moist, sometimes dry, but invariably sharp; the voice and cough were feeble, but of the same sharp tone. The parents stated that these characters had existed for one day only.

Autopsy, thirty hours after death. — Extreme emaciation; deep ulceration of each of the angles of the mouth, otherwise buccal mucous membrane healthy; an irregular patch of exudation about 0·01 inch in thickness, white and opaque, and having raised edges, on the posterior surface of the left half of the velum; abrasion and patchy redness of the mucous membrane in the neighbourhood; the pharyngeal mucous membrane generally redder than natural, but free from appreciable lesion.

On opening the laryngeal cavity, its mucous membrane was seen to be covered, from the inferior margin of the epiglottis to the second ring of the trachea with a pultaceous fluid, on removing which, and allowing a stream of water to pass over the surface, the following appearances were observed:—On the inferior aspect of the margin of the epiglottis and aryteno-epiglottidean folds, the mucous membrane, though smooth, was perfectly opaque, and of a white colour. This condition extended on each side over the mucous membrane covering the arytenoid cartilages, where it was bounded by a distinctly raised margin of irregular outline. At and below the level of the cricoid cartilage the mucous membrane appeared healthy; above this it was white and opaque, but the opacity was not limited by so distinct a raised edge as that just described. Between the portions of the mucous membrane thus altered, that structure was irregularly ulcerated over an area extending for about one-third of an inch above and below the vocal cords. The ulcerated surface was very uneven, and more or less covered with semi-detached shreds or flakes of altered mucous tissue. The mucous membrane of the trachea at its lower end was pale and of natural appearance. In tracing it downwards into the bronchial tubes, it became gradually more and more congested. The condition of the lung was that which is commonly found in the bronchitis which succeeds measles. There was emphysema of the anterior margins and apices of both lungs, a condition of their bases varying between collapse and congestion, the collapsed or congested lobules adjoining others exhibiting various degrees of emphysema; extreme congestion of the smaller bronchial tubes, especially of those leading to the congested lobules. In one situation the bronchial mucous membrane exhibited white patches of opaque thickening.

On microscopic examination the white and opaque pellicle, from the aryteno-epiglottidean folds, was found to contain no structural elements, excepting those belonging to the mucous membrane itself. Its superficial layers consisted entirely of epithelial cells, the outlines of which were rendered indistinguishable by opaque interstitial material, but on the addition of acetic acid became clearly visible, and were found on comparison to correspond completely with those of the adjoining healthy membrane. In the deeper parts there were no epithelial elements, but the contorted and branched elastic fibres of the membrana propria could be distinguished; thus showing that the morbid change consisted in an interstitial deposit, not in a concretion on the surface. The examination of the pultaceous fluid elicited the fact that it contained no new corpuscular elements whatever, nor any structure, excepting here and there deformed epithelium cells. There floated in it numerous shreds of a granular material, which exhibited under the microscope no trace of fibrillation. On the addition of acetic acid the outlines of epithelium cells could be generally indistinctly made out. In the material which formed the ulcerated surface, bundles of white fibrous tissue and contorted elastic nuclear fibres were met with, both of these structures being obscured by granular infiltration, but no epithelial elements could be discovered.

The kidneys were found on microscopic examination to be absolutely healthy.

The phenomena which presented themselves during life in the above case, and the circumstances under which it occurred, correspond so closely with those which belong to diphtheritic croup consecutive on measles, that we should be inclined to admit a very close relation between them, notwithstanding the facts above related, and the absence of all symptoms of affection of the kidneys.

Similar lesions to those we have been describing present themselves in the mucous membrane of the mouth and pharynx. The epidemic disease known as *fégar*, *stomacace*, or *stomatyphus*, although frequently presenting the characters of a truly diphtheritic affection, is not less dependent on interstitial granular infiltration of the substance of the mucous membrane. The prominent result of this disease is extensive sloughing of that structure, distinguished from the ordinary gangrene of the mouth by its superficial character and much greater extent; and anatomical investigation has shown that this sloughing, although frequently associated with, and consecutive to, a diphtheritic concretion covering the affected surface, is in reality dependent upon a granular infiltration of the mucous tissue. I shall endeavour to show that a similar condition of the pharyngeal mucous membrane exists, in conjunction with superficial concretion, in the worst forms of faucial or pharyngeal diphtheria; and that there is reason for believing that it constitutes the characteristic lesion of some epidemics of malignant sore-throat.

5. *True croupous concretion* — i.e., *coagulation of liquid plasma on the surface of the mucous membrane.* (Diphthérite, according to Bretonneau.) — The consideration of this process includes—1, the characters of the act of coagulation; and 2, the transformation of the resulting coagulum, and its disintegration at its free surface, by the formation of cells in its substance.

Fibrinous liquid plasma, when effused into a cavity, the walls of which are formed by living structures, necessarily spontaneously undergoes changes which are completely analogous to the coagulation of the liquor sanguinis. The solidifying fibrine forms a meshwork, the trabeculæ of which at first include the whole bulk of the coagulating fluid. By the contraction of its fibrils the coagulum gradually diminishes in volume, while it increases in solidity and firmness, at first being gelatinous, subsequently becoming a concrete mass (the consistence of which varies according to the degree to which contraction takes place), which upon contact with living tissues is transformed into white fibrous tissue. The same liquid when exuded slowly from a free surface undergoes changes of which those above described are the type, although modified very considerably by the circumstances under which coagulation takes place. Every true fibrinous concretion is in its earliest stage gelatinous, and in this form is seen in many cases of diphtheria, sometimes as a thin glazing over the surface, sometimes constituting a layer of considerable thickness, in which the structural character of the coagulum can be satisfactorily studied, as in the following instance.

E. C., a little girl, aged eight, first came under my observation on Tuesday, September 6th. Sore throat had existed for five days, and exudation had appeared the day before I saw her, and covered the whole of the left tonsil, the uvula, and velum. It could be easily removed, and left the mucous surface entire. Its consistence varied extremely. For the most part it possessed considerable toughness and density; in other parts, however, evidently of recent formation, it was of great thickness, but of gelatinous consistence. These exhibited on microscopical examination, a reticulum of distinct but extremely delicate fibrils, the arrangement and appearance of which were wholly indistinguishable from those of the colourless gelatinous coagula found in the cavities of the heart. They contained in general no corpuscular elements. In the earlier portions of the concretion the same appearances presented themselves; the fibrils, however, were stronger and more distinct (as shown in Fig. 1); and in some parts the development of corpuscular elements had commenced.

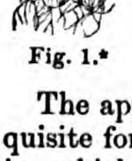


Fig. 1.*

The appearances just described are characteristic of the most exquisite forms of diphtheria, as defined by Bretonneau, those, namely, in which the concretion is most rapidly developed, and shows a marked tendency to increase. All true fibrinous concretions, what-

* **Fibrinous reticulum from a rapidly extending pharyngeal concretion in a child aged six.**

ever be their consistence, agree in exhibiting the appearance of an intricate reticulum of interlacing fibres, which varies but little in different cases, excepting in so far as it is obscured by the subsequent interstitial formation of exudation cells. Whether such concretion be obtained from the larynx or fauces, it is equally easy, especially at the margins of the preparation, to distinguish this appearance. Generally speaking the fibres exhibit in their arrangement no special direction; to this there are, however, some exceptions. Thus, in the larynx, I have observed a general tendency of the fibrillation in the direction of the axis of the organ. (Fig. 2.) Again, concretions often exhibit at their growing margins a worn appearance, as if they were bevelled off. On examining these attenuated edges, I have found that the fibrine always exhibits an aspect which is well represented in Fig. 4, in which the direction of the fibrillations is so uniform that the whole much resembles white fibrous tissue. As this appearance presents itself in the newest portion of the membrane, it is impossible to imagine that it is indicative of the transformation of the fibrine into this structure. Indeed, it is difficult to understand how the diphtheritic concretion should undergo this transformation at all. In one instance, however, I have observed appearances (Figs. 5 and 6) which do not admit of any other explanation—viz., the occurrence in a firm concretion derived from the fauces, of spindle-shaped fibre cells and nuclear fibres, resembling closely those which I have myself studied in the transformation of the blood coagulum.§

Fig. 2.*

Fig. 3. †

Fig. 4.‡

Diphtheritic concretions, if not detached gradually, waste at their free surfaces. This wasting depends on the disintegration of their substance by the development therein of exudation corpuscles, which takes place first in the older, that is, the superficial layers of the concretion. Hence we invariably find, that while in the neighbourhood of its applied surface, its substance consists entirely of fibrine, cells or their nuclei are met with in abundance at the free surface. (Fig. 3.) These cells constitute, in fact, the mechanism by which the disintegration of the solid fibrine is brought about, which in this manner softens into a pultaceous liquid, having rather the character of pus than of mucus.

Figs. 5 and 6.‖

With the view of determining how far the fact of fibrinous concretion may be considered a consequence of the anatomical or physiological peculiarities of the structure affected,

* Fibrinous reticulum, from a laryngeal concretion in a child aged twelve.

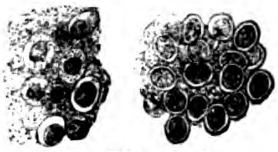
† Material from the free surface of the same concretion, exhibiting the nuclei of exudation cells embedded in a fibrinous substratum.

‡ Natural attenuated margin of a bronchial fibrinous concretion.

§ On the Metamorphosis of the Blood Corpuscles in Extravasated Blood. Edin. 1851.

‖ Fusiform cystoid bodies and nuclear fibres from a fibrinous concretion removed from the pharynx of a child aged eight.

independently of the constitutional state, or of the morbid condition of the blood arising from the disease, I made the following experiments: I injected into the air-passages of several dogs small quantities of a solution of cantharides in olive oil, and examined the consequent alterations of the mucous membrane after various periods. As I propose to recur to these experiments at another part of this paper, I will only describe the results generally here. Two hours after the introduction of the solution it was found that the mucous surface of the larynx was scattered over with patches, scarcely perceptibly redder than the surrounding membrane, and that that structure was covered co-extensively with these patches, with a gelatinous coating of tolerably firm concretion, differing from that of diphtheria only in its greater transparency—a character probably mainly attributable to the absence of lamination, arising from its simultaneous formation. This concretion possesses a structure which is identical with that of the early condition of diphtheria, consisting of a fibrinous matrix or substratum, in some parts of which cells are embedded. The substratum appears on microscopic examination to be transparent, or faintly granular, but sometimes exhibits indistinctly the characters of fibrillation. The cells resemble pretty closely those exhibited in Fig. 8. The cell wall is of extreme delicacy, and encloses a spheroidal nucleus distinguishable without the addition of acetic acid. On the addition of that reagent, the former becomes distended, but does not disappear, while the latter either assumes the form of a spheroidal highly-refractive body, of about two-thirds of its previous apparent diameter, or is resolved into the double, triple, or horse-shoe shaped forms often described as characteristic of the pus corpuscle. On examining the mucous membrane subjacent to the concretion, it was found to have lost its columnar or ciliated epithelium, but the cells of the subjacent epithelial layer existed in an unaltered condition. They differed so completely in appearance, size, and structure from the exudation cells, that there was no difficulty whatever in distinguishing them. In two days the process of transformation of the substance of the concretion into fibrous tissue had commenced, as was evidenced by the appearance of nuclei resembling those shown in Fig. 6, arranged in a uniform direction. The pellicle possessed great firmness and elasticity, and could be stripped off the affected patches with ease to any extent.



a Fig. 7.* b

On these facts I will not further comment than to observe, that the only important difference between the cantharidic and the diphtheritic concretion, consists in the absence of any tendency in the latter to transformation into permanent tissue, as contrasted with the early period at which that process commences in the former. So far as concerns the mere fact of fibrinous concretion, we are perhaps entitled to infer that it indicates nothing more than the intensity of the process

* Cells embedded in granular matter from the tonsil: *a*, from a child aged eleven; *b*, from an adult.

of exudation, and that in diphtheria the subsequent changes are prevented or retarded either by a morbid modification of the fibrine itself or by an abnormal condition of the adjacent living structures.

6. *Granular deposit on the surface of the epithelium or in its substance* (Angine couenneuse (?) of Bretonneau).—In a great number of cases designated as diphtheria, the white coating of the mucous membrane possesses characters which differ entirely from those above described. It consists, not of fibrillated coagulum, but of a granular or amorphous deposit. Even when most recent it is never gelatinous; it possesses, when first formed, a creamy consistence, and communicates to the part an appearance as if it were coated with white or yellowish-white paint. It never acquires much elasticity or thickness, in which respect it contrasts strongly with the other form of concretion. When recent it contains no new cellular elements, but consists entirely of granular material, such as is represented in fig. 7*a*; these, however, are soon developed interstitially, especially in the oldest, that is, the most superficial layers. I have not been able to discover any essential distinctions between these and those already described. The various forms I have observed are shown in fig. 8 *a* and *b*, and much resemble those which present themselves in recent pus. Some of the cells exhibit the appearance of transparent vesicles, the smallest of which are scarcely larger than the coloured blood-corpuscle, fig. 8*a*; these either vanish entirely or lose their vesicular character on the addition of acetic acid. Others resemble young pus-cells, both in their appearance before they are exposed to any reagent, and in the form of the nucleus as exhibited after the addition of acetic acid (fig. 8*a*). I have never seen the trefoil or horse-shoe shaped nuclei which are so commonly observed in the vesicles of mature pus. The granular matter when first formed is deposited interstitially among the epithelial scales, as I have satisfied myself by numerous examinations of recent pellicles. This first deposit carries with it superficial epithelial layers, under which the succeeding strata of concretion are formed, so that their anatomical position would seem to be between the cellular and squamous layers of the epithelium. The granular material is so opaque that it completely obscures the outlines of the epithelial scales, but on the addition of acetic acid to the preparation, all the finer (albuminous) granulation disappears, the coarse (fat granules) remaining, as shown in fig. 9. Between the former, that is, the finely-granular matter, and true fibrine, there is perhaps no greater difference than that which distinguishes the

Fig. 8.*


Fig. 9.†

* Cells of various forms: *a*, finely granular vesicles, without nuclei and nucleated cells (weak acetic acid), from the concretion, the structure of which is shown in fig. 7*b*; *b*, cells from fibrinous laryngeal concretion; *c*, cells from soft fibrinous concretion in the bronchial tubes, much resembling pus-corpuscles; *d*, the same, exhibiting the triple nucleus after the addition of strong acetic acid.

† Superficial layers of epithelium infiltrated with granular matter (acetic acid).

granular masses which, under certain general morbid conditions (blood disease), take the place of the true coagulum. If, with Virchow, we confine the term fibrine strictly to the result of coagulation, we cannot possibly consider this granular, creamy, or caseous deposit we have been describing as fibrinous. If, however, we reflect that both are albuminous compounds, and that they occur under pathological conditions which are so similar, it must be admitted that, notwithstanding the differences in their genesis and ultimate structure, a close relation exists between them.

7. *Relation between the morbid anatomy and the symptoms of diphtheria.*—In the preceding paragraph I have endeavoured to distinguish three pathological states of the mucous membrane as observed in cases which come more or less strictly under the designation of diphtheria in its usual acceptation. Although these differences in the resulting organic changes do not strictly coincide with equally marked distinctions in the symptoms of the cases in which they are observed, it will not be difficult to show that they correspond generally to those varieties of disease which have been distinguished by authors as the croupous, the ulcerative, and the simple forms of pellicular sore-throat.

The croupous angina is the true diphthérite of Bretonneau (*croup descendant* of many other authors), a disease which generally commences in the pharynx or fauces, rarely in the larynx, and is marked by the tendency to extend rapidly downwards; it is not accompanied by any constitutional symptoms, and if fatal, invariably destroys its victim by suffocation.

The purely ulcerative epidemic sore-throat, which we may assume to be associated with a morbid condition of the membrana propria, is more rarely met with than either of the other two. I have, however, had the opportunity of studying an extensive epidemic affecting nearly a thousand persons, in which by far the greater number of mild cases were entirely of this character. Of 125 cases of which the records have been furnished me, ulceration was the main feature in 49, and in many of them there was no trace of the diphtheritic concretion. The ulcerative process was in general at first confined to the tonsils, but frequently extended to the neighbouring parts. The appearances observed in slight cases were those of simple abrasion of the mucous membrane, the abraded surface not being covered with any deposit. Ultimately the ulcer usually became coated with a creamy material, the gradual disappearance of which indicated the termination of the morbid process, and preceded the healing of the excavation.*

I have already referred to a case of acute laryngeal ulceration

* As it appeared to me of great importance to determine the fact of the epidemic prevalence of simple ulceration in conjunction with true croupous diphtheritic concretion, I undertook some months ago a journey to Cornwall for this special purpose. I had the opportunity of seeing cases near Launceston in which the ulceration above described existed, associated however with diphtheritic concretion, and of satisfying myself as to their nature. The fact that at the height of the epidemic similar ulceration frequently occurred alone, rests on the general testimony of all the leading practitioners of Launceston, and especially of my friend Mr. David Thompson, to whose kindness I owe most of the opportunities of inquiry that I then enjoyed.

originating under conditions similar to those that produce diphtheritic croup, which exhibited all the symptoms of that disease; so that we are fairly entitled to assume the possibility of acute ulcerative affection of the mucous membrane, whether of the fauces or larynx, independently of the influence of a superficial concretion. Destructive ulceration of the membrana propria, when it exists in its most intense form, gives rise to that kind of sore-throat which is usually designated angina maligna or gangrenosa, but between this and the trivial lesion just described every degree of ulceration is met with. These intermediate conditions are illustrated in the following cases:

M. F., aged nineteen, previously in good health, first complained of sore throat on the 24th of August. I saw her August 25th, when her condition was as follows:—No complaint ("feels better"), except of weakness; skin natural; pulse regular, but weak; breathing natural; no cough; no headache; urine loaded with albumen; the voice weak and nasal; intense swelling of the tonsils and adjacent parts on left side. A dirty-white, apparently thin coating covered the tonsils, velum, and arches as far back as could be seen, and extended forwards on the left side over the soft palate.

August 27th.—Pulse 88, weak; urine still albuminous; the membranous concretion partly detached from soft palate; swelling diminished; other symptoms as before. After this she improved gradually, and in the course of a few days the concretion had for the most part become detached, when it was found that an ulcer existed of the left half of the soft palate, which had completely divided the anterior faucial arch on that side, and occasioned a hiatus which was rendered apparently larger by the retraction of its margins by muscular action. The cavity of the ulcer was covered with an adherent pultaceous film, and its edges were surrounded by a border of crimson mucous membrane. When last seen (Sept. 14th), the cavity had diminished, but was still considerable; the voice was much improved, but was still nasal; there was no regurgitation of liquids by the nose.

Mrs. H., aged thirty-five, first complained of general indisposition, and had shivering and sickness April 2nd. On April 4th she first had sore throat, and applied to Mr. S., who observed superficial ulceration of the right tonsil, but no concretion. On the following day the concretion appeared, and extended over the right tonsil, soft palate and uvula, and right side of pharynx. Several large patches were detached and reproduced. She was seen by me on the 14th of April, a large concretion having shortly before been removed, of great firmness and thickness, and exhibiting on microscopic examination the characters of true fibrine.

The condition of the patient was as follows:—Countenance expressive of extreme depression, but no marked muscular weakness; pulse 70, natural; fauces deeply congested; a white excavated ulcer extended along the anterior aspect of the right half of the velum, from the depression behind the tonsil to the base of the uvula, which was scooped out and drawn to the right side. The margins of the ulcer were raised and swollen, and somewhat more crimson than the surrounding surface. The area of mucous membrane to which the concretion had been attached could be readily distinguished by the intensity of its colour. The comparison of this area with the concretion itself convinced me that it had not extended over the ulcerated surface, but had formed a border around it.

I have seen three other instances of ulceration affecting one or other of the arches of the palate, in all of which the result of the excavation was to leave a permanent perforation of the fold of membrane, through which an instrument could be passed from the cavity of the mouth either into the depression above the tonsil or into the cavity of the nares.

The third variety of lesion of the mucous membrane—the granular superficial infiltration of the epithelium—characterizes in its simple form the most common cases of epidemic sore-throat: those in which the tonsils, uvula, soft palate, and neighbouring parts are covered with a soft, or at all events never tough or leathery coating, having little or no tendency to extension. These cases usually do well under any or no treatment.

8. *Relation of the pathological changes to the mode of termination of the disease.*—(*a.*) In the case of pure fibrinous diphtheria, the termination, if fatal, must always be by mechanical obstruction of the glottis, for the constitutional state is not such as to endanger life independently of the mechanical results of the local disease. If recovery take place, it can only be by the cessation of the fibrinous exudation.

(*b.*) Ulceration of the mucous membrane when it assumes a distinctive, that is, a gangrenous character, is invariably attended by a group of symptoms indicative of a special constitutional state. Death is preceded by rapid diminution of temperature, acceleration but increasing feebleness of the pulse, hippocratic countenance, involuntary evacuations, and extreme agitation, often muttering delirium, or a state approaching coma. These are the characteristic symptoms of purulent infection, but differ entirely from those which ordinarily present themselves even in dangerous cases of diphtheria, the fatal adynamia of which disease certainly does not depend merely on a septic poison common to it and all the septic diseases, but on a special dyscrasia of its own, which differs in a very marked manner from that of any other known disease.

(*c.*) The deposit of granular matter in the substance and on the surface of the epithelial layer has no relation whatever to the mildness or malignancy of the disease. Although, as has been already noticed, it always accompanies the mild form of diphtheria, it is also met with in cases which terminate fatally. (See table on p. 194.) This fact shows that the local lesion affords no indication of the danger to which the patient is exposed, nor even of the existence of the constitutional state on which that danger seems to be dependent.

9. *Relation of albuminuria to diphtheritic sore-throat.*—The first discovery of the relation of abuminuria to diphtheria seems to be referable to a case recorded by Mr. Wade, of Birmingham, and communi-

cated by him to the Queen's College Medico-Chirurgical Society in December, 1857, and subsequently published in his very original 'Observations on Diphtheria.' During the following year, MM. Bouchut and Empis made a similar discovery in Paris, founded on fifteen cases, in twelve of which there was albuminuria. Both of these observers attach very great importance to the renal complication as affording an anatomical explanation of the fact that, in many cases of diphtheria in which death occurs by neither of the two modes already referred to—those by suffocation and septic poisoning—it cannot possibly be attributed to anything in the local condition. On this point M. Bouchut arrives at very decided conclusions: "Albuminuria," says he, "in the absence of scarlatina or asphyxia (dependent on laryngeal obstruction), is a sign in diphtheritic diseases of a commencement of purulent infection, and coincides with a very great gravity of the disease." This conclusion he founds on the observation of two facts—viz., 1. The alteration of the colour of the blood, which assumes the tinge of bistre. 2. The existence of more or less numerous masses of pulmonary apoplexy, resembling those which precede the development of metastatic abscesses in the lungs, or of ecchymoses of purpura of the skin, the serous membranes, and the viscera.

In the summary of this paper which has recently appeared, no information is given as to the number of cases in which these changes were observed.

Mr. Wade is of opinion that albuminuria produces a diminution in the total amount of solid excreta; or, in other words, that the special functions of the kidney are suspended, and that by reason of this, symptoms arise which are indicative "of the retention within the body of those matters which should be excreted."

The facts which I am about to relate, although too few to form the basis of an inference as to the true prognostic value of albuminuria, may yet be sufficient to show that neither of these doctrines is admissible. In eight cases in which I have had the opportunity of making repeated observations as to the condition of the urine, the only ones which have occurred to me since my attention has been directed to the subject, it has been albuminous in all. The following is a summary of the facts observed in each of these cases relating to the occurrence of this symptom.

CASE I.—R. T., female, aged eleven: albuminuria appeared the second day of disease, lasted till termination of case; abundant, accompanied with waxy casts.

General character of symptoms.—Grave throughout; fever at onset, followed by extreme adynamia; abundant faucial concretions; no ulceration; concretion granular.

Result.—Death by adynamia. Post-mortem refused.

CASE II.—A. B., female, aged sixteen; albuminuria first observed the third day, disappeared after the fourth; abundant; waxy casts.

General character of symptoms.—Grave; relapse after albuminuria had ceased; concretion granular, superficial.

Result.—Recovery. No sequelæ.

Case III.—M. R., female, aged thirty-nine: albuminuria appeared the first day; abundant at first, subsequently accompanied with hæmaturia; both diminished rapidly after the sixth day; waxy casts at first, subsequently casts containing epithelium and blood corpuscles.

General character of symptoms.—Slight throughout; no marked depression; concretion granular, limited to tonsils; principal complaint of lumbar pain.

Result.—Recovery. No sequelæ, but persistence in urine of small quantity of albumen.

Case IV.—S. B., female, aged four: albuminuria first observed the third day; lasted till termination; abundant.

General character of symptoms.—Grave; adynamia, followed by symptoms of extension of concretion to larynx; concretion fibrinous.

Result.—Death by croup at sixth day.

Case V.—M. F., female, aged fifteen: albuminuria appeared abundantly the second day; disappeared the fifth day, diminishing gradually.

General character of symptoms.—Slight throughout; extensive granular faucial concretion; consecutive ulceration.

Result.—Recovery. No sequelæ.

Case VI.—W. D., male, aged thirty: albuminuria first observed about the eighth day; disappeared three days after; abundant.

General character of symptoms.—Extremely grave; intense adynamia, with nervous agitation and busy delirium. Concretion not examined.

Result.—Recovery. Slow convalescence, with extreme muscular weakness.

Case VII.—E. S., female, aged ten: albuminuria first observed the seventh day, and continued till termination.

General character of symptoms.—Extremely grave; excessive prostration, with coldness of surface; concretion extending over tonsils, uvula, soft palate, of great thickness.

Result.—Death by adynamia at the tenth day.

Case VIII.—M. A., female, aged fifteen: albuminuria first seen the eighteenth day; albumen abundant; continued till termination.

General character of symptoms.—Grave; gradual exhaustion; extreme adenitic swelling, with suppuration.

Result.—Death by exhaustion at the twenty-fourth day.

Although in several of the cases above related, the cessation of albuminuria was clearly coincident with the amelioration of the patient and the disappearance of the most alarming symptoms, it is not less certain that in one or two others albumen existed in large quantities in the urine, although the cases maintained a mild character throughout. From this it may be inferred that albuminuria is not in itself so alarming a symptom as M. Bouchut is inclined to imagine.

The early period of the disease at which the albumen appears, and the short time during which it lasts, are facts full of importance. In the case No. 3 the urine was found loaded with albumen eighteen hours after the patient had been apparently in perfect health, the exudation having already appeared on one tonsil. It scarcely needs to be pointed out that such a fact as this does not admit of being attributed to a secondary dyscrasia approaching in its nature to purulent infection. A morbid change of the blood of this nature could only originate *consequently* on a previous local change, and could not exist without being accompanied by easily recognised constitutional symptoms. Setting this aside, the fact only admits of two explanations—either the kidneys must be the seat of the primary morbid process, or the albuminuria must depend on an original change in the blood. The first supposition is rendered inadmissible by the coincidence of the renal affection with disease elsewhere, that is, in the fauces; so that we are compelled to conclude that the special morbid blood poison is the primary cause not only of the albuminuria but of all the other symptoms. This cannot be better illustrated than by comparing the poison of diphtheria to that of cantharides, which, from the moment that it enters the circulation, manifests its presence by albuminuria, and produces a series of anatomical changes in the kidney which are identical, as my own observations show, with those described by Mr. Simon and Dr. Bristowe in diphtheria.

As it appeared to me of importance to ascertain whether the existence of albuminuria coincides with the diminution of the solid excreta of the urine, and especially of the urea, I repeatedly sought for the opportunity of determining the question. Owing to the extreme difficulty of meeting with suitable cases, and collecting the urine without loss, I am only able to offer one satisfactory observation, the subject of which is Case No. 6 in the above series.

W. D., aged thirty, was admitted into St. Mary's Hospital, under the care of Dr. Sibson, September 9th, 1859, about the third day after the first symptoms of the disease. He was suffering from sore throat, dysphagia, and considerable depression. Pulse 90. He was ordered quina in grain doses every three hours, beef-tea *ad libitum*, and brandy ten ounces. During the four succeeding days he became progressively worse, and when I saw him on Sept. 13th, his condition was thus noted:—The aspect of the patient is that of impending delirium tremens; the expression is anxious and alarmed; the conjunctivæ are a little injected; the hands in constant tremulous movement, being passed alternately under and over each other; the tongue is bare, red, and is protruded tremulously; pulse 104, soft but regular; respiration 28. The mucous membrane of the fauces is everywhere intensely red and raw-looking, and exhibits here and there small patches of concretion. The lateral walls of the pharynx are covered as far as they can be seen.

Sept. 14th.—General condition of patient as before, but the expression of alarm is less observable; great pain in swallowing; on attempting to sit up he becomes rapidly exhausted; pulse 88; respiration 26; no concretion on fauces; pharynx as before.

Sept. 15th.—General aspect of patient worse; tongue red, dry, and shrivelled; skin warm and moist; answers questions more unwillingly than before; the exudation extends further backwards than yesterday, covering great part of the posterior wall of pharynx; pulse 98; respiration 20.

Sept. 16th.—Aspect improved; tongue moist, covered with a thin white fur; movement of hands diminished. The concretion no longer forms a continuous coating over the pharynx, but is in patches, between which the mucous membrane is exposed.

The next day there was still further improvement, and the albuminuria, which had existed up to this time, disappeared. During this period the following observations were made as to the condition of the urine. The two quantities analysed were collected continuously for twenty-four hours.

	Sept. 15th.	Sept. 16th.	Means.
Quantity in cubic centimetres	1599	1391	1495
Specific gravity	1019	1020	1019
Per-centage of urea	2·88	2·85	2·86
Grammes of urea in 24 hours	46·05	36·0	41·02
Per-centage of chlorides	·29	·26	·27
Grammes of chlorides in 24 hours	4·6	3·3	3·9

Albumen abundant in both cases.

During the period of observation there was complete anorexia, the patient took only an undetermined but small quantity of beef-tea.

For the purpose of comparison with these observations, I made three other analyses after the establishment of convalescence, at which period his general condition is noted as follows:—Countenance pale and thin; marked muscular weakness, as evidenced by his tottering gait and the feebleness of his grasp, and his complaints of aching in back, knees, and insteps after very slight exertion; tongue red and bare, with white fur at base; pulse from 90 to 100; breathing natural. He has during his convalescence suffered from furunculi in considerable numbers, which are now disappearing. Simple diet, with chop and rice pudding, and half pint of porter.

	Oct. 18th.	Oct. 19th.	Oct. 20th.	Means.
Quantity in cubic centimetres	1901	1882	1864	1882
Specific gravity	1011	1012	1011	1011
Per-centage of urea	1·16	1·08	1·12	1·12
Grammes of urea in 24 hours	22·05	20·32	20·88	21·08
Per-centage of chlorides	—	·27	·45	·31
Grammes of chlorides in 24 hours	—	5·08	8·4	6·7

It thus appears that at the acme of the disease, when the urine was intensely albuminous, when there was complete anorexia, and the ingesta were reduced to a minimum, the quantity of urea excreted in a period of twenty-four hours was about twice as great as that excreted during a similar period when convalescence was established, and he was eating with an appetite the ordinary diet of the hospital, with extras.

The above facts show that diphtheria agrees with the other pyrexiæ in being attended with a marked increase in the excretion of urea, and that the existence in the kidney of the condition which is implied by albumen and fibrinous casts in the urine does not necessarily interfere with that increase in the elimination of nitrogenous material. There is, therefore, no reason to apprehend the occurrence of uræmia as a consequence of the renal complication in diphtheria; this complication not being the cause of the dyscrasia, but merely the index of its existence.

ART. II.

On Poisoning by Sausages. By JOHN W. TRIPE, M.D., Medical Officer of Health for the Hackney District.

THE medical literature of this country, which contains so accurate a description of the symptoms and pathology of almost every variety of poisoning, possesses but few records of cases caused by diseased or putrefying meat. Indeed, so rare are these, that Dr. Taylor, in his work on 'Medical Jurisprudence' (sixth edition, p. 157), when writing on poisoning by cheese and sausages, observes, that "although these articles of food have frequently given rise to symptoms of poisoning in Germany, there is, I believe, no instance of their having proved fatal in England." In this passage Dr. Taylor evidently refers to sausages prepared for keeping, and not to those intended for immediate use, as he notices on the same page three fatal cases which resulted from eating pig's-liver sausages.

The present contribution is intended to supply what appears to be a slight gap in medicine and pathology, and contains, therefore, at greater length than otherwise would have been necessary, an abstract of the evidence taken at the inquest in the case which proved fatal, and also an account of the symptoms which occurred in several of the persons who recovered.

In order to obtain a thorough comprehension of the case, it is necessary to premise that a pork-butcher residing at Kingsland (a northern district of London), and in a decidedly poor neighbourhood, sold, on Nov. 3rd, 4th, and 5th, a quantity of "beef" sausages, partly retail, and partly wholesale. The sausages were made of lean beef, pork fat, bread, sage, and condiments, were sold at fourpence halfpenny per pound, and consumed by mechanics and their families. As far as my knowledge goes, sixty-six people partook of them, and sixty-four were attacked at periods varying from three and a half to thirty-six hours subsequently with symptoms resembling those of narcotico-irritant poisoning; and one man died on the seventh day afterwards. Diarrhœa was not prevalent in the district at the time, whilst the vomiting and purging occurred only in those who had eaten the sausages. Another very striking point in the history is, that twenty-six persons living about a mile from Kingsland, purchased at a chandler's shop sausages which had been supplied from the pork-butcher at Kingsland, and all were seized with similar symptoms.

I propose relating the evidence taken at the inquest first, and then giving a brief summary of those cases in which the symptoms presented the greatest difference in severity, duration, and period of attack. Perhaps fewer cases would have sufficed, but I thought it better to give too many rather than too few facts.

The inquest was held on November 14th, by the coroner, Mr. Humphreys. The first witness was Mrs. Eaton, the wife of the deceased, who testified "that she lived at Fox's-place, Kingsland; that her husband, who was thirty-nine years of age, was quite well and at his usual employment up to Friday night, November 4th. He dined about twelve, and had turnips and potatoes, no meat. About half-past six he had his supper, consisting of some sausages, bread, butter, and tea. He ate one of the sausages raw, and three cooked; I and four of my children partook of them, but I did not eat so many as my husband. We all went to bed about nine o'clock, feeling quite well; but about one o'clock (six hours after eating the sausages) my husband was taken ill with severe vomiting and purging; he shivered a great deal, and became very delirious afterwards. I was soon afterwards taken ill with purging and vomiting, and my children also, but not so violently as my husband. Mr. Sutton, a medical man in the neighbourhood, was sent for at half-past six on Saturday morning, and attended him for two days, after which I sent for Mr. James. After the first attack, my husband appeared sometimes better, and sometimes worse; he was very heavy for sleep during the whole time. My husband, self, and children all ate of the same food, *except one*, Richard, aged eleven years, who did not have any, as he was not at home at the time they were cooked. *He was not taken ill.* We had not had any other animal food on the Friday. I ate three myself, but they were all cooked."

I would add to this testimony, that the witness told me that the cat stole two of the sausages, and was taken very ill with vomiting and purging, and that she thought it would have died; this is a very important fact, and was known all over the neighbourhood.

Charles Eaton, aged seven years, proved the purchase of one pound and a half of sausages.

Robert Sutton, registered practitioner in medicine, stated, "I was called to the deceased on Saturday morning, November 4th, at about six o'clock. I found him vomiting violently; there were several vessels in the room containing vomit and evacuations; the quantity contained in the vessels was very small; I saw a quantity of bile, about two ounces, in one of the vessels. Deceased complained of great pain in his abdomen, violent headache, and he appeared very much prostrated. Saw him again in the middle of the day, and found the vomiting and purging somewhat abated. Saw him again on the Sunday, and to my surprise found him sitting up in bed. He said that the sickness and purging had abated; his pulse was very feeble and quick; he appeared rather delirious at my former visits, but now he seemed better than his wife, who had also suffered severely from sickness and purging; his children were attacked in a slighter degree. I did not see him afterwards."

As a pound and a half only were bought—i.e., twelve sausages—and the father had four, the mother three, and the cat two, there remained but three for the four children, which accounts for their suffering so slightly.

Joseph James, Surgeon, of 25, Nelson-terrace, Stoke Newington: "I was called to the deceased on Monday, November 7th, and found him affected with symptoms of English cholera; I prescribed accordingly, and stopped the purging on the Monday evening. On the Wednesday he complained of vomiting, but not to any great extent. He was much exhausted when I first saw him; he gradually became more debilitated, and died on the Thursday. His wife and children were apparently suffering from the same affection. I have since made a post-mortem examination, and found the organs within the chest quite healthy; the stomach and intestines were inflamed and congested, especially the latter. I did not open them, but placed them with the kidneys, spleen, and liver, in a jar for analysis. I consider death to have arisen from severe irritation of the bowels. There were no marks of external violence."

Mr. James informed me that the man became swelled about the head and face, and the eyes injected, on the Tuesday, that he became worse on the Wednesday, so as to present the symptoms which now follow.

In order to complete this part of the case, I insert here, although I was not examined on this subject, an account of the condition in which I found him on the day before his death. Wednesday, November 9th, two o'clock. Deceased was lying on his back in bed in a semi-comatose state, and took no notice of my coming into the room. He was easily roused, and answered readily when spoken to loudly, but relapsed immediately into his former condition. He took no notice of what was going on around him, and did not volunteer any answers to the numerous questions I put to his wife respecting the cause and progress of his and their illness. His hands were cold and clammy, but his body and feet were warm; his pulse was 80 per minute, very feeble and small; his head and face were swollen; his eyes injected, quite ferrety, the pupils small, but sensitive to light. He said that he had no pain or confusion of the head now, but that he felt very giddy and confused when he was taken ill on the Saturday. That he had had a very great deal of pain in his bowels, and had remained very weak ever since he was taken ill. I had some difficulty in getting this account by questioning him. His tongue was much enlarged, marked at the edges by the teeth, had a sodden look, was pale, and covered with a yellowish creamy fur, there was no glaze or redness either at the tip, edges, or in the centre.

Dr. Letheby stated, "That he had received a jar from Mr. James, containing a human heart, kidneys, spleen, liver, stomach, and intestines; also a clot of blood. He had carefully examined the heart, kidneys, and spleen, and found them quite healthy; had tested them, as well as the blood, for mineral and vegetable poison, but had not detected any. On cutting into the liver, he found it gorged to a most unusual extent with bile—indeed, he never remembered one so gorged. The bile was very viscid and thick, and the gall-bladder was empty. He tested the liver for poison, but with only a negative result. On opening the stomach, he found it empty, except a layer of gruel-like matter, which covered the whole of its interior. There was not any odour of poison; the gruel-like matter was alkaline, and evolved ammonia from decomposition. He next examined the mucous membrane of the stomach microscopically, and could not detect anything unusual. There were some particles of food adherent to it, mixed with the mucus. *The stomach did not present any signs of inflammation whatever, or trace of the action of any irritant.* He tested it for mineral and other poisons, but without finding any. He next examined the intestines, which were filled with a thick and somewhat fæcal matter. They were stained of a very deep yellow colour by bile, and contained large quantities of bile throughout their length. He particularly examined the mucous coat, which presented alternations of healthy intestine and of intestine highly inflamed, especially at the lower end of the small intestine, where it presented an appearance like purple velvet. He considered this an undoubted sign of a local irritant. He made a microscopic and an analytical examination of the intestines, but without detecting any evidence of poison. He found some cholesterine and a large quantity of greenish-yellow fat, like altered bile. He next examined the sausage microscopically, but did not discover anything except bread, meat, pepper, salt, and herbs. On treating it with alcohol, it yielded a quantity of an ammoniacal putrid animal matter, and on adopting a similar course with æther, he obtained only ordinary fat. It was then analysed for mineral and vegetable poisons, but with a negative result, nothing unusual having been discovered, except the putrid animal extractive. Cannot tell whether or not this putrid animal extractive existed in the sausage before cooking, but the sausage had no unpleasant smell when he received it. The intestine showed marks of intense irritation, quite sufficient in his opinion to cause death. He did not consider any chemical irritant likely to produce the symptoms during life or the appearances after death."

Dr. Letheby made some other statements on cross-examination, as to the effects of eating a raw sausage, as to the action of bile on the intestines, and on other matters which would not interest a medical reader.

George Peachey, who sold the meat, was next examined. He stated "that beef sausages are made from beef, pork fat, sage, pepper, salt, and bread. The proportion of meat to fat is twelve pounds of beef and four pounds of fat. He bought the meat in Newgate Market, from Mr. Carter. It was heifer beef, and he paid 1*s.* 6*d.* per stone, or $2\frac{1}{4}$*d.* per pound for it. Sausage-meat is generally quite second-rate—is often that of a cow which has ceased to give milk. He bought the meat on Wednesday, Nov. 2nd, and mixed it with pork fat, which he also bought at Newgate Market, but of different persons. He worked the beef into sausages with this fat and some beef he had had left from the previous Saturday. His own family and some other persons ate some of the sausages without being ill. Is certain the cow was killed, and did not die from disease."

William Carter, meat salesman, "considered the meat good; bought it on the Tuesday, and sold it on the Wednesday. It was quite fresh, and exposed publicly for sale."

Mr. Fisher, Inspector of Meat for the City, "does not recollect the meat in question, but was told that he inspected it. Inquired about it, and ascertained that it was cow-beef, and that the animal had been bought from a respectable cow-keeper, who said the cow was quite healthy, but had become dry. Considers that a cow which has been fed for milk and become dry is good to eat. Could not say that he could invariably distinguish the flesh of a beast which had died from epizootic disease from that of one which had been slaughtered in good health."

The next witness proved that he had sold the pigs from which the fat was obtained to Mr. Peachey on Nov. 29th. That the pigs had been killed in Scotland and sent up to London by rail, that they had been probably killed three days before—i.e., on the 26th—that they had become muddled (i.e., moist inside) during the journey, but were otherwise very good, being of excellent quality, and were in his opinion quite fit for food. At the fourth sitting of the inquest it was given in evidence that a number of cows had been attacked with a disease of the mouth and feet on this farm, and that some of them had consequently been sent up to London and killed for food.*

The rest of the inquest had no particular bearing on the matter medically, and has therefore been omitted.

I now propose stating the particulars of some of the other cases which I investigated. It would be tedious, and answer no good purpose, to relate all, as the symptoms varied chiefly in degree, and in the time which elapsed between the eating of the sausages and the supervention of diarrhœa and vomiting.

Amelia Young, residing at 10, Elizabeth-place, Kingsland, bought some sausages at Peachey's on Friday, Nov. 4th. She and two of her children, aged five and three years respectively, ate them at two o'clock. She bought one pound between them. She went to bed about ten o'clock, feeling quite well, and was awoke about two A.M. on the Saturday with a burning pain of the throat and pit of the stomach, vomiting, and diarrhœa. These were soon followed by giddiness and great debility. Her two children who ate them were also taken ill in a similar manner, *but her husband and another child who were out when the dinner was cooked, and did not eat any, remained quite well.* She was very ill all day on Saturday, became a little better on Sunday, and ceased to be sick or purged on the Monday. Is still weak (Wednesday), and felt giddy for two or three days. Her tongue is furred and pulse weak.

Elizabeth Dalby, residing at No. 16, Elizabeth-place, Kingsland, bought some sausages on Friday, Nov. 4th. She, her husband, and three children partook of them about twelve o'clock. They were all taken ill about twelve at night with sickness, purging, giddiness, and great feeling of weakness. She and her husband had a good deal of burning in the throat. She noticed that her stools smelt very badly, and looked like dirty soap-and-water. She remained ill until the Tuesday, but feels better now, though rather weak.

Sarah Moseley, No. 2, Railway-place, Back-road, ate some sausages on Friday, Nov. 4th, at one P.M. Her son, aged nineteen, and a girl

* At the last sitting, the person who sold the cow stated she was healthy and fit for food.

about fifteen, her servant, also had some. She was taken ill about half-past four, or rather less than four hours after eating them. She first felt a burning sensation in the throat, and in a few minutes was very sick, and then purged. The purging and vomiting continued, with severe cramps and pains in her stomach, until next day, and she felt very giddy. Her son and servant suffered in a similar manner, but she was the worst.

Mrs. Jones, residing at 6, Tingey's-buildings, ate some sausages on Thursday evening at nine at night; her two daughters, aged twenty-two and fifteen, also her granddaughter, aged two years, ate some. They had a pound between them. She and all the other people (with one exception) who ate them, stated that there was nothing unusual either in the look, smell, or taste. She was seized with cramps, violent pains in the stomach, purging, and vomiting at half-past five on Friday, being eight hours and a half after eating them. She was very ill until the Tuesday, and felt very much prostrated, with great confusion in the head. On Wednesday, when I saw her, she had a very pallid look, her tongue was enlarged, pale, and covered with a yellow fur; her pulse was very small. There were five other persons in the house who did not eat of the sausages, and who remained quite well.

Mrs. Strutton, residing at 5, Cock and Castle-lane, ate three sausages on Thursday night, felt ill and good for nothing on the Friday, was seized with sickness, &c., on the Saturday (thirty-six hours afterwards), and suffered severely until the Monday morning. Felt great prostration. There were several other persons in the house who did not eat any of them, and who continued quite well.

Mrs. Langley, No. 15, Cock and Castle-lane, ate four sausages at half-past twelve on Friday, and her son, aged seven, ate half a sausage. She was seized with a burning pain of her throat, pain and confusion in her head, about five o'clock, then pain in her stomach and cramps, and at half-past seven, with violent vomiting and purging. These continued from the Friday to the Tuesday inclusive. She was delirious at times, but never lost her senses. Her child was taken ill slightly. Her husband and two children, one aged nine and the other two, did not have any of the sausages, and were not affected as she was. Her tongue was furred; her appearance and pulse were indicative of great prostration and weakness.

Elizabeth Clark, residing at 1, Orchard-street, Kingsland, her husband, and her son who had just returned from Woolwich on leave of absence, also Mrs. Rule, and her daughter, aged twenty-two, visitors, ate two pounds of sausages between them, at three P.M. on Thursday. They were all taken ill with the symptoms described in the other cases, either on the same or the following day. Mrs. Rule and her daughter came a short distance to see them, and were quite well, each had three sausages a-piece; Mrs. Rule was seized with vomiting three hours after eating them, whilst returning home, then with the other symptoms. Her daughter was not attacked until twelve hours afterwards. Mrs. Clark was seized with a burning sensation in the throat, giddiness, and great prostration nine hours afterwards, speedily followed by vomiting and purging. Her husband did not experience any ill-effects for twenty hours, when he had diarrhœa and vomiting, and the son was attacked six hours afterwards. The mother and son suffered most severely, the vomiting, purging, and abdominal pains having continued about every quarter of an hour for three days. When I saw them six days afterwards, the son was confined to his bed, and still complained of occasional purging and vomiting, great giddiness and prostration, a furred, yellowish, sodden-looking tongue, and very weak pulse. The mother was nearly as prostrated, and in other respects in a similar condition. *There were four children at home, and also several other persons in the same house, who did not partake of the sausages, and they remained quite well.*

As the other cases in this neighbourhood were similar to these, I need not multiply them, but will relate one case which occurred a mile off. It appears that the party whose case I am about to relate kept a chandler's shop, and had been in the habit of dealing with the sausage-maker for several years, and of retailing the sausages so bought. She purchased thirteen pounds on Saturday, November 5th, and sold the whole on the same day. All the people, twenty-six in number, who partook of them, were attacked at various intervals afterwards, with the same symptoms as those who ate them in Kingsland, and as far as could be ascertained, no person in any of the houses at Stoke Newington or at Kingsland had sickness and diarrhœa except those who ate the sausages.

Mrs. Jeffreys, residing at 22, Bowling Green-street, Stoke Newington, stated that she had always found Mr. Peachey's sausages very good, and did not observe any difference between the lot and those which had been bought before. Indeed, she thought the flavour better than usual, and is quite positive that there was no unpleasant smell about them. She ate three at about two o'clock on Saturday, November 5th, and was taken ill about twelve hours afterwards, with burning pains in her throat and stomach, cramps, pains in her head and limbs, and giddiness, which were soon followed by some purging and vomiting, for which she took some antibilious pills early on Sunday morning. The sickness and vomiting left her on the Monday, but she still continues weak and languid. Her husband, daughter, and son were all attacked in a similar manner.

There were thirteen families attacked in this neighbourhood, after eating these sausages.

The cases just detailed are of considerable interest as regards the symptoms, post-mortem appearances, and chemical results. There can be no doubt, on a careful examination of the evidence at the inquest, and of the symptoms described by the sufferers, and especially from the immunity from illness of all those who did not partake of the sausages, that the symptoms of narcotico-irritant poisoning were caused by the food, and not by any local cause. I have been particular in giving the leading symptoms in all the cases I have selected, because a medical man of rather high standing in sanitary science expressed his opinion to me that the symptoms probably resulted in great part from the state of those who partook of the food, and from the conditions by which they were surrounded. The facts, that in several instances the symptoms supervened in some members only of the family, the whole being exposed to the same local influences; that they occurred in two persons who did not reside in the district, and that they also happened in a group of cases a mile distant from Kingsland, and in all instances in those only who partook of this particular batch of sausages, in my opinion sufficiently prove the cause to have been in the sausages alone. Another important point is that, with few exceptions, those who ate the most, suffered the most severely. The time, however, which elapsed between the eating and the supervention of the symptoms did not appear to depend on the quantity eaten. Thus, in the cases which occurred at Clark's, in Orchard-street, it appears that of five persons two were visitors, and one, the son, had that day returned from Woolwich; that they ate three sausages each, that one of the visitors was attacked with violent vomiting in three hours, her daughter in twelve hours, Mrs. Clark in nine hours, her son in six hours, and the husband not until twenty hours. The duration of attack also varied very much: in the visitors the vomiting and purging ceased after two days, when they soon became well; the father did not suffer for so long a period; whilst the mother and son were confined to their beds for five days, and remained in a very weak, debilitated state for some time. In the majority of the cases the vomiting and purging lasted two days, and the feeling of debility three or four more. The tongue in nearly all those I saw six days afterwards, presented a soddened look, was pale and furred. Nearly all I questioned about the diarrhœa stated the evacuations to have been very watery and fœtid; some said they looked like dirty soap-suds, others like the washings of putrid meat. The vomited matters were stated by all to have been very bitter and like bile; indeed, the general idea amongst them was, that they were suffering from a severe bilious attack.

The earliest symptom was a sensation of burning in the throat and at the epigastrium, followed, usually in a few minutes, but in some delayed for hours, by the vomiting and purging. The next symptom was a feeling of extreme prostration; then giddiness, or noises, or confusion in the head, and in some delirium. In the man Eaton the cerebral symptoms were extremely marked; the delirium was like that of delirium tremens, and very decided; the eyes were very red; and he was semi-comatose, but could be roused so as to answer questions correctly, or to take his medicine. There was, however, one peculiarity about his case—namely, that all the urgent symptoms, which were so severe on the Saturday and part of the Sunday, had a most marked remission for about forty-eight hours, when the cerebral symptoms and prostration returned with greater severity than ever, and continued up to his death.

The very unusual flow of bile, with the extreme and irregular congestion of the small intestines, were very characteristic marks of the action of some intestinal irritant. The absence of congestion or ulceration of the stomach, and of ulceration of the intestines, proved that the irritant was not of a mineral kind; whilst the failure to discover by the microscope anything except the *débris* of food, showed that no savin or other vegetable irritant had been inadvertently used.

The results of the chemical analysis quite corresponded with the symptoms during life, and the post-mortem appearances after death; the only difficulty in the case arising from the time which elapsed between the sausage being cooked and the analysis being made. The sausage was cooked on Nov. 4th, and was not analysed until about a fortnight afterwards; when, although it had not any unusual smell, yet a rather large quantity of a peculiar stinking animal extractive was obtained by alcohol. Dr. Letheby could not therefore state whether this animal extractive existed in the sausage before it was cooked, or was formed afterwards. Taking, however, the symptoms during life, both of the man Eaton and of the other sufferers, with the post-mortem appearances after death, there can, I think, be but little doubt that the animal extractive had been formed before the sausages were cooked. The partial improvement in the man Eaton, and the violence with which the symptoms returned (especially those indicative of cerebral disorder), may have arisen from a new development of poison about four days after the sausages were eaten.

The next question which arises is as to the meat which caused the illness. Was it the pork fat, or the beef? If the beef, it must have been in consequence of previous illness of the beast, as there was not time for any putrefactive action to have occurred before it was used. This cannot be shown with certainty, for although there is great suspicion of the animal having had some illness, and having been killed in consequence, yet there is an absence of proof. Whilst, on the other hand, it is far more likely to have been the pork fat, as the pigs were killed on the 26th of October, and were not used until the 3rd or 4th of November, having been packed up for three days, and become "muddled"—i.e., heated so as to become covered with moisture inside. Dr. Taylor observes, "the poisonous effect is supposed to depend on a *partial* decomposition of the fatty parts of the sausages."

The only other point which our limits will allow us to consider, is the question—What are the differential symptoms from those of arsenical poisoning? The time which elapsed between the eating of the food and the supervention of vomiting was much longer than ordinarily occurs from arsenical poisoning, having varied from three and a half to thirty-six hours, whilst those from the latter ordinarily occur in from a quarter of an hour to three or four hours, or it may be nine or ten. The bilious vomiting in these cases, instead of the brown, turbid matter ejected in poisoning by arsenic. The peculiar stinking alvine evacuations would also serve to distinguish between the two. The delirium and semi-comatose condition were different to those which I have seen in arsenical poisoning; also the comparative absence of thirst, and constriction of the throat, would serve for additional evidence.

The post-mortem appearances, however, even without the analysis, were almost conclusive of themselves; as there was a total absence of ulceration, either of the stomach or intestines, which almost invariably occurs in the former organ, even when arsenic has obtained an entrance into the body through the lungs, skin, or rectum. The uniformly pale colour of the stomach was also opposed to arsenical poisoning. The state of the tongue was also very different to that which I have noted in arsenical poisoning—having been pale, enlarged, marked by the teeth, and covered with a dense yellow fur.

Art. III.

Contribution to our Knowledge of Digestion. By George Harley, M.D., Fellow of the Royal College of Physicians of Edinburgh, Professor of Medical Jurisprudence in University College, and Physician to the Northern Dispensary.

At the meeting of the British Association for the Advancement of Science, held at Leeds, in September, 1858, I read a paper, entitled, 'Notes of Experiments on Digestion.' The observations that I then laid before the members of the Association, were the result of a series of experiments which I had performed during the preceding year, on the nature and properties of the more notable of the digestive fluids. At that time I contented myself with merely directing attention to those points which appeared to me to be the most important, in consequence either of their differing from the commonly received doctrines, or of their tending to elucidate some of the questions which were still *sub judice.* The communication, however, had no pretension to be considered as a monograph on the digestive process. It was merely a collection of jottings from my note-book. On the present occasion I purpose relating a few of the facts then alluded to, together with the results of some experiments since performed, limiting myself, as on the former occasion, to a mere statement of naked facts, reserving the explanation of them for a future occasion.

In order to condense my material into the smallest possible space, and prevent repetition, I shall treat of the subject under three heads. 1st, *Buccal Digestion;* 2nd, *Stomachal Digestion;* and 3rd, *Duodenal Digestion.*

I. Buccal Digestion.

The experiments on the salivary secretion were chiefly made with my own saliva. It was collected between meals, and was of course "mixed,"—that is to say, composed of a mixture of the secretions from the parotid, submaxillary, and sublingual glands. The specific gravity of the saliva, after filtration, varied between 1003·9 and 1005·0. (Longet and Bernard give the specific gravity of the human saliva at 1004 to 1008.)

On making a qualitative analysis of this mixed secretion, I was able to confirm the statement of Berzelius, that it contained a peculiar animal ferment—*ptyalin;* of Brande's, that it contained albumen; and of Dr. F. Simon, that it likewise contained casein. The presence of the latter substance has been denied by some recent observers, but I was able to satisfy myself of its existence, by adding lactic acid to saliva from which the albumen had been previously removed.

On making a quantitative analysis of my own saliva, I found that it contained in 100 parts,

Water			99·331
Solids		0·669	
Ferment... ... ... Albumen ... ... Casein ... ... Mucus, and Epithelium... ...	Organic matter... ...		0·391
Chloride of Sodium ... ... Sulphate of Potash ... ... Sulphocyanide of Potassium... Phosphate of Lime ... ... " Magnesia... ... And Iron	Inorganic matter ...		0·278
			100·000

Bernard says that iron is not a normal constituent of human saliva. I have tested for it in the saliva of four individuals whom I considered healthy, and never found it absent. Of course it was only the ashes, after incineration of the saliva, that I tested; for, until the organic matter is destroyed, the iron gives no indication of its presence with the ordinary reagents. The last-mentioned author has made a most erroneous statement with regard to the sulphocyanide of potassium. He says that this substance can only be found in saliva that has begun to decompose, and that if detected in freshly-secreted human saliva, it is on account of the person from whom the saliva is taken having bad teeth, the decayed matter of which has caused the decomposition of the secretion while still in the mouth.* I have not a single bad tooth, and although I have tested my saliva not less than twenty times, have invariably detected in it sulphocyanide of potassium. Moreover, I one day examined the saliva of eleven of the gentlemen attending my Practical Physiology class, and it was present in each case. Some of the gentlemen had bad, some had good teeth,—and, curiously enough, the one who happened to possess the worst set of teeth—nearly all the molars being decayed—had the least amount of sulphocyanide in his saliva.

Digestive Powers of Saliva.

AMYLACEOUS FOOD.—The peculiar action of saliva on amylaceous food, I need scarcely say, I confirmed. One part of freshly-secreted healthy human saliva converted a saturated solution of boiled starch into grape sugar, at a temperature of 38° Cent., in 100 seconds. Its action on raw starchy matters is exceedingly slow and imperfect. It

* Leçons de Physiologie, vol. ii. p. 140.

is, I believe, on this account that uncooked fruit, especially when unripe, is so prone to bring on an attack of indigestion.

Adipose Food.—It is very generally believed that the secretion from the salivary glands does not assist in the digestion of fats. The results of my experiments have led me to agree with Monsieur Longet, in believing that the saliva has—although only to a very limited extent—the power of emulsioning fats. This power, I believe, is entirely due to its alkaline reaction, and consequently can be of comparatively little moment in the digestive process.

Albuminous Food.—Saliva is said to have no action upon protein substances. When acidulated with hydrochloric acid—fifteen drops to the ounce,—and kept in contact with slightly boiled albumen of egg, during forty-eight hours, at a temperature of 38° Cent. (the temperature of the human body), I find that it transforms a small quantity of the albumen into peptone.* The manner of detecting this transformation of albumen I shall presently allude to.

Bidder and Schmidt estimate the quantity of saliva secreted by an adult in twenty-four hours at from 2 to 3 lbs. I have performed some experiments with the view of ascertaining this point, and find that on an average I can secrete in the course of four hours, between meals, 6 ozs. of saliva, of a specific gravity of 1004. This experiment is performed without putting any sialogogue in the mouth—merely by sucking the tongue. Supposing the secretion of saliva to continue at the same rate throughout the day, 6 ozs. in four would be equal to about $2\frac{1}{4}$ lbs. in twenty-four hours. As very little is secreted during sleep and between meals, I regard Bidder and Schmidt's estimate as far too high. I imagine the probable quantity secreted in the twenty-four hours by an adult is from 1 to 2 lbs.

In reply to the question whether the salivary glands have the power of excreting foreign matters from the blood, Bernard has specially pointed out that they possess this property with regard to the iodide of potassium. He says that after this salt is taken into the stomach, it appears in the saliva long before it can be detected in the urine. I have carefully repeated this experiment several times, but with somewhat different results from those got by Bernard. The following may be cited as an average example.

Having first completely emptied the bladder, I took 5 grains of iodide of potassium, dissolved in 6 ozs. of water. After rinsing the mouth, other 15 ozs. of water were drunk. The saliva and urine were then tested alternately every minute. In ten minutes exactly the salt was detected in the saliva, and in one minute later—the next testing—it was also found in the urine. I think therefore we are justified in saying, contrary to Bernard, that the kidneys excrete iodide of potassium from the body as quickly as the salivary glands. Both organs excreted the iodine in the combined form. If the salt was decomposed in the body, the iodide must therefore have become again united with the potassium, or some other base, either before or at the time of its excretion.

* Longet, in his Treatise on Physiology, relates some very interesting experiments that he made on this point.

After twenty-four hours, the presence of the iodide could still be detected in both secretions, and in forty-eight hours it was found in the urine only. On one occasion, however, it was present in the saliva after seventy-two hours, but its presence in the urine was doubtful. Some experiments made with the iodide of potassium in the form of pill, gave very unsatisfactory results. Its presence in the saliva could never be detected in less than half an hour, and on one occasion it did not appear at all.*

II. STOMACHAL DIGESTION.

In order to be able to study natural digestion, I made a gastric fistula in a dog's stomach, and placed in it a silver canula. In eleven days after the operation, the animal was ready for experiment. The fluid found in the dog's stomach, during fasting, had a slightly alkaline reaction. When the animal was prevented from swallowing his saliva, however, the mucus secretion of the stomach was invariably neutral. The true gastric juice—which is only secreted during digestion—was of course always acid.

With the view of obtaining the gastric juice as pure as possible, in order to subject it to analysis, the dog was kept fasting during thirty-six hours. The stomach was then thoroughly washed out with water injected through the fistula, and afterwards tripe, carefully freed from fat and mucus membrane, was introduced by the opening. The dog was gagged in order to prevent any saliva from getting into the stomach, and he was not allowed anything to drink while the gastric juice was being collected. Still further to insure the perfect purity of the secretion, it was filtered before being analysed.

100 parts of gastric juice so obtained were found to contain—

Water			97·288
Solids		2·712	
Organic Matter, chiefly pepsin			2·247
Chloride of Sodium	Inorganic Matter ...		0·465
„ Potassium			
Phosphate of Lime			
„ Magnesia... ...			
And Iron			
			100·000

Digestive Powers of Gastric Juice.

AMYLACEOUS FOOD.—Gastric juice does not possess the faculty of transforming amylaceous matters into sugar. Notwithstanding its acid properties, and its power of neutralizing the alkaline saliva, it does not, however, prevent the latter secretion, after entering the stomach, from changing starch into glucose; indeed, I found that saliva, mixed with an equal quantity of gastric juice, acted almost as quickly upon boiled arrowroot as when no gastric juice was present.

* From a variety of experiments which I have made on the rapidity with which substances act, I have been led to the conclusion that the form of pill is the very worst that can be adopted in administering a remedy. I have therefore almost entirely discontinued it in practice.

Saccharine Food.—Gastric juice transforms cane into grape sugar.* The pepsin takes no share in this transformation. It is the free acid which alone acts upon the saccharine matter, for as soon as the acid is neutralized, the transformation ceases to occur. Gastric juice has no influence over sugar of milk or grape sugar.

Adipose Food.—Fats are liquefied in the stomach partly by the cell walls being dissolved through chemical and mechanical agency, and partly by the elevated temperature of the organ; they are not, however, emulsioned. Dr. Marcet has recently shown that the neutral fats are changed into fatty acids in the stomach.

Albuminous Food.—The chief action of the gastric juice is exerted upon protein substances. It possesses the faculty of rendering them soluble in water; not simply by dissolving, but by transforming them. When an acid acts upon coagulated albumen, it merely dissolves it; but if pepsin be coupled with the acid, it transforms the albumen into another substance—peptone—having entirely different physical and chemical properties. Longet has pointed out an easy way of distinguishing between albumen dissolved and albumen digested.† Digested albumen is endowed with the faculty of preventing the sulphate of copper and potash test from indicating the presence of sugar, while at the same time it gives to the solution a beautiful violet colour. Dissolved albumen possesses no such power. At first, this test appeared to me to be a very valuable one, and does so still, notwithstanding that a great part of the glory has been shorn from it, by the discovery that gelatine acts like digested albumen. This fact, which was first pointed out by Bernard, I have confirmed. Moreover, I have gone a step further, and found that there are several substances endowed with the faculty of masking the presence of sugar when the sulphate of copper and potash test is employed for its detection; among these, casein and meta-albumen stand pre-eminent. But I find that albumen, acted upon by an acid in presence of any animal (ptyalin) or vegetable (yeast) ferment, has, although in a much less degree, the same property. Nay, more, I have observed that protein substances undergoing putrefaction not unfrequently give the beautiful violet colour with Longet's test, and at the same time conceal, to some extent, the presence of sugar. Lastly, I have found that even with the true peptone the test must be carefully employed, as the power which it possesses is limited. A given quantity of peptone can only mask a definite amount of sugar. Whenever an excess of saccharine matter is present, its usual action upon the copper becomes manifest.

The power which the gastric juice has of coagulating milk is entirely due to the action of its free acid. Neutral pepsin, I find, does *not* coagulate casein. The next point to which I would advert is the quantity of gastric juice secreted by the stomach in twenty-four hours. According to Bidder and Schmidt, the daily secretion of gastric juice

* Blondlot, Frerichs, and Funke deny this; but Bouchardat, Sandras, Schiff, Longet, and my own experiments confirm the fact.

† Longet's Text-book of Physiology. Article, Digestion, p. 154.

is equal to a quarter of the weight of the whole body. If, therefore, the estimate given by these observers be correct, a man weighing twelve stone ought to secrete no less than three stone of gastric juice in twenty-four hours. This appearing to me to be an excessive quantity, I made the following experiment upon a dog, which although it had had a gastric fistula for six months was in excellent health and condition; the animal weighed twelve pounds. After washing out the stomach, a quantity of well cleaned tripe was introduced by the artificial opening. In five hours, $4\frac{1}{2}$ oz. of gastric juice were obtained. The secretion ceased after this time, and the animal, which had been prohibited drinking any water during the experiment, appeared very thirsty: 4·5 oz. in five hours, if calculated at the same rate over the entire day, would be equal to 21·6 oz. in twenty-four hours; in round numbers, one-ninth of the weight of the dog. Lehmann estimates the quantity of gastric juice secreted in twenty-four hours at a little less, namely, one-tenth of the animal's weight. I think, however, that even this estimate is too high, for the secretion of gastric juice does not continue during night and day, being interrupted except at the time of digestion. I am inclined, therefore, to estimate the daily secretion of gastric juice as a fifteenth of the weight of the whole body.

WHY IS THE STOMACH NOT DIGESTED? We know that it is not on account of its being ordinarily indigestible, for under certain circumstances it digests itself. That it is not because the organ is living, Bernard has shown. He introduced the hind legs of a living frog through the fistulous opening in a dog's stomach; after a couple of hours, notwithstanding that the animal was still alive, the limbs were found digested away. This experiment has been repeated by Dr. Pavy, and myself, and confirmed. The living tissues of warm-blooded animals are also digested. If, for example, some gastric juice be taken from an animal's stomach and introduced under the skin of its back or belly, in a very few hours the fluid will be found to have digested its way out, and left a hateful looking wound.

This experiment I have also repeated, and confirmed. If, then, the non-digestion of the living stomach is neither on account of its being indigestible nor a living tissue, is there anything contained in the organ which prevents the gastric secretion from attacking its walls?

Bernard believes that it is on account of the organ being lined with a layer of epithelium cells, not readily acted upon by gastric juice. And that as layer by layer of these cells are removed, others are ever ready to fill their place. The cells, he imagines, pour forth a quantity of mucus which aids in averting the dissolution of the stomach. This opinion, although adopted by all subsequent writers, is opposed to the results of my experience. In the first place, proof is wanting that the epithelium lining the stomach resists the chemical action of the gastric juice. Bernard's own experiment shows that epithelium covering another part of the body entirely lacks such power, and I am unacquainted with any evidence to prove that the epithelium cells of the stomach are not as readily acted upon by the gastric juice as those covering the frog's skin. As the present communication consists of the notes of a series of experiments which are intended to appear at a later period in a more complete form, I shall confine myself at present to stating my belief that it is chiefly, if not solely, the mucus which protects the stomach from the chemical action of its own gastric juice. On killing an animal, such as a rabbit, in full digestion, it is not uncommon to find a thick layer closely surrounding the bolus of food, in addition to that covering the walls of the organ. In the pig I have occasionally seen the layer of mucus lining the stomach at least one-eighth of an inch in thickness. In proof of its being the mucus chiefly, and not the epithelium, I may mention, that if a stomach be divided into two parts and from one of the halves the mucus be carefully removed, with as little as possible injury to the epithelium, while the other half is left intact, it will be found, on making each into a sort of bag, and filling it with pure gastric juice, that a few hours' exposure to an elevated temperature will suffice for the solution of the half from which the mucus was removed, whereas the other, notwithstanding a similar treatment, will remain unacted upon.

III. Duodenal Digestion.

Action of the Biliary Secretion.—Bernard, in his Lectures,* already so frequently referred to, has revived an old theory regarding the action of bile upon chyme. He states that when the bile comes in contact with the chyme on its escape from the stomach into the duodenum, it (the bile) precipitates the digested azotized matters, and not these only, but even the pepsin also. Corvisart questions the correctness of this opinion, and says, that the bile, instead of precipitating the organic substances contained in the chyme, is in reality itself precipitated by the acid of the latter.† I have performed several experiments with the view of ascertaining which of these observers is correct, and the results have led me to adopt Corvisart's opinion. I may cite the following as the chief reasons for my so doing:—

1st. Alkaline human bile, when added to pure chyme, gives a copious precipitate.

2nd. The same bile added to chyme, the acid of which has been previously neutralized, gives a turbidity; but no precipitate.

3rd. Alkaline bile added to a neutral solution of pepsin, gives a turbidness; but no precipitate.

4th. Alkaline bile added to distilled water, gives a turbidness; but no precipitate.

5th. Alkaline bile added to water, acidulated with hydochloric acid, gives a copious precipitate.

On employing alkaline bile from the sheep and the snake (coluber natrix) instead of that from the human subject, I obtained precisely similar results; I think, therefore, we may safely conclude that the precipitate which bile gives with chyme, is entirely due to the presence of the acid in the latter. And that this precipitate consists neither of

* Vol. ii. 42, 23. † Compt. Rend. 6th April, 1857.

the peptone nor pepsin, as Bernard supposes; but of one or more of the constituents of the bile, as Corvisart believes.

AMYLACEOUS FOOD.—I have confirmed the statement that bile does not transform starchy matters into sugar.

ADIPOSE FOOD.—The action of bile upon fats being a question still *sub judice*, I made several experiments with this secretion, and liquid as well as solid fatty matters. The results I obtained were very contradictory; but on the whole led me to the conclusion that bile has, to a limited extent, the property of emulsioning fats. Since my first experiments were performed, Dr. Marcet* has confirmed a very interesting fact, first noticed by Lentz, which at once explains why the results of experiments with bile and oleaginous bodies are so captious. He found that it is only the fatty acids, and not the neutral fats which form an emulsion with the biliary secretion. On repeating some of my experiments, with the light thrown upon the subject by Lentz and Marcet, I was no longer troubled with contradictory results.

ALBUMINOUS FOOD.—Bile has not the power of digesting albumen. When added to gastric juice, bile greatly diminishes its solvent action upon protein substances. Most probably by neutralizing the acid, without which gastric juice cannot act.

PANCREATIC SECRETION.—Pancreatic juice obtained from a fistula, which I established upon a dog in the usual manner, had the appearance described by Bernard. It was a clear, pale, opaline, slightly glutinous liquid, with a well marked alkaline reaction. I confirmed the following facts.

AMYLACEOUS FOOD.—Pancreatic juice, as first pointed out by Valentin, transforms starchy matters into sugar with much greater rapidity than saliva or any other known ferment. It continues its action, too, upon boiled arrowroot starch at a lower temperature than other ferments. Neutralization with gastric juice does not hinder its transforming amylaceous substances into sugar.

ADIPOSE FOOD.—As pointed out by Eberle, in 1835, and confirmed by Bernard, in 1848, pancreatic juice emulsifies fats, and makes with oil an artificial chyle, closely resembling what we find in the lacteals.

ALBUMINOUS FOOD.—In 1835 Eberle discovered that the secretion from the pancreas fluidified chyme loaded with peptone. In 1836 Pappenheim and Purkinje found that it had the power of digesting albuminous substances. I have confirmed both of these statements. In 1857 Corvisart published a monograph, entitled, 'On the Digestion of Azotized Alimentary Bodies by the Pancreas,' in which he relates a number of experiments upon animals, the results of which led him to the conclusion that pancreatic is a more powerful solvent of protein substances than gastric juice. I have repeated, and *to a certain extent* confirmed his results. My most successful experiment was the following:—After rendering a strong healthy cat, while fasting, insensible with puff-ball smoke, I opened the abdomen and placed a ligature on the duodenum, close to the pylorus. The intestine was now emptied

* Proceedings of the Royal Society, vol. ix. p. 306.

as completely as possible, by drawing it through the fingers; a second ligature, placed near to the commencement of the jejunum, and a third about twelve inches further down. Into the two loops of intestine thus formed, equal portions of moderately hard white of egg were introduced. A third loop of intestine, close to the last, was then secured by another ligature, and the wound in the abdominal parietes closed up. In twenty hours the animal was killed by section of the medulla oblongata. On opening the abdomen scarcely a trace of peritonitis was visible. The duodenal portion of the intestine was distended with fluid of a slightly acid reaction, and having much the appearance of chyme without the intermixture of bile. No fragments of egg remained. On filtration I obtained from it a clear opalescent liquid not at all unlike pancreatic juice, of which no doubt it chiefly consisted. It transformed starch readily into sugar. It gave a white precipitate with alcohol, alum, acetate of lead, and gallic acid. When tested for digested albumen, by Longet's test, it gave only a faint purple.

The second portion of the intestine included in the ligatures contained no liquid. The fragments of egg remained in large lumps, but were much changed in appearance. They had become hard, friable, and gritty to the feel. When pounded in a mortar with a little water, and filtered, the solution, on Longet's test being applied, yielded slight indications of the presence of peptone.

The third loop of intestine, into which nothing was introduced, remained empty.

The pancreatic juice appears therefore to be the most remarkable of all the digestive fluids, for it seems to unite in itself the functions of the salivary, gastric, and biliary secretions.

I have refrained on the present occasion from making mention of the experiments which have been recently made, with a watery extract of the pancreas of various animals, by Keferstein, Hallwacks, Funke, Meissner, and Brinton, as I purpose giving to them due attention elsewhere.

ART. IV.

On French Millstone-Makers' Phthisis. By THOMAS B. PEACOCK, M.D., F.R.C.P., Assistant-Physician to St. Thomas's Hospital and Physician to the Hospital for Diseases of the Chest, Victoria Park.

IN 1727, Wepfer* referred to the frequency of consumption among the workmen employed at Waldshut, on the Rhine, in the excavation and cutting of millstones. In 1775, Le Blanc† described, on the authority of M. Clozier, of Etampes, a form of disease prevalent among the men employed in cutting the grits or freestones of that neighbourhood, and popularly known as the "Maladie du Grès" or the "Maladie de Saint Roch,"‡ and in 1799, Dr. Johnstone§ drew attention to the frequency of

* Observationes Medico-practicæ, &c. Scaphusii; 1727. Obs. civ., f. 444.

† Mém. sur la formation et l'endurcissement du Grès, &c., Paris; 1775, p. 585.

‡ This term seems to be popularly applied in France to several diseases, especially to the plague and some form of scorbutic cutaneous disease.

§ Memoirs of Medical Society of London, vol. v., 1799, p. 89.

consumption among the workpeople employed in Worcestershire in needle-pointing, and stated that the disease was locally known as the "grinder's rot." Since these observations and the more casual notices of the frequency of consumption in stone-cutters, metal grinders, &c., by Morgagni, Ramazzini, Kirkland, &c., the subject has attracted much attention; and the account given by Dr. Alison* of the prevalence of pulmonary disease and the short duration of life among the stone-masons of the neighbourhood of Edinburgh, and the similar reports as to the dry-grinders of Sheffield, by Knight,† Holland,‡ and Favell,§ have established the fact of the injurious influence of occupations in which the particles of stone or metal are thrown off, and, becoming diffused through the atmosphere of the shops, are inhaled by the work-people. More recent writers have indeed shown that—as in the instances referred to by Wepfer and Johnstone—other causes besides the direct injury sustained by the respiratory organs conduce to the injurious effects of these trades. M. Burgoin, from an investigation into the relative proportion of births and deaths at Meusnes, in the department of the Loire et Cher, the seat of the manufacture of gun-flints for France, from 1680 to 1709, before the establishment of the manufacture, and from 1760 to 1790, when it was fully in operation, inferred that the duration of life had been greatly reduced by the introduction of the employment, and he regarded this result as epecially due to the gritty particles inhaled by the workpeople. M. Benoiston de Chateauneuf,‖ however, showed that in certain localities in which the population was employed in stone-cutting, the rate of mortality was not higher than in other parts of France, and that some trades in which the workpeople inhaled a gritty dust were not especially injurious, he therefore inferred that the high rate of mortality at Meusnes was due to other causes, and, as he supposed, especially to the confined position assumed by the workpeople, preventing the free expansion of the chest, and the impure air of the quarries and workshops in which they were employed. Subsequently, M. Lombard,¶ of Geneva, from a very elaborate investigation of the causes of death in the hospitals of Paris, Hamburgh, and Vienna, and especially of the mortuary registers of Geneva, from 1776 to 1830 for men, and from 1816 to the same year for women, was led to infer, that, though the inhalation of gritty particles, and especially of the finer and heavier kinds from stone and iron, was injurious, it was less productive of phthisis than other causes which act by deteriorating the general health, such as want of fresh air and exercise, and of the necessaries of life. In the following paper I propose briefly to draw attention to the prevalence of pulmonary disease in a class of workmen among whom it has not, so far as I am aware, been hitherto particularly noticed: I refer to the French

* Transactions of the Medico-Chirurgical Society of Edinburgh, vol. i. p. 373.

† North of England Medical and Surgical Journal, vol. i., 1830-31, pp. 85 and 167.

‡ Diseases of Lungs from Mechanical Causes. London, 1853.

§ Transactions of Provincial Medical and Surgical Association, vol. xxiv. (N.S. vol. ii.), 1846, p. 143.

‖ Annales d'Hygiène Publique, t. vi., 1831, p. 5.

¶ Annales d'Hygiène Publique, t. xi., 1834, p. 5.

millstone-makers, or builders, as they, perhaps more properly, call themselves.

The stone which these men work is known in the trade as the "French Burr." It is imported into this country by way of Rouen and Havre, chiefly to London, Hull, and Liverpool, in which towns the shops for the manufacture of millstones are chiefly situated. During the war, when the intercourse between France and England was cut off, the millstones employed so imperfectly fulfilled the requirements, that a prize was offered by the Society of Arts for the discovery of a substitute in this country; but without success, and since the peace the French stone has again been extensively imported. The burr is a peculiarly hard kind of flint. It is known to French mineralogists under the names of "Silex molaire" or "Pierre meulière;" and according to M. Guibourt,* who appears to have derived his information from Brongniart, is found in certain tertiary strata of lacustrine or fresh-water origin. It is situated in the Paris basin above the gypsum containing bones, and in strata of sand and sandstone. It occurs either in beds or in smaller masses, and the beds are sometimes so large as to admit of two or even three millstones being cut from the same piece, but never more. They are of no great thickness; are generally cellular, and the cells are irregular, and sometimes contain plates or fibres of silex. The stone is more tenacious and less easily broken than gun-flint; it is slightly translucent, and is generally of a tarnished whitish, yellowish, or reddish colour. It is obtained in open quarries, and in removing it grooves are cut, into which wedges of iron or wood are driven, so as to break off portions of the required size. According to Brongniart, the best stones are obtained from La Ferté sous Jouarre on the Marne, to the east of Paris, where the trade has been carried on for some centuries. The stones which are of a bluish-white colour, and do not contain fossils are the best. The smaller portions are cut into *carreaux*, or quadrangular blocks, and are extensively exported to England and America.

Though the masses sufficiently large to form entire stones are occasionally brought to this country, it is usually in the form of the smaller blocks, measuring about fifteen or sixteen inches in length, ten or eleven in breadth, and six or seven in depth, that the burr is seen in the millstone-makers' yards. The stone is not only brought from La Ferté sous Jouarre, but also from the neighbourhood of Épernon, to the south-west of Paris, and it is said to exist extensively in other parts of France.† The stones are of a yellowish brown colour, and are more or less thickly studded with small cellular or tubular spaces of irregular form, and lined by a yellowish powder. They are stated sometimes to contain fossil shells,—different quarries differing in this respect. The stones also vary in the degree of translucency.

* Drogues Simples, 4th edition, vol. i. p. 100. 1849.

† According to Brard (Minéralogie Appliquée aux Arts, Paris, 1821), the silex molaire is obtained at Tarterai près de la Ferté sous Jouarre, Département du Seine et Marne; Domme, Département de la Dordogne; Ferté sur Loire, près de Nevers, Département de la Nièvre; and Saint Maixent, Département des deux Sevres.

From the "carreaux," or blocks, the millstones are manufactured in this country by cutting their surfaces into angular forms, so as to fit them together, cementing the portions with plaster of Paris, and binding the whole with strong bands of iron round the circumference; grooves, radiating from the central opening towards the circumference, and about an inch and a half apart, are then cut on the grinding surfaces.

The rough working of the stones is effected by a steel chisel, "the pritchell," which is struck by a metal hammer, and the surfaces are finished by picking with a double-pointed steel instrument fixed in a wooden handle—the "bill and thrift." As the burr is extremely hard, every stroke of the chisel is attended by a bright flash of light, and a cloud of dust and larger or smaller particles of stone, forming a sharp grit, are thrown off. Portions of the stone and of iron from the chisel, not unfrequently become embedded in the hands or face of the workmen, so that the backs of the hands of those who have been long at the trade are studded with small bluish spots, and occasionally the men sustain serious injuries to their eyes.

The men who are engaged in this work universally regard it as a very injurious occupation, and state that the burr is a much more dangerous stone to work than the Yorkshire or Derbyshire grit, the Scotch granite or the German basalt, which are also employed for the formation of millstones.* A very intelligent young man, the foreman of one of the yards, who informed me that he had been bred to the trade, stated that he had known at least twenty persons die of chest affections, out of the small number, not, I believe, exceeding fifty, who are employed at the work in London. He also told me that the oldest workman he knew had only been in constant employment at the trade for thirteen years, and that all who work at it suffer sooner or later. One of the masters informed me, that those who are apprenticed to the trade, or take to it early in life, never live beyond forty years; and one of the foremen said they seldom do more than live out their time. These statements are confirmed by my own observations during visits made to the yards in the spring of 1859; I found in three out of the four London shops, forty-one men at work, of whom twenty-three stated that they had been apprenticed to the trade, or had taken to it when they were not above twenty years of age. Of this number the average age was 24·1 years, and the ages of the five oldest workmen were 38, 29, 29, 28, and 28. The average period during which they had worked at the trade was 8·9 years, and the three who had been longest engaged had been 18, 17, and $14\frac{3}{4}$ years. It also appears that it is common for the men to leave the millstone making and work for a time as millwrights or engineers, so that it is quite possible that some of the twenty-three may not have been in constant work as millstone builders; but however this may be, the age of the men was certainly very low, and their period of work short. The difference in these respects is indeed very remarkable between the men

* The burr is only used for grinding wheat, and of cement-stones, and for these purposes its extreme hardness is said especially to adapt it. The other stones are used for rice, barley or beans.

employed as millstone-makers, and those who, in some of the same establishments, are engaged in wire-weaving, &c., for the formation of sifters for flour, for drying the pulp in the manufacture of paper, and for other purposes. In one establishment I found nineteen men so engaged; of these, thirteen informed me that they had been apprenticed to the trade, or had joined it at or before twenty years of age. Of this number the mean age was 35·84 years, and five of them had attained the ages of 71, 43, 42, 40, and 40. The mean period during which they had worked at the trade was 20·69 years, and eight of them had been engaged for 51, 32, 29, 25, 25, 24, 24, and 22 years. These men were certainly less healthy-looking than the millstone-makers; but they stated that they enjoyed good health, and this was confirmed by the very small number who had received relief from the sick fund during the period of five years, of which the accounts were shown me. The contrast between the different classes of workmen is the more striking, in that, so far as most circumstances are concerned, the metal-weavers are in less favourable sanitary conditions than the millstone-makers; for their workshops are some of them underground and close; but I understood that they are steadier in their habits, and that their employment is more regular.

My attention was first attracted to the prevalence of pulmonary disease among the French millstone-makers by the number who applied to me at St. Thomas's and the Victoria Park Hospitals, and before I had made any inquiries at the shops which might bring the men more particularly under observation. I feel therefore convinced that the occupation is one which predisposes to pulmonary affections; but it is open to inquiry in what way it exercises an injurious influence—whether it be, as supposed by the men themselves, from the dust which they breathe, or from the influence of other causes which rather operate by deteriorating their general health.

When I visited the shops during the spring, they were certainly dusty, though, from the dampness of the weather, it was said less so than usual. There can also be no doubt that the dust is extensively inhaled; for in a case the particulars of which will shortly be detailed, siliceous particles were found in consolidated portions of lung. It is evident that such particles lodged on the mucous membrane of the smaller bronchial tubes or in the cells of the lungs must be a serious source of irritation, tending in persons of healthy constitution to the production of chronic bronchitis and asthma, and in those inheriting a constitutional predisposition to phthisis, to the development of tubercle.

Other causes doubtless conduce to the unhealthiness of the occupation. Thus, though, as a general remark, the workshops are sufficiently roomy and protected from the weather, in some cases they are very defective. In one yard, some of the men were at work in underground cellars, which, though freely open above, must be damp and unwholesome; and others occupied open sheds, where they must be much exposed to the weather. The want of general exercise is also objectionable. The men work at the stones standing up or leaning over them, and, except in their arms, use little muscular exertion, and their chests cannot be well expanded.* Some of the men also habitually take an amount of stimulus which must be very injurious. They state that their occupation is an exhausting one, and they in consequence drink a large quantity of beer. Four or five pints is, I believe, by no means an unusual quantity, and some take spirits also. One of the masters, a fine healthy-looking middle-aged man, who said that he had worked at the trade for many years, and had always enjoyed good health, ascribed his immunity from the usual effects to his temperate habits, and stated that if the men lived temperately they suffered much less. In his yard he allowed the workmen a pint of beer morning and afternoon, but interdicted all going to the public-house, or having beer brought upon the premises; and stated that his men were in consequence healthier than in other yards. This statement is confirmed by the fact, that though his shop is not far from St. Thomas's Hospital, and several of his men have applied to me for other ailments, I have seen no case of phthisis or chronic bronchitis among them.

So far as the ordinary necessaries of life are concerned, the millstone-makers are generally favourably placed. They earn good wages, being paid at the rate of 6d. per hour, or 5s. a day, or by piecework, at which they can earn still more. They are well clad, and live well. The occupation is also a tolerably certain one, but the men may occasionally be thrown out of work, and so suffer privation.

The causes which have been named do not therefore appear sufficient to explain the great tendency to pulmonary affections among the millstone-makers, apart from the injurious influence which is exercised by the gritty particles of silex which they inhale while at work. This is indeed, I believe, the main cause of their sufferings.

The following cases afford examples of pulmonary disease in millstone builders:

CASE 1.—J. S., aged twenty-three, was admitted as an out-patient at St. Thomas's Hospital, on the 7th of October, 1858. He then stated that he had suffered for twelve months from an affection of the chest, but had enjoyed good health before that time. His illness commenced with spitting of blood, which, however, was not to any great extent. The hæmoptysis recurred nine months after, and he was expectorating blood at the time he applied at the hospital. He then complained also of difficulty of breathing, pains in the chest, severe cough, and expectoration. His appetite was defective, but his digestion was good, and his bowels regular. He was pale, thin, and phthisical-looking. His father and mother and all their family, consisting of twelve besides himself, were healthy, except a brother, a millstone-maker, who has been under my care for symptoms of consumption.† A third brother, also a millstone-maker, has worked at the trade eleven years without suffering. He himself went to the work when about sixteen years of age, and he has continued at it regularly, except five years ago, when he

* It is probable that the powerful muscular exertion which is undergone by the wire-weavers is one cause of their freedom from the affections which prove so fatal to the millstone-makers.

† He has since died.

left it for a year, and worked as an engineer in a flour mill. He has constantly been employed in working the French burr. He regards himself as having been a temperate man, and has only been in the habit of taking three pints of beer daily, and has rarely committed an excess. On examining the chest there was found to be some deficiency of the resonance on percussion at the left apex, with obscure tympanitic resonance at the right; bronchial respiration and crepitation at the left apex, and cavernous respiration and rhonchus at the right. He was at first directed to take the cinchona and acid mixture, with compound tincture of camphor during the day, and hyoscyamus and Dover's powder pills at night. Subsequently cod-liver oil was prescribed. He continued regularly under care during the winter, and though he thought himself better, steadily lost ground, and on the 3rd of March, 1859, in consequence of his declining strength having rendered him incapable of work, he was admitted as an in-patient into Jacob's Ward. The physical signs then indicated more advanced and extensive disease; his cough was very troublesome, the expectoration profuse and occasionally bloody, and the dyspnœa greater. He had profuse night sweats, his pulse was quick and weak, and he complained of pain in the right side. There was marked dulness on percussion at the lower part of that side. His appetite was very defective, and he could take little food, and was much prostrated and emaciated. He died exhausted on the 20th.

The post-mortem examination was performed by Dr. Bristowe, who took the following notes:—The backs of the hands, especially in the neighbourhood of the knuckles, were thickly studded with papulæ, containing in their interior a small blackish-looking encysted mass, which proved to be an elongated angular fragment of stone or iron, varying from the size of a poppy seed downwards. The left pleura presented a few old adhesions. The lung was voluminous, for the most part crepitant, and indeed somewhat inflated. The upper lobe presented numerous grey miliary tubercles, which had a tendency to run together and form irregular indurated patches of various sizes. The apex and upper third of the lower lobe were similarly affected. The right pleura at the extreme apex, and to some extent behind and at the base, was firmly adherent, but the rest of its extent formed a cavity, lined by a pulpy pus-infiltrated lymph, and containing between three and four pints of healthy-looking pus. The lung was very much reduced in size, compressed, and perfectly airless. It was studded thickly throughout with grey miliary tubercles, and in its apex contained several cavities, more or less freely communicating with one another, and of considerable size. There were two or three distinct and rounded orifices, from one-third of an inch in diameter downwards, by which they communicated with the empyema. The bronchial tubes of both lungs were somewhat congested, and contained a large quantity of purulent fluid. There was slight redness and excoriation of the larynx over the anterior process of the left arytenoid cartilage, but the larynx and trachea were otherwise healthy. Many of the bronchial glands were studded with tubercles, and infiltrated with black pigment. The pericardium and heart were healthy. The peritoneum, with the liver, spleen, pancreas, supra-renal capsules and kidneys, displayed no appearance of disease. The stomach and intestines were healthy, except that a few tubercular ulcers existed in the ileum and cæcum.

CASE II.—W. C., aged thirty-seven, applied as an out-patient at St. Thomas's Hospital, on the 11th of November, 1858. He stated that he laboured under symptoms of affection of the chest for five years, very severely during each winter, but less so in summer, though he was never entirely free from difficulty of breathing and coughing. He suffered extremely in damp and foggy weather, and got worse each winter. His illness, he says, was first brought on by his having got wet through and having allowed his clothes to dry on him; but he thinks that he has been made much worse by the dusty atmosphere which he breathes in his trade of millstone-maker. He was brought up as a millwright, but took to the French millstone-making when about twenty years of age, and he has continued at that trade ever since, except that he used to leave it for a month or six weeks during the winter, when work was slack, and act as an engineer. For the last five years, however, he has been constantly employed as foreman of one of the largest millstone-making yards, and he believes himself to be the oldest man in the trade in London. He has always been a "middling steady man," but used formerly to take spirits and much beer; he now takes four or five pints of beer daily, and occasionally more, but never drinks spirits.

He complained chiefly of difficulty of breathing, cough, and expectoration. His face was tumid, and somewhat livid; the expectoration chiefly troubled him in the morning, and was occasionally streaked with blood. His breathing was extremely difficult during foggy weather. On examining his chest, the resonance on percussion was found to be somewhat abnormally clear in the mammary and impaired at the clavicular regions. The inspiratory sound was generally feeble and the expiratory loud and prolonged, and bronchitic rhonchi were heard in all parts. The backs of his hands were covered with black spots, which he said were produced by the pieces of iron thrown off from the chisel in millstone-making. He stated that the men employed in working the "French burr" all suffer with coughs and asthma sooner or later, and this he ascribes to the sharpness of the dust which is thrown off in striking the stone; and he says that the workshops are very dusty. From this time he continued to attend at St. Thomas's Hospital till the spring of 1859; he presented the usual symptoms of chronic bronchitis, but occasionally spat blood, though never in large quantity. In April, feeling himself better, he left me, and I did not see him again till I heard he was very ill, and visited him at his home on the 15th of November. I then ascertained that though he had been free from his asthmatic symptoms during the summer, he had never obtained the usual amount of relief; and when the cold and damp weather of the autumn set in, he got much worse. When seen, he had been incapable of following his occupation for six weeks; he was suffering from dyspnœa, so urgent that he was incapable of lying down in bed. The expectoration was very dark, almost black, and was viscid and airless, and very profuse; his pulse was feeble and somewhat irregular, and there was œdema of the ankles. Bronchitic rhonchi were heard in all parts of the chest, and the resonance on percussion above in front was impaired. I again saw him with Mr. Wilson, of Cecil-street, his private medical attendant, on the 19th; he was then obviously sinking rapidly. His lower extremities had become very anasarcous, and his intelligence was much impaired; he died on the evening of the 23rd.

I am indebted to Mr. Hughlings Jackson for the performance of the post-mortem examination, and for the following statement of the facts disclosed. The right lung was everywhere adherent to the parieties, and was removed with difficulty; the left was only attached at its apex. Both lungs were tuberculous, but the right especially so, and the deposit was so copious at the apices as to render them solid. The tubercles existed in masses, with interposed pulmonary tissue comparatively sound. The lower lobe of the left lung had merely here and there small miliary tubercles, which were especially superficial at the base. In the right lung there were two small irregular cavities containing pus, one in the apex and the other in the centre of the lung, and each about the size of a walnut. The bronchial glands were very large, hard, and of a deep black colour. The pericardium was healthy, the heart was flabby, and its cavities dilated, so that when emptied, it was smaller than usual; there was no valvular disease; but a white patch existed on its anterior surface. The kidneys were unusually large, but healthy; and the liver and other abdominal organs were apparently free from disease.

Portions of the indurated pulmonary tissue, and of the diseased bronchial glands, were obligingly examined by Dr. Bristowe, who has furnished me with the following notes:—"The diseased portions of lung were much indurated, having generally an opaque whitish hue, but being thickly studded with black pigment. Under the microscope little or no trace of original lung-structure was visible, but the diseased masses appear to be made up of dense closely-arranged fibroid tissue, studded here and there with numerous irregular groups of black pigment, and generally with an abundance of transparent granules and globules of various sizes. The tissues were rendered comparatively transparent under the influence of strong acetic acid, the fibroid tissue becoming a little expanded, and many of the granules and globules disappearing. The bronchial gland presented characters identical with those of the diseased lung."

Portions of the indurated lung-tissue were subjected by myself to ignition in the flame of a spirit-lamp. The white ash which remained dissolved to a great extent in hydrochloric acid, and partly with effervescence; but a portion was left which was seen under the microscope to consist of small angular transparent granules, exactly resembling the finer portions of the siliceous dust collected from one of the workshops. Dr. Moldenhauer, assistant in the chemical laboratory at St. Thomas's Hospital, also subjected portions of the indurated lung to the action of fire and nitric acid, and found that a considerable quantity in gritty matter remained, which had an amorphous aspect under the microscope, and was inferred to be siliceous. The bronchial gland did not contain any similar material.

The cases which have been detailed do not appear materially to differ in their symptoms from similar forms of disease originating under other circumstances. The first affords a sufficiently characteristic example of tubercular phthisis; the second of chronic bronchitis lapsing into phthisis. It is, however, possible that closer observation of other cases of disease in the same class of persons, may lead to the detection of some special facts by which they are characterized. It seems also probable that in persons who become phthisical from being exposed to local irritation from the inhalation of particles of stone or metal, the disease will be slower in its progress, and be preceded by more marked symptoms of faucial and laryngeal irritation, than under other circumstances.

So far as we know at present, the disease induced by this cause does not admit of any special treatment, but our attention must be mainly directed to prevention, and there can be no doubt that in this way much may be accomplished.

1st. It would appear that much of the deleterious influence of French millstone-making may be obviated by not allowing persons to enter the trade till after they have attained their full growth and vigour of constitution. As I have before mentioned, one of the foremen said that when boys were apprenticed to the trade they scarcely do more than live out their time; and though this may be somewhat too strongly expressed, it appears to be mainly true, for one of the masters said that he had declined to take apprentices, from the number of persons whom he had known die of consumption when put to the trade early. On the other hand, all the information obtained tended to show that the work is much less injurious to those who take to it when more advanced in life. I found at work several middle-aged men who had been some years at the trade without suffering, and one man of fifty-four told me that he had taken to the work when thirty-four years of age, had continued at it for twenty years without suffering, and was then in good health.

2ndly. The men should be cautioned to be careful to protect themselves against the usual causes of cold, by wearing suitable clothing, and especially to avoid all excess in the use of stimulants. Unfortunately, men engaged in trades which are known to be injurious are apt, partly perhaps from recklessness, but partly also from ignorance, to be dissipated in their habits; we are informed that such was the case with the millstone-makers of Waldshut, and with the needle-pointers of Worcestershire; and the intemperance of the dry-grinders of Sheffield is notorious. The French millstone-makers have often told me that they require a large quantity of stimulating beverage, and that if a man is abstemious he dies all the sooner; and this is made the excuse for their taking a very immoderate amount of stimulus. There can be no doubt that habits of intemperance very greatly aggravate the other evil influences to which they are exposed, and that were they to live temperately, but well, they would suffer less, and live much longer. I cannot but think that representations of this kind would have much weight with the men ; and I am informed that their habits have latterly improved, and with advantage to their general health.

3rdly. Much may probably also be done to mitigate the injurious effects of the work by lessening the liability of the men to take cold from exposure to the weather, by reducing the quantity of dust thrown off and diffused in the atmosphere, and protecting the workmen against its inhalation. The workshops should be roomy and well ventilated, and should admit of being freely opened in dry and warm, and closed in damp and cold, weather. Working in underground cellars or in the open air are both objectionable. The men should be advised to work as much as possible in the upright position, instead of stooping over the stones, so as to expose themselves as little as possible to the inhalation of the dust. The stones might, perhaps, be equally well worked wet instead of dry, and thus much less dust would be thrown off; and the only objection to this which I have heard stated is, that the tools would the sooner wear out. Lastly, they might avoid the inhalation of the dust by wearing respirators, either at all times, or when the shops are more particularly dusty.

PART FOURTH.

Chronicle of Medical Science.

HALF-YEARLY REPORT ON PHYSIOLOGY.

By Hermann Weber, M.D.
Fellow of the Royal College of Physicians, Physician to the German Hospital.

I. Food and Digestion.

1. Hayes, T.: *Observations upon the Relations existing between Food and the Capabilities of Man to Resist Low Temperatures.* (American Journal of Medical Sciences, July 1859, p. 114.)
2. Smith, E.: *Practical Deductions from an Experimental Inquiry into the Influence of Food.* (Proceedings of the Royal Med. and Chir. Society, vol. iii., p. 82, 1859.)
3. Mulder: *The Peptons.* (Archiv f. Holländ, Beiträge z. Natur. u. Heilkunde, vol. ii., p. 1; and Meissner's Bericht über d. Fortschritte d. Physiol. in 1858, in Zeitsch. f. rat. Med., vol. vi., p. 204, 1859.)
4. Meissner: *Researches on the Digestion of Albuminous Substances.* (Zeitsch. f. rat. Med.; Third Series; vol. vii., p. 1, 1859.)
5. Lehmann: *On the Mineral Nutritive Constituents, especially on the Earthy Phosphates as Nutriments of the young Animal Organism.* (Annal. d. Chemie und Pharm., vol. cviii., p. 357; and Meissner, l. c., p. 217.)
5. Keferstein and Hallwachs: *On the Action of the Pancreatic Juice on Albumen.* (Göttinger Nachrichten, 1858, No. 14; and Meissner, l. c., p. 207.)
7. Corvisart: *On the Function of the Pancreatic Juice.* (Zeitsch. f. rat. Med.; Third Series; vol. vii. p. 119, 1859); and *Contribution to the Knowledge of the Function of the Pancreas.* (L'Union Méd., 1859, No. 87.)
8. Brinton: *On the Action of the Pancreatic Juice on Albumen.* (Dublin Quarterly Journ. of Med. Science, August, 1859, p. 194.)

The communication of Dr. Isaac Hayes contains facts of great importance regarding the value of various articles of food under the influence of low temperatures. The author was one of the surgeons to the second U.S. Grinnell Arctic Expedition on board the *Advance*, and spent the years 1853, '54, and '55 on the western coast of Greenland, in latitude 78° 37′. There he had the opportunity of closely watching the manner of living of a tribe of wandering Esquimaux. These form, he says, a strong, robust, and healthy race. After giving a graphic description of their huts of ice or snow during midwinter, where they have scarcely any fire, and are comparatively only slightly dressed, the author points for the explanation of this wonderful power of resistance to the quantity and quality of the food consumed. "They subsist entirely upon animal food—the flesh mainly of the walrus, seal, narwhal, and bear; and the quantity which they eat seems really enormous. I have frequently seen an Esquimaux hunter, when preparing for the hunt, eat from six to twelve pounds of meat, about one-third of which was fat; and I should place the daily consumption of the men at from twelve to fifteen pounds; and in this large consumption they find their shield against the cold. This food is mostly taken raw, and in their long journeys they stop from time to time, unlash their sledges, and cutting off strips of frozen blubber, eat them with apparent relish." "The same laws," the author continues, "govern the Esquimaux and the white men; and just in proportion as we of the *Advance* accustomed ourselves to the diet of the Esquimaux, did we gain power to expose ourselves with impunity to low temperatures. We found ourselves continually craving animal food, and especially fatty substances, which to us in these latitudes would be exceedingly distasteful. Frozen blubber became quite palatable. . ." "The process of acclimatation with us," he continues, "was gradual. I remember well how, in the autumn of 1853, we suffered intensely from temperatures which a year later produced no impression whatever upon us, and I am satisfied that this increased power of resistance was in direct proportion to our ability to eat and digest animal food." Hayes' experience corroborates the view that salt meat is much less nourishing, and promotes scurvy. Of raw meat, on the contrary, especially when frozen, he speaks very favourably. The operation of freezing destroys entirely the repulsiveness of the raw flesh, and it is even generally thus preferred by the sick.

The regular use of alcohol is considered as not only unnecessary but even injurious in the Arctic regions. Even as a stimulant, the author places it vastly below tea and coffee. "They both operated upon fatigued and overtasked men like a charm, and their superiority over alcoholic stimulants was very marked."

The importance of the physiological questions connected with the facts contained in Hayes' communication will be a sufficient excuse for our having given the preceding extracts and quotations.

Dr. Edward Smith read an interesting paper on the value of various articles of food before the Royal Medical and Chirurgical Society, on May 10th, 1859. He contrasted the action of starch (arrowroot and similar substances) with that of cereals; the former as being unable to sustain life; the latter as possessing that quality in an eminent degree. Beef tea, wines, and brandy he described as merely tending to prevent loss of vital power; milk, analogous to the cereals, as really increasing the amount of vital power. Referring to the essay itself for the author's view regarding fat, sugar, and nitrogenous substances, we mention, with respect to tea, that it was considered as causing increased waste, and as exciting every function of the body, and hence well fitted to cases where there was a superfluity of material in the system, or where we otherwise desire to induce a temporary increase in the vital action; but injurious to those who are under-fed, or in any case where there is greater waste than supply." In illustration, the author cited the increase in the loss of weight in the prisoners at Wakefield when tea was added to their food. He suggested the use of tea "instead of spirituous liquors, by soldiers on march, or otherwise exposed for a lengthened period to great heat; since, by its powerful influence in increasing respiration and the action of the skin, without increasing pulsation, it was particularly fitted to counteract the influence of heat in its tendency to induce heat-apoplexy, or as more suitably termed by Mr. Longmore, 'heat-asphyxia.'" • Smith considers the action of tea as differing from that of coffee, by the action of the former on the skin, "and thereby tending to cool the body."

All alcohols the author described "as having their chief influence in sustaining the action of the heart," and recommended that they should be given in small quantities and often repeated, so as to accumulate their action, rather than allow reaction to follow each dose by permitting a long interval between the single doses. He is of opinion "that alcohol increases the respiratory action indirectly through the nervous ystem, and that in fine old wines and spirits this action is lessened by the volatile elements, which have a conservative tendency."

Mulder compares the peptons obtained by the action of artificial gastric juice on proteinaceous bodies, with the solutions of these bodies obtained by merely diluted acids. For the preparation of the artificial gastric juice we must refer to the essay itself. As characteristic relations of the peptons, Mulder mentions that they are not precipitated out of a weakly acid solution, by means of boiling alcohol, nitric acid, carbonate of ammonia, neutral acetate of lead, yellow ferrocyanide of potassium and sulphate of soda; precipitation, on the contrary, is effected by tannic acid, and also by bichloride of mercury, when the hydrochloric acid has been previously neutralized and acetic acid added. Under the influence of Millon's reagent the peptons assume a red colour. The author's experiments with gluten show that it is only in part dissolved by the acid alone, but that it is entirely transformed into peptons by the digestive fluid. The legumin was changed into peptons, as well by the acid alone as by the digestive fluid. Mulder is inclined to think that during digestion part of the legumin is transformed into pepsin, and assists therefore in the solution of proteinaceous bodies. Fibrin is easily transformed into pepton by the digestive fluid, and by the mere acid, too, a part of the fibrin becomes pepton. Casein, like legumin, is changed into pepton by the diluted acid alone. No true pepton was derived from gelatine, either by means of the digestive fluid or of the diluted acid. The peptons are, according to Mulder, a mixture of various bodies; he could distinguish one which is insoluble in water, one which is soluble in alcohol, and one which is insoluble in boiling alcohol.

Meissner experimented with artificial gastric juice, which had the peculiarity of being prepared by means of the pepsine of shops with diluted hydrochloric acid. He found that by the digestion of albumin, casein, fibrin, syntonin, and gluten, besides the pepton, another similar but not identical body is formed, which he calls parapepton, and which is at once recognised by the fact that it is precipitated in delicate white flakes out of the solution of the albuminous substances, by carefully neutralizing the acid contained in the digestive fluid, while the true pepton remains dissolved in it. For the detailed description we refer to the original, adding here only that the parapeptons are not soluble in water, but are so in acids and alkalies, and that they are precipitated out of an acid solution by neutral salts. Respecting the proportion of the parapeptons to the peptons, Meissner obtained from a solution of the white of egg not quite twice as much of peptons as of parapeptons, and a similar proportion from a solution of boiled beef.

J. Lehmann experimented on a healthy calf, about five months old, with a view to see whether the alkaline earths and the phosphoric acid contained in the food of young animals are entirely absorbed, and whether the admixture of earthy phosphates in a finely powdered state leads to the absorption of an increased quantity of these substances. The food of the calf taken within forty-eight hours contained 49·062 grammes of lime, 24·492 grammes of magnesia, and 78·334 grammes of phosphoric acid. The fæces of the same period contained 28·320 grammes of lime, 18·396 grammes of magnesia, and 30·030 grammes of phosphoric acid; the urine contained only traces of lime, 4·387 grammes of magnesia, and 12·042 grammes of phosphoric acid. There were therefore retained in the body, 20·742 grammes of lime, 1·709 gramme of magnesia, and 36·262 grammes of phosphoric acid. During the two following days the calf received, in addition to the same quantity and quality of food, 25·694 grammes of earthy phosphates from calcined bones, being composed of 8·570 grammes of lime, 0·086 gramme of magnesia, 10·930 grammes of phosphoric acid, and 6·108 grammes of water; the ingestion of earthy phosphates amounted therefore to 57·632 grammes of lime, 24·578 grammes of magnesia, and 89·264 grammes of phosphoric acid. Of these remained in the body, 26·776 grammes of lime, 1·725 grammes of magnesia, and 32·024 grammes of phosphoric acid. The admixture of the earthy phosphates had therefore caused an increased retention in the body of 6·034 grammes of lime, 0·016 gramme of magnesia, and 5·785 grammes of phosphoric acid. We learn from this, that although the usual food contained a great surplus of earthy phosphates over the quantity contained in the body, yet the admixture to the food of powdered earthy phosphates from calcined bones caused a considerable increase of absorption of these salts, especially of the lime and phosphoric acid, while the increase in the retention of magnesia is very small.

While Keferstein and Hallwachs deny completely the correctness of Corvisart's conclusions* regarding the influence of the pancreas on the proteinaceous constituents of food, Meissner's experiments corroborate the results obtained by Corvisart, with certain restrictions. Meissner arrives at the inference "that the pancreatic juice is able to digest albuminous substances, and that it transforms them into bodies closely allied to the peptons, provided the experimental fluid (viz., the infusion of the pancreas) be prepared from animals which were killed while in the act of digestion, provided further the experimental fluid be of acid reaction." The former of these conditions is quite in accordance with Corvisart's own observation, the latter deviates from it. The discoverer of this important function of the pancreas has in the mean time continued his researches, and from experiments made in the presence of Kühne and Snellen, Corvisart is enabled again to assert—1. That the mixed liquid poured into the duodenum (viz., bile, pancreatic and intestinal juice), with exclusion of the gastric juice, digests albumen; 2. That coagulated albumen can be digested in large quantity by the infusion of the pancreas alone, by an action peculiar to this, without the intervention of the intestinal juice, bile, &c. The causes of the failure of Keferstein and Hallwachs the author sees in the facts that these gentlemen operated partly with pancreatic juice obtained through a fistula of eight days' standing, and partly with infusion of pancreas taken from animals which were not in the act of digestion. With regard to the time in which the pancreas is most active, Corvisart states that in a young healthy dog it is about five or six hours after a mixed meal, that at an earlier period the juice does not yet possess its full activity, while at a later, part of its activity is already exhausted: that the pancreas therefore commences its action when the stomach has almost completed its work. With respect to Meissner's assertion—that the experimental fluid must be of acid reaction, Corvisart's further experiments prove that the quantity of albumen dissolved is almost the same, whether the fluid is acid, neutral, or alkaline, and that also the mixture of fluids contained within the duodenum was sometimes acid, sometimes alkaline, sometimes neutral, and, that in spite of this difference of reaction, the action on albumen appeared to be very similar.

Dr. Brinton likewise communicates the results of some experiments made concerning the solution of coagulated albumen by infusion of the pancreas. He finds that the influence of pancreatic juice on coagulated albumen is very variable, that in some instances it dissolves a large quantity, in others a very small one; that again, in others no albumen is dissolved. Brinton mentions two important differences as existing between the solution of albumen by pancreatic infusion and that by the artificial gastric juice. "One is," he says, "that in most cases, and for equal proportions of albumen, the pancreatic solution is a slower process, occupying from two to six times as long a period. The other is, that it is accompanied by unmistakeable putrefaction." The author, however, adds, "that an energetic solution is often seen to occur, when as yet none but a peculiar penetrating quasi-syrupy odour is discernible in the mixture." He further allows "that there is often a singularly close relation between the quantities of albumen which can be dissolved by a given weight

* Conf. this Journal, No. xxxix. p. 240; and No. xliii. p. 224.

of gastric mucous membrane and pancreas, or pepsine and pancreatine respectively. The numerical proportions are frequently almost identical." Regarding the influence of acids and alkalies, Brinton states, that in proportions of $2\frac{1}{2}$ to 3 per cent. hydochloric acid often hinders the solution of albumen; while the presence of alkali is sometimes indifferent, even injurious to the solvent power, in general, however, considerably exalts it, "an exaltation which usually concurs with a heightened putrefaction." Finding that the pancreas of the ordinary human corpse showed a solvent power transcending that of healthy newly-killed animals, the author compared the action of a perfectly fresh infusion with that of infusions in successive stages of putrefaction. These experiments demonstrated that infusions in a state of more or less progressed putrefaction exhibited a much greater power of dissolving albumen than perfectly fresh infusions, which appeared to have no such power whatsoever. According to the temperature of the air being higher or lower, the period at which the infusions acquire the faculty of dissolving albumen will arrive sooner or later. Although, however, Brinton is led to believe, that putrefaction promotes the influence of the pancreas in question, yet he allows that "whatever the exact nature of this process, it seems to be quite specific to the pancreas." Yet the author declares Corvisart's assumption—that the pancreas discharges in the healthy living body the function of dissolving albuminous substances—a hasty deduction. Contrasting the solution through the pancreatic infusion with that effected by artificial gastric juice, he attributes to the former a "slow, dead, irregular, putrefactive character," while he considers the influence of the gastric juice as rapid, energetic, and antiseptic.

As the subject is one of great importance, and as it still remains *sub judice*, as, farther, the manner in which the experiments are made is evidently of great influence on the result, we ought to mention that Brinton does not say whether the specimens of pancreas used in his experiments were taken from animals killed during the process of digestion or not; and yet, according to Corvisart and Meissner, this difference is essential.

II. Blood; Circulation; Respiration; Animal Temperature.

1. Davy, J.: *Some Observations on the Coagulation of the Blood.* (Transactions of the Royal Soc. of Edinb., vol. xxii., part 1.)
2. Rouget: *Remarks on the Existence of Coloured Blood-globules in Several Species of Invertebrate Animals.* (Brown-Séquard's Journal de la Physiologie, vol. ii., p. 660, 1859.)
3. Hammond: *Observations on the Colourless Blood-corpuscule.* (Americ. Journ. of Med. Scien.; No. lxxiv., p. 348, 1859.)
4. Bernard: *On the amount of Oxygen in the Venous Blood of the Glandular Organs during their state of Activity and of Rest.* (Archiv f. Anat. und Physiol.,* Jahrgang 1859, p. 412.)
5. Mitchell: *Notes upon the Effect of Alcohol, Glycerine, Water, Gum, Ammonia, and the Vacuum upon the exposed Hearts of Frogs, Snapping-Turtles, and Sturgeons.* (Amer. Journ. of Med. Scien., No. lxxiv., p. 343, 1859.)
6. Busch, W.: *On the Retrograde Movement of the Blood in the large Veins adjacent to the Heart.* (Verhandl. d. Naturhist Vereins d. preuss-Rheinland; and Canstatt's Bericht. über d. Forschritten in d. Physiol. in 1858, p. 53).
7. Müller, W.: *Contribution to the Theory of Respiration.* (Annal. d. Chemie und Pharm., vol. lviii., p. 257; and Schmidt's Jahrbücher, vol. cii., p. 156 1859.)

* This is the late Müller's Archiv, and is at present edited by Reichert and Du Bois-Reymond.

8. Fernet: *On the Absorption and Emission of Gases by Saline Solutions and by the Blood.* (Compt. rend., 1858; and Canstatt's Bericht, l. c., p. 146.)
9. Brown-Séquard: *Researches on the Influence of Change of Climate on Animal Heat.* (Brown-Séquard's Journ. de Physiol., vol. ii., p. 549, 1859.)
10. Budge: *On the Influence of the Pneumogastric Nerves on the Respiration.* (Sub. VI.)
11. Einbrodt: *On the Influence of the Pneumogastric Nerves on the Movements of the Heart.* (Sub. VI.)

J. Davy having performed various experiments regarding Richardson's view that the cause of the coagulation of the blood is referable to the escape of ammonia, failed to discover the escape of volatile ammonia during the coagulation of the blood of the fowl, and by the addition of liquor ammonia in smaller or larger quantities to the blood, he was unable to prevent coagulation. Of the sesquicarbonate and carbonate of ammonia the author says, that "used in small quantities they have the effect of retarding coagulation, analogous in this their influence to most of the salts of the alkalies." A set of experiments, having for their object to ascertain what is the amount of loss of ammonia from evaporation, led Davy to believe that no appreciable quantity can escape in the short period of two or three minutes, which is about the time required for the coagulation of the blood of the fowl. Another set, designed to ascertain the solvent power of the volatile alkali on fibrin, led to the conclusion "that the volatile alkali, in its action on fibrin, is chiefly remarkable for rendering it viscid, and that its solvent power is inconsiderable." The author concludes his essay by remarking that as the results of his experiments "are opposed to the inference of the coagulation of the blood being due to the escape of any volatile matter, and a portion to the volatile alkali, the existence of which in the blood even remains to be proved, they leave the phenomenon, as hitherto, a problem for solution and open to question, whether it be the result of loss of vitality, or a chemical result depending on a new arrangement of elements of the coagulating part the fibrin, without, as regards their numbers, any change of their sum."

While it is almost generally believed that the invertebrate animals contain only colourless blood-globules, Rouget found in the blood of numerous species of mollusca and radiata coloured globules. Thus after having described the bloodvessels in the mantle of the *Ascidia venosa*, he gives the following account concerning the blood contained in these vessels: "In these vascular channels circulate, under the impulse of the heart, large corpuscles of $0{\cdot}010^{mm}$ to $0{\cdot}015^{mm}$ diameter, round or oval, of mulberry-like surface, and formed by an agglomeration of small globules, of $0{\cdot}002^{mm}$ to $0{\cdot}003^{mm}$ diameter, the single ones firmly pressed against each other, and appearing surrounded by a common cellular membrane. These corpuscles exhibit a uniform bright red colouration, which remains unchanged by ether, alcohol, and water. Acetic acid and diluted mineral acids render the corpuscles pale, but still possessed of a faint orange-yellow hue; ammonia, as well as a concentrated solution of potassa, destroys the colouring matter." No nucleus was discovered. The fluid in which these corpuscles float is colourless, and it contains also white corpuscles, but these in smaller quantitiy than the red ones. In the *Ascidia viridescens*, the blood of which has a milk-white, slightly yellowish appearance, the author found rather large cells, containing one or two small light yellow globules. In various species of ascidia Rouget found scarlet-red, orange-red, blue, yellow, and violet coloured globules of different shades. In the plasma of very inferior animals, as several species of radiata, the author observed coloured globules. In the sipunculus nudus, sipunculus communis, and sipunculus oxyurus, these coloured globules are very distinct. After giving a detailed description of the blood-globules, and of some phenomena of circulation in these animals, the author concludes, "1, that the presence of coloured globules in the blood is in no relation with the place which an animal occupies in the one or the other of the two great zoological divisions; 2, that the reason for the existence of coloured or colourless elements in the blood of animals must be looked for in the special conditions of the organization of the one or the other species."

Hammond shows that the human white blood-globule is not completely broken up in the act of blood-drying, as Robin and Flemings have asserted; that, however, under similar circumstances, the red globule will remain much longer intact than the white. Hammond performed his experiments by soaking pieces of cotton cloth in human blood, and examining them after various intervals. After twenty-four hours many white globules were unbroken, after forty eight hours only a few, after seventy-two hours none were left; while even after a fortnight red globules were easily discovered. Human blood, however, dried in a thin film, and immediately covered with thin glass, air and moisture being excluded by cement, allows the discovery of white globules after a fortnight, and probably longer.

In a former number* we communicated Bernard's important discovery, that the venous blood issuing from glands while in the act of secretion, is of bright red colour: and that, besides, the rapidity with which blood passes from the vessels is much greater than it is during the time when the gland does not secrete. In the present memoir Bernard informs us that this bright red colour of the venous blood is due to a large proportion of oxygen. For the accurate description of the experiments we refer to the original, mentioning here only that the author employed for the quantitative determination of oxygen carbonic oxide, which possesses a greater affinity to the blood globules than oxygen, and has therefore the property of expelling this gas. By this method Bernard found for 15 cubic centimetres of the various kinds of blood obtained from the arteries and veins of the kidneys of a healthy dog the following figures:

1. For the arterial blood . . .	19·46	Volumes of oxygen.
2. For the bright red venous blood .	17·26	
3. For the dark venous blood . .	6·40	

It appears therefore that glands while in action withdraw less oxygen from the blood passing through them than glands in a state of rest. The author draws attention to the difference which exists in this respect between the animal properties of glands and those of muscles, the latter abstracting while in action more oxygen from the blood than during the state of rest. At the same time Bernard is inclined to think that our manner of using the terms "rest" and "activity," with regard to the physiology of glands, is not quite correct; that what we call "state of rest" is in reality a "state of chemical activity;" what we call "state of activity" is a "state of mechanical activity." We must, however, observe that Bernard has not mentioned the quantity of blood passing through the gland while in the act of secreting, within a certain space of time, compared to the quantity passing through during the period of "rest." If, for instance, in the former case the quantity were much larger than in the latter, the figures above given would not allow the same interpretation as if the quantities in both cases were equal.

Mitchell draws from his experiments made with the hearts of frogs, snapping-turtles, and sturgeons, the following inferences:—1. That the hearts of the frog and turtle beat much less rapidly in vacuo, and sometimes cease to act until the air is re-admitted; that the vacuum most probably retards the heart's action by the mechanical effects it induces, as well as by depriving it of oxygen, since the beat in vacuo is long and laboured, and the accelerating influence of the re-admitted atmosphere is almost instantaneous. 2. That mere isolation from the air, as by placing the heart in oil, does not alter the rate of the heart's

* Conf. this Journal, No. xliii. p. 226. 1858.

movements for some time, but lessens their ultimate duration. 3. That water at aërial temperature stimulates the heart, and very soon causes it to cease to pulsate; that water at higher temperatures, as 100° Fah. to 113° Fah., produces much more rapidly the same result. 4. That glycerine at aërial temperatures affects the heart but little, except as shortening the time during which it continues to pulsate; that glycerine at 32° Fah. depresses the heart's action, lessening the number of pulses per minute at least one-half, and soon checking its movements altogether. 5. That olive oil at 32° Fah. affected the heart very little at first, as to the number of beats per minute, but soon rendered them feeble, and finally stopped them, though at the close of a longer interval than was required by glycerine at the same temperature. 6. That when the heart has ceased to respond to one stimulus, however violent, it will usually remain sensitive to others apparently far less powerful.

Busch confirms the view of Meckel, that substances contained in blood may be carried backwards from the right ventricle into the inferior vena cava and the hepatic veins. When he injected a mixture of finely-powdered coal and water into the jugularis externa of animals, death followed rapidly, probably by obturation of the capillaries of the lungs. The examination of the body exhibited, in all cases except one, particles of coal in the inferior vena cava and also in the hepatic veins, twice in the diaphragmatic veins, once as far down as the renal veins, and once in the coronary vein of the heart.

W. Müller's experiments, performed in Ludwig's Physiological Institution, on living animals, corroborate the results obtained by L. Meyer,* in Bunsen's Chemical Laboratory. The author's careful researches lead to the inference, that the absorption of oxygen by the process of respiration is due to the chemical affinity of the oxygen to constituents of the blood, and is regulated by the composition of the latter; while the emission and immission of carbonic acid is due to simple absorption.

Some experiments instituted to determine the proportion of oxygen in the air necessary for the maintenance of life, lead to the conclusion that reduction of the per-centage of oxygen to two-thirds of its normal amount can be borne without perceptible disturbance, but that reduction to less than one-third of the normal amount endangers life. If the air contains only 3 per cent. death by suffocation is quickly produced.

Fernet endeavours to answer the question whether the absorption and emission of gases during respiration is the result of pressure alone, or due to chemical influence. He examined therefore the quantity of gases absorbed by solutions of the different constituents of the blood in varying degrees of concentration and under varying pressure. Thus he found that the quantities of oxygen, nitrogen, and carbonic acid absorbed by solutions of chloride of sodium, are simply proportional to the amount of pressure under which they are placed. As we cannot enter into the details of these researches, we restrict ourselves to mentioning that the author corroborates the generally-adopted view, that the blood globules are the most important constituents for absorbing and fixing oxygen in a proportion much larger than would be absorbed by mere pressure. Fernet finds the quantity condensed by the blood during respiration twenty-five times larger than that corresponding to pressure. He further states, that under the same pressure the blood absorbs five times as much oxygen as serum alone does. With regard to the question whether the oxygen thus condensed in the blood enters at once into chemical combinations with the constituents of the blood, the author expresses the opposite view, as almost the whole amount of the oxygen absorbed can be separated again by means of the air-pump, as also by forcing through the blood a stream of another gas (Marchand), which would not be the case if the oxygen were chemically bound to the globules.

* Conf. this Journal, No. xli. p. 234. 1858.

Brown-Séquard availed himself of a journey from Nantes, in France, to the Mauritius, to ascertain the changes of temperature occurring in himself, and seven other healthy passengers. The temperature was always taken under the tongue, and in the middle of the day. The following table shows the results of Brown-Séquard's observations :—

	Temperature of the air.	Average temperature of eight men.
1. At Nantes (between 46° and 47° N.L.) on February 10th, 1854	64·4° Fahr. ...	97·925° Fahr.
2. Six days later, on February 16th (under 33° N.L.)	55·4 „ ...	99·05 „
3. On the 21st of February	77·0 „ ...	99·37 „
4. On March 5th (at the Equator) ...	85·1 „ ...	100·22 „
5. On April 19th (under 37°4′ S.L.) ...	61·8 „ ...	99·99 „

It appears, therefore, that the temperature of man changes more under climatic influences than is generally supposed. Brown-Séquard promises to discuss in a future communication the question whether this change in the animal temperature is due merely to a change in that of the surrounding atmosphere, or to the co-operation of various circumstances. The author alludes to the researches of John Davy, and Eydoux, and Soubyet on a similar subject.

III. Lymphatic System.

1. Heidenhain: *On the Anatomy of Peyer's Glands.* (Breslau, 1859; and Schmidt's Jahrbücher, vol. ciii., p. 291.)
2. Schwanda: *On the Quantity of Lymph Secreted within certain periods of Time and under varying Circumstances.* (Wiener Med. Wochensch., 1858, Nos. 15 & 16; and Canstatt, l. c., p. 45.)

Heidenhain's researches made on rabbits and dogs corroborate Brücke's view that Peyer's glands are not organs of secretion, as was formerly supposed, but that they are to be considered as lymph or chyle-glands, their microscopic structure being in every respect analogous to these organs.

Schwanda communicates a series of experiments on the quantity of lymph obtained from the cervical lymphatic vessels of dogs under varying circumstances. The animals were, during the experiments, in the state of sopor from injection of tincture of opium into the vena saphena. The quantity of lymph was largest in animals with vigorous muscular system, limited cellulo-fatty development, and lively temperament. Thus, the largest amount—viz., 7,839 grm. per hour, was yielded by a dog of 7740 grm. weight of body; while another dog weighing 13,303 grm. yielded only 0·314 grm., the mean of 13 cases being 3·965 grm. per hour. In animals of the same constitution the amount was in proportion to the weight of body. The celerity of the progress of lymph is increased by movements of deglutition, contraction of the muscles of the neck, and also by every inspiration; while during the act of expiration the flow of lymph is retarded.

IV. Absorption, Secretion, and Excretion; Metamorphosis of Matter.

1. Lambl: *On the Epithelial Cells of the Mucous Membrane of the Digestive Tube as Protective Organs, and on the Mechanism of Absorption.* (Wiener Med. Wochenschrift, Nos. 24 and 25, 1859; and Schmidt's Jahrbücher, vol. ciii., p. 293.)

2. MARTIN-MAGRON, and BUISSON: *On the Action of the Extract of Nux Vomica and of Curare on the Animal Organism.* (Brown-Séquard's Journ. d. Phys., vol. ii., p. 584, 1859.)
3. SCHLOSSBERGER: *Human Milk, with excessive proportion of Fat.* (Annal. d. Chem. und Pharm., vol. cviii., p. 64; and Canstatt, l. c., p. 157.)
4. LUYS: *The Corpuscula Amylacea a normal production on the surface of the Skin.* (Gaz. Méd. de Paris, 1859, p. 1; Schmidt's Jahrbücher, vol. ciii., p. 154, 1859.)
5. SCHMIDT, C.: *On the Constitution of the Animal Amyloid.* (Annal. d. Chem. und Pharm., vol. cx., p. 250, 1859; and Schmidt's Jahrbücher, vol. ciii. p. 157.)
6. ROUGET: *On the Occurrence of the Glycogenous Substance in the Embryonic Tissues.* (Gaz. des Hôp., 66, 1859; and Schmidt's Jahrbücher, vol. ciii., p. 154.)
7. VON BABO and MEISSNER: *On the Action of Uric Acid on Fehling's Solution of Oxide of Copper.* (Zeitsch. f. rat. Med.; Third Series; vol. ii., p. 321; and Meissner, l. c., p. 348.)
8. NEUSCHLER: *On the Determination of Diabetic Sugar by optic means.* (Archiv f. Physiol. Heilkunde, vol. ii., p. 401; and Meissner, l. c., p. 352.)
9. FEHLING: *The Quantitative Determination of Sugar.* (Annal. d. Chem. und Phar., vol. cvi., p. 75; and Meissner, l. c., p. 352.)
10. MULDER: *Indigo as a Test for Grape and Fruit Sugar.* (Archiv f. Holl. Beitrage, vol. ii. p. 44; and Meissner, l. c., p. 261.)
11. LOWENTHAL: *A delicate Test for Grape Sugar.* (Journ. d. Pract. Chemie, vol. lxxiii. p. 71; and Meissner, l. c., p. 261.)
12. BRÜCKE: *On the Existence of Sugar in the Urine of Healthy Men.* (Sitzungsberalt d. k. k. Akad. z. Wien., vol. xxxviii., p. 568; and *On the Glycosuria of Lying-in Women.* (Wiener Wochenschrift, Nos. 19 and 20, 1858; and Meissner, l. c., p. 351.)
13. LÉCONTE: *On the Examination of Sugar in the Urine.* (Brown-Séquard's Journal de Phys., vol. ii., p. 593, 1859.)
14. PASY: *On the Lesions of the Nervous System producing Diabetes.* (Proceedings of the Royal Society, vol. x., No. 35, p. 27, 1859.)
15. ISAACS, C. E.: *Researches into the Structure and Physiology of the Kidney; and on the Functions of the Malpighian Bodies of the Kidney.* (Transactions of the New York Academy of Medicine, vol. i., part ix., 1857.)
16. UHLE: *Contribution to the Knowledge of the Urine of Children.* (Wien. Med. Wochensch., Nos. 7 and 9, 1859; and Schmidt's Jahrb., vol. civ., p. 10, 1859.
17. ROBERTS, WM.: *A Contribution to Urology, embracing Observations on the Diurnal Variations in the Acidity of the Urine, chiefly in relation to Food.* (Memoirs of the Manchester Philosoph. Society, 1859.)
18. BERNARD: *Lectures on the Physiological Properties and the Pathological Alterations of the Fluids of the Organism.* (Paris, 1859; and Schmidt's Jahrb., vol. civ., p. 9, 1859.)
19. RANKE, H.: *Observations and Experiments on the Excretion of Uric Acid.* (München, 1858.)
20. SCHUNK: *On the Occurrence of Indigo-blue in Urine.* (Memoirs of the Manchester Philos. Society, vol. xiv., p. 293; and Schmidt's Jahrb., vol. civ., p. 32.)
21. CARTER: *Notes on Two Cases in which the Urine yielded Indigo.* (Edinb. Med. Journal, July, 1858.)
22. KLETZINSKY: *On the Chemical Constitution and the Semiotic Indication of the Indigo in the Urine.* (Wiener Med. Wochensch., 1859, 27; and Schmidt's Jahrb., vol. civ., p. 36.)
23. BÖDEKER: *Communication from the Chemical Laboratory of the Physiolo-*

gical Institution at Göttingen. (Zeitsch. f. rat. Med.; Third Series; vol. vii., p. 127, 1859.)

24. ZENKER: *Relation between Hematin and the Pigment of the Bile.* (Virchow's Archiv., vol. xvi., p. 562, 1859.)

Lambl's researches regarding the epithelial cells of the intestinal mucous membrane are, on the whole, confirmatory of Brücke's* view; he contends that these cells do not present a closed wall towards the intestinal cavity, but terminate in a funnel-shaped opening. For the hypothesis of absorption of chyle, based on this structure, we refer to the essay. Lambl could not discover the porous channels ("Torenkanälchen") described in the epithelial cells of the intestines by Kölliker and Funke.

Schlossberger found the milk from the hypertrophied breast of a vigorous woman aged twenty-six years, to possess the following most remarkable constitution. The specific gravidity at 15° C. (59° F.) was 0,98–0,99, while that of normal milk is, according to Vernois and Becquerel, 1,02 to 1,046.

	Chemical composition of the milk examined by Schlossberger.	Of normal human milk, according to Vernois and Becquerel.
Water	67·52	87 — 89
Solids	32·48	13 — 11
	100·00	100·0 100·0
Water	67·52	— —
Fat	28·54	0·6 — 5·66
Sugar and extractive substances	0·75	2·5 — 5·9
Casein	4·74	3·9 —
Salts	0·41	0·05 — 0·33

The author does not express a view with regard to the circumstances connected with the secretion of this abnormally fatty milk.

Martin-Magron and Buisson prove by experiments on guinea-pigs, that the supposition entertained by La Condamine, Humboldt, Pelouze and Bernard, as also by Fontana and others, that curare does not kill, when introduced into the intestinal tube, is incorrect. The view therefore, that curare is not absorbed by the intestinal mucous membrane is likewise incorrect. The author's experiments demonstrate also, in opposition to those of Munter and Virchow, that the cutaneous surface of frogs absorbs curare as well as strychnia, leading to the characteristic symptoms of poisoning by the respective substances. In these two respects there is, therefore, no essential difference between the two poisons, strychnia and curare; the authors endeavour to prove elsewhere that such an essential difference, or even antagonism, as has been asserted by some experimenters, does in reality not exist between these two poisons.

Luys found the corpora amylacea as well on the surface as within the tissue of healthy skin. The author is inclined to bring these bodies in connexion with Bernard's glycogenous substance; he has, however, not proved that they can be transformed into sugar. Schmidt examined, chemically, portions of choroid plexus and spleen containing a large quantity of corpora amylacea, with the view to ascertain whether any sugar could be obtained from these bodies by the usual means for transformation of starch and cellulose into sugar; he thus, however, never was able to detect any sugar, and, therefore, maintains that the so-called animal amyloïd is not a non-azotized substance, analogous to cellulose, but an azotized albuminous body.

Bernard's† discovery of glycogenous substance in the placenta, led to the discovery of the same substance in many other organs. Thus Rouget found it

* Conf. this Journal, No. xxv. p. 263. 1854.

† Conf. this Journal, No. xlvii. p. 240. 1859.

in the ossifying cartilages of the fœtus of sheep, in the cells of the mucous membrane of the digestive canal, of the respiratory organs, and of the urogenital apparatus, in the skin, in muscles, in the amnion and other fœtal tissues. Rouget thinks, therefore, that there is no reason to consider the presence of this substance in the placenta as the sign of a new function of this organ as a temporary liver.

The tests for sugar in the urine have occupied the researches of various observers. Von Babo and Meissner proved that in normal urine lithic acid is the principal substance which reduces the oxides of copper in an alkaline solution. If a solution contains one per cent. of lithate of potash, or a larger amount of lithic acid, then the addition of Fehling's solution of sulphate of copper causes sometimes already in the cold, at all events when heated, a discoloration and white precipitate (lithate of copper). There are some substances which have the properties of preventing the formation and precipitation of the suboxide of copper, and prevent through this more or less the appearance of the proper reaction. Such substances are ammonia, kreatin, kreatinin, a product of decomposition of lithic acid in alkaline solutions, and, in a slight degree, also urea.

Neuschler finds the quantitative determination of sugar by Soleil's "saccharimeter," or Robignet's "diabetometer," more accurate than the methods of reduction of oxide of copper.

Fehling, on the other side, gives the preference to the test by reduction, if the quantity of sugar is to be determined in alcoholic solutions, containing between fifteen and twenty per cent. Rarely, however, such large quantities of sugar will be met with in the examination of animal fluids.

Mulder recommends *indigo* as test for grape-sugar, as it reduces, in the presence of alkalies, indigo-blue to indigo-white.

Löwenthal avails himself of the reduction of oxide of iron as a test for sugar. Sixty grm. of tartaric acid and 120 grm. of soda are dissolved in 250 grm. of water, and, again, 120 grm. of soda in 250 grm. of water, both solutions, when cold, are mixed, and five-sixths of a grm. of crystalline chloride of iron is added; after a few minutes of boiling, the mixture is filtered. The solution thus obtained remains clear yellow when boiled. The admixture of sugar renders it darker, and if the quantity of sugar is not very small, a bulky deposit, containing suboxide of iron, is formed.

Brücke employed in his researches partly the liquor potassæ-test; partly Trommer's test; partly the less generally known fact of the reduction of the basic nitrate of bismuth by means of sugar after the addition of potash. The author controlled, however, his observations by the formation and isolation of the sugar as sugar-potash (zucker-kali). In this manner Brücke convinced himself of the presence of small quantities of sugar, as well in normal urine as in that of lying-in women. He considers, however, his experience as not sufficient to entitle him to say that the amount of sugar in the urine of lying-in women is larger than that in the urine of other persons.

Léconte divides the tests for sugar into *general* and *special* tests. Under the former designation he enumerates Trommer's, Barreswil's, and Fehling's tests—the liquor potassæ, chromic acid and bismuth tests; all these tests he considers as useful, but none as sufficiently accurate to allow the inference of the presence of small quantities of sugar. As *special* and only trustworthy tests, Léconte describes: 1. A prompt fermentation in the presence of yeast when used with proper precautions; and, 2: The extraction of the sugar itself, by means of Lehmann's process. For the description of these tests, we refer to the essay, which the author concludes by repeating that he cannot admit the presence of notable quantities of sugar in the urine of healthy persons or of women while nursing.

Pavy commences his communication to the Royal Society by the information that his further researches on the sugar-forming substance of the liver had confirmed the inferences arrived at in his essay on "the alleged sugar-forming function of the liver."* The author's views appear to be "that in the healthy liver during life there is a substance (hepatine) which possesses the chemical property of being most rapidly transformed into sugar when in contact with nitrogenized animal materials. In the liver, after death, this transformation takes place; but in the liver, during life, there seems a force or a condition capable of overcoming the chemical tendency to a saccharine metamorphosis." The same author's inferences with regard to the influence of lesions of the nervous system producing diabetes, will be found Sub. VI. of this report.

The New York Academy of Medicine has published two communications by Isaacs relating to the structure and physiology of the kidneys. With regard to the structure, the author says, that "contrary to the opinions of some of the highest authorities," he has shown the epithelial lining of the tubes of the kidneys to be of the pavement or tesselated variety; he maintains the existence of ciliary motion, although weak and imperfect, in the kidneys of higher animals; he further asserts, that the Malpighian tuft of the higher animals has been clearly demonstrated by him to be covered with oval, nucleated cells. Concerning the physiology of the kidney, it is especially the function of Malpighian bodies with which Isaacs appears occupied in the present researches. He is opposed to the view that the cells lining the tubuli uriniferi are the instruments by which the solids are principally secreted, while the prominent function of the Malpighian bodies is, according to this view, the excretion of water. Previously to enunciating his own view, Isaacs reasons in the following manner:—1. Urea, lithic acid and salts are preformed in the blood, and are merely separated from it by the kidney. 2. These substances must necessarily enter the Malpighian tufts. 3. The same is the case with diuretics. 4. The Malpighian tufts are not "a bare or naked system of capillaries," but covered with oval nucleated cells, possess therefore "a glandular structure," and are "every way adapted for the separation of the proximate elements of the urine." 5. In serpents, alligators, turtles and other animals the Malpighian bodies abound throughout the cortical substance, and in some of these animals, throughout the whole structure of the kidney, yet their urine is secreted in the semi-solid state. 6. The presence of the colouring matter of the bile was ascertained in the capsule of the Malpighian tufts and in the commencement of the uriniferous tube of a patient who had been jaundiced for several years. 7. Experiments made by introducing various colouring matters into the stomach and intestines of animals, showed the colouring matters as well in the Malpighian bodies as in the tubes. 8. In two cases the author made the existence of lithic acid in the Malpighian bodies very probable to himself. 9. The Malpighian bodies appear intensely red and congested, sometimes even ruptured after the administration of strong diuretics. "From all these considerations," Isaacs says, "it seems that we are justified in concluding that the Malpighian tuft separates from the blood most of the proximate elements of the urine. Any element of the urine which is not secreted by the Malpighian tuft is then probably separated by the epithelial lining of the tubes, as is generally believed."

Uhle's researches on the secretion of urine in children corroborate the inferences drawn by Scherer, Rummel and others, that the metamorphosis of matter is more rapid in children than in adults. The tables furnished by Uhle show, that for the same weight of body with the increase of age, the amount of urine, urea and chloride of sodium becomes constantly less; the diminution in the excretion of these substances being so great, that a person between sixteen and eighteen years of age excretes for the same weight of body only about the third part of the amount excreted by a child between three and five years of

* Conf. this Journal, No. xlv. p. 171. 1859.

age. The author has extended his researches also to children under pathological conditions, but we must refrain from entering into these at present.

Roberts' researches on the acidity of the urine were made on a single individual, "a healthy man, twenty-eight years of age, taking moderate exercise, living in most favourable hygienic conditions, and weighing 144 pounds." The mode of life of the subject of experiment was as nearly as possible uniform during the time of observation. He rose at 7 a.m., breakfasted at 8 a.m., dined at 2 p.m., sometimes at 4 p.m., and took no further solid food until breakfast next morning. The experiments are divided into several series, in order to examine the influence of mixed food, of purely animal and of purely vegetable food. The degree of acidity or alkalinity was ascertained by a test solution after the volumetric method. For the determination of the acidity in the urine caustic soda was employed, for that of the alkalinity dilute sulphuric acid. The results arrived at by the author are: 1. The immediate and primary effect of a meal, whether of purely vegetable, purely animal, or mixed food, was in from one to three hours to diminish the acidity of the urine; and very frequently to render it alkaline. 2. The remote or secondary effect of a meal was to uphold and increase the acidity of the urine. 3. The remote effect of animal diet appeared considerably greater than that of vegetable food. So that a highly animalized diet tends in the long run to heighten the acidity of the urine. 4. After breakfast, the greatest depression occurred at the second hour; and the period of depression continued from two to four hours. 5. After dinner, the greatest depression occurred at the third, fourth and fifth hours, and lasted from four to six hours. The effect of dinner was greater and more prolonged than that of breakfast. 6. The effect of mixed and purely animal diet seemed almost identical. Vegetable diet, when used on alternate days with mixed or animal food, had a decidedly feebler effect; but when used for several days successively, its effect was equally powerful. 7. Alkaline urine after a meal owed its reaction to a fixed alkali. It was generally, but not always, turbid, when passed, from precipitated phosphates. Its odour resembled that of the fresh urine of the horse. It was richer in uric acid and in earthy and alkaline phosphates than the urine of fasting. 8. The depression of the acidity after a meal coincided in point of time with absorption rather than with digestion. The solids of the urine began to increase simultaneously with the declension of the acidity. So that the passage of food into the blood and the diminished acidity of the urine seemed to be connected together as cause and effect.—Roberts further considers the following deductions as warranted: "1. That the power of a meal to depress the acidity of the urine depends on its mineral constituents. These contain phosphoric acid and the alkalies in such proportion, that, if we suppose them dissolved, the alkalies invariably preponderate. Hence arises the alkalinity of the blood. If this be so, every meal that is dissolved and absorbed into the blood must for the time raise the alkalescence of that fluid above the natural level." 2. "The kidneys have the special function to regulate the degree of alkalescence of the blood. When it is too high they separate alkali, and the urine becomes alkaline; when it tends to become too low, on the other hand, they separate acid, and this gives to the urine its common acid reaction." 3. "A meal then, in so far as its mineral ingredients are concerned, acts like any other dose of alkali." 4. "The emission of urine turbid with phosphates is, within certain limits, a natural phenomenon; and earthy phosphates constitute the only urinary deposit which can appear in the healthy urine *on passing*." 5. "Urines may be divided into two chief classes: first, *urines of fasting (urinæ sanguinis)*; secondly, *urines of food (urinæ cibi)*. Fasting urines are scanty and of high acidity, they present only one variety, namely, that of sleep, which differs from other fasting urines in possessing more colouring matter. The urines of food fall naturally into two divisions—

those with a diminished and those with a restored acidity; they are abundant in quantity." The author tried also on two days the effects of cane-sugar and honey. Neither seemed to produce any depression of the hourly quantity of urinary acid discharged.

Bernard ascribes the difference in the reaction of the urine of carnivores and herbivores to the difference in the food. The urine of the former is generally considered as acid, that of the latter as alkaline. The urine, however, as well of horses as also of rabbits becomes acid, after they have been fed with oats; the urine of herbivores is further found acid, when the animals are subjected to starvation. The author corroborates on carnivorous animals the observation made by others on man, that the urine of these animals, too, although fed, as usual, on animal food, becomes alkaline four to five hours after a meal. But, while Roberts (see the preceding extract) and others, too, found the alkaline reaction remaining even after boiling, Bernard found the reaction to become acid after boiling, and he infers from this, that the alkalinity of the urine of carnivorous animals after meals is caused by a volatile alkali, the carbonate of ammonia; the alkalinity of the urine of herbivorous animals on the contrary being due to fixed alkalies.

Ranke communicates interesting researches on the excretion of uric acid in man, especially during health, but also in various morbid conditions, and further under the influence of large doses of quinine. For the influence of quinine we refer to a report in No. xxxix. of this Journal. We restrict ourselves in the present report to the excretion of uric acid under physiological conditions. 1. The quantity of uric acid excreted in the urine of healthy persons appears to be subject to but slight variations in the same individual, as long as the manner of living remains unchanged. 2. A certain relation between age and sex and between the excretion of uric acid does not appear to exist. 3. The author could also not find any relation between the amount of uric acid excreted and the height and weight of body. 4. The temperature, too, is without influence on the excretion of uric acid, an observation which is in accordance with Lehmann's experience. 5. With the ingestion of food, on the contrary, the quantity of uric acid excreted is intimately connected. Abstinence causes some diminution of its amount, which again rapidly increases when food is taken. The quality of food appears to be of less importance. The variations in the excretion during the various periods of the day appear to depend on the time when the principal meal is taken. 6. Moderate exercise appeared to cause a slight decrease, fatiguing exercise an increase in the excretion of the uric acid. The latter part of this proposition is in accordance with the observations of Genth* and Keller,† in opposition to those of Hammond.‡ 7. The average proportion between uric acid and urea appears to vary between 1 : 70 and 1 : 80. The quality of diet exercises, however, a considerable influence. The author observed on himself, when he lived on mixed food, the proportion of 1 : 61; when on pure animal diet, 1 : 49; when on pure vegetable diet, 1 : 41. Soon after meals the proportion is greatest (1 : 38—1 : 30); while fasting it is smallest (1 : 69—1 : 61). 8. Decrease in the amount of uric acid does not coincide with increase in that of urea. In general both substances are found simultaneously either in increased or in diminished quantity. 9. The proportion of uric acid to the solids may, during health, vary between 1 : 60 and 1 : 138. We will not repeat here the results of Ranke's experiments on the influence of quinine on the excretion of uric acid, but we cannot refrain from quoting the inference to which these results and other facts just mentioned lead the author. "The connexion," he says, "between the excretion of uric acid and digestion, and between digestion and swelling of the spleen;

* On the Influence of the Ingestion of Water on the Metamorphosis of Matter. Wiesbaden. 1856.

† Keller's Archiv, N. F. i. ‡ American Journal, Jan. 1855.

the increase of uric acid in splenic leukæmia" (i.e., the splenic variety of leucocythemia of English authors), "and in ague" (according to the observations of others) "the prevalent existence of the uric acid in the splenic juice, and finally the property of quinine to reduce splenic tumours and to diminish the amount of uric acid, render it highly probable that the spleen is a principal source of the uric acid."

The important researches of Schunck and Carter, for the details of which we must refer to the respective essays, prove that indigo is present in normal urine. The cases described by Heller, Hassal, and others, in which cases the amount of indigo in the urine was so great, that it, when acted upon by nitric acid, assumed a beautiful blue colour, which passed to a yellowish red tint when the mixture was heated or an additional quantity of acid added, are through this discovery rendered less obscure.

Kletzinsky endeavours to secure for Heller the priority of the normal occurrence of indigo in the urine, and it is true that Heller's former researches on the colouring matter of the urine led to results in some way similar to those of Schunck, although, and this is important, Heller did not apply to the substances found by him the terms belonging to the products obtained from indican. Heller's uro-glaucin is blue indigo or indigo-blue; his urrhodin is indigo-red, his uroxanthin is indican. According to Kletzinsky the increased appearance of indigo in the urine is principally caused by conditions of irritation of the spinal marrow and its nerves, by certain renal affections and exudative processes of the serous membranes.

Bödeker, assisted by G. Fischer, has succeeded in forming a syruplike substance from proteinaceous bodies. This syruplike substance is free from nitrogen, "reduces not only the oxides of copper, bismuth, and silver when boiled with caustic soda, but there is also real fermentation produced in it by the addition of yeast." The author promises further and detailed communication concerning this important fact of the possibility of the transformation of nitrogenous substances into sugar.

Bödeker further communicates that he found cholestearin in the watery extract of the spleen. He is of opinion that the cholestearin passes into the watery solution in the state of emulsive suspension.

Zenker found in various instances blood-crystals (hæmatoidin) in localities where they could not have been formed by stagnating blood, but only by stagnating bile, as in dilated gall-ducts, and in the bile of the gall-bladder. The author further communicates the fact that Virchow's bilifulvin can be artificially transformed into hæmatoidin crystals. On the other side he found in a cicatrix of the spleen a yellow pigment, presenting all the phenomena of biliary pigment. Zenker considers, therefore, hæmatin, biliary pigment, and hæmatoidin as various stages of metamorphosis of the same substance, and is inclined to think that, normally, hæmatin is transformed into biliary pigment, and as such excreted; that, however, under pathological conditions, hæmatin may pass through the stage of biliary pigment into hæmatoidin. With regard to physiology, we remark, that the author is inclined to see in the facts just mentioned, a proof that the biliary pigment is formed by the hæmatin derived from the perishing blood-globules.

[Want of space compels us to postpone a portion of the Physiological Report.—ED.]

HALF-YEARLY REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.
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I. *On the Employment of Tonics in the Treatment of Typhoid Fever.* (Bulletin Général de Thérapeutique, June 30th, 1859.)

Very few physicians in the present day treat typhoid fever with tonics from its commencement to its termination. But it is undoubted that tonics are useful at certain periods, and that their use is often observed, from the very time of administration, to be followed by a diminution of very severe nervous symptoms. M. Monneret adopts a mixed treatment in typhoid fever, the following being a sketch of the plan he pursues. In the first place, an emetic is administered, and as soon as it has operated, a purgative of seidlitz water is given: then the patient is allowed to rest for two or three days if the bowels are opened, but if, on the contrary, the constipation returns, and if there is distension of the abdomen, another purgative is ordered. If the distension increases and is accompanied with great want of power, M. Monneret employs ice internally and externally, with negus for drink. The poultices are composed of linseed-meal and fragments of ice: they are placed upon the abdomen and the ice slowly melts. In the second week, when signs of prostration appear, the tonic treatment is commenced, consisting in the employment of sulphate of quinine in the dose of fifty to sixty centigrammes in a draught, and wine is also administered. Besides these measures, broth is given two or three times a day, and this treatment is continued during the second and third week. Two cases are given in illustration of the successful results of this treatment.

II. *On the Use of Sulphate of Copper and Opium in the Treatment of Diarrhœa occasioned by Dentition.* By Dr. Eisenmann, of Würzburg. (Bulletin Général de Thérapeutique, June 30th, 1859.)

At the period of their first dentition, children are often attacked with diarrhœa, not generally of a severe character, but sometimes, and especially when the dentition coincides with weaning, the evacuations are copious, and the diarrhœa passes into the chronic form. An extraordinary emaciation then ensues, and sometimes nervous symptoms of various kinds supervene, especially the *hydrocephaloid* disease, which has been mistaken for acute hydrocephalus. When it has passed into the chronic stage, this diarrhœa is very frequently fatal, the patients dying of marasmus. For this affection, Dr. Eisenmann has successfully employed a combination of sulphate of copper with opium. He prescribed it first in the case of a child whom he attended in the year 1838. The diarrhœa had not at first excited much attention, but the evacuations soon became serous, lasted three months, and reduced the child to a mere skeleton. She was in a continual state of *coma vigil*, the pulse was very frequent and scarcely to be felt, the appetite was lost, and the thirst was inextinguishable. The sulphate of copper and opium, both in very small doses, were prescribed in powders to be taken three times a day. In the first four days of the treatment there was already a perceptible amendment, the evacuations became less frequent and less serous, and three days afterwards the patient began to be convalescent. The diarrhœa ceased, the appetite returned, and the digestion was re-established, the strength and the normal size of the body re-appeared, and four weeks afterwards the cure was complete. Dr. Eisenmann was afterwards called upon to treat other cases of the same kind, and by employing the same remedies, similar favourable results were obtained. In the year 1840, he had occasion to observe a fourth case of this kind of diarrhœa. The patient presented the same symptoms as those above described, with this difference, that there was vomiting in addition, and that the disease had lasted only a fortnight. The emaciation was, however, already very considerable: the abdomen was somewhat tumefied and painful on pressure. The appetite was bad and the thirst urgent; the respiration was accelerated, and there were mucous rhonchi, without any symptom of pulmonary lesion, the face was pale and a little puffy, and the looks expressed apathy. Sulphate of copper and opium, in very small doses, were prescribed, and twelve doses brought about convalescence in four days.

III. *On the Employment of Iodide of Potassium in the Treatment of Aneurisms.* (L'Union Médicale, March 8th, 1859.)

The 'Gazette des Hôpitaux' has lately reported a clinical lecture of Professor Bouillaud upon aneurisms, and upon the results of the treatment of aneurismal tumours by the iodide of potassium. One of the patients was a man suffering under an aneurism of the brachio-cephalic trunk and aorta, and the other was a woman with an aneurism of the carotid artery. In the latter case, the iodide of potassium was administered for some days in the dose of a gramme, and afterwards in doses of two grammes for two months. At the end of this period, the tumour, which was at first as large as a pigeon's egg, had diminished so much that it might be considered to have disappeared completely. In the case of the man, the tumour, which was of considerable size, underwent displacement contemporaneously with a very well-marked diminution in volume, under the same treatment as that adopted for the woman. At the time of the report, however, the man was still under the treatment by iodide of potassium, and therefore no positive conclusions could be drawn. M. Bouillaud has treated other cases with iodide of potassium. In the case of a man with a large aneurismal tumour at the point of origin of the carotid and subclavian arteries, he found that the swelling was considerably diminished in a few weeks by the use of the iodide. In another patient, treated in the same manner for an aneurism of the carotid artery, he observed at the end of a few weeks that the tumour had almost entirely disappeared. These cases are considered to be sufficiently satisfactory to encourage practitioners in making further trials of the iodide of potassium in aneurismal tumours.

IV. *On the Hypnotic Properties of Chloroform.* (Bulletin Général de Thérapeutique, May 15th, 1859.

Dr. Uytterhoeven, of Belgium, having recommended chloroform in small doses as an excellent hypnotic, Dr. Fonssagrives has also called the attention of practitioners to the employment of this agent, which for several years has never completely failed in his hands. Want of sleep, according to M. Fonssagrives, is attributable to two very different causes. Sometimes it is the result of the permanence of some painful symptom which prevents rest, and at other times it constitutes a merely nervous symptom, originating in an acute moral affection, or a too active or too protracted intellectual effort: sometimes the cause is a morbid habit of the cerebral centre, and at other times insomnia results from the abuse of narcotic medicines, or it marks the course or the decline of certain acute diseases. The latter cases are those which are the best suited to the employment of chloroform. The formula of M. Uytterhoeven consists in administering a varying dose of five to ten drops in some mucilaginous fluid, and M. Fonssagrives has given it in this manner with the greatest success.

V. *On the Treatment of Phthisis by Mineral Waters.* By Dr. DURAND-FARDEL. (Bulletin Général de Thérapeutique, June 30th, 1859.)

Although at first sight it does not seem very evident how mineral waters can be made applicable to the treatment of phthisis, Dr. Durand-Fardel still thinks that this disease may be relieved by such therapeutical agents. The waters which he thinks best adapted to the treatment of phthisis are the *sulphuretted* waters and those containing *chloride of sodium*. The latter are not specially applicable to the treatment of phthisis, although they are useful remedies in some scrofulous cases. Sea-air affords the most efficacious remedy in those phthisical cases which are likely to be benefited by chloride of sodium. M. Durand-Fardel knows only one place where the treatment of phthisis by chloride of sodium is pursued, namely, at Soden, in Nassau; but even there, the results are not considered to be always satisfactory, and the beneficial effects appear to be due to exceptional climatic conditions, and not to the medication itself.

The greater part of the sulphurous waters, however, appear to be useful in the treatment of pulmonary consumption. Whether their basis is lime or soda, and whether they are warm or cold, their appreciable physiological characters are everywhere pretty much the same. All of them being feebly mineralized, sulphuretted hydrogen is always disengaged from them in the same form, whatever the base may be. Some localities have obtained a great reputation in the treatment of phthisis, and it is rather difficult in many cases to reconcile the popular belief with strict physiological deductions. Among such spots may be mentioned Eaux-Bonnes, Cauterets, Amélie, Vernet, Allevard, and Enghien. The seclusion, the altitude, and the climatic characters of certain places afford conditions which are very differently appreciated by the patients, according to the nature of their disease. Thus the cold waters of Enghien are less favourable to hæmoptysis than the warm springs of the Pyrenean stations. The waters of La Raillère, at Cauterets, appear to be less exciting than those of Eaux-Bonnes, and those of Amélie still less. Besides, the same methods of treatment are not pursued at these different stations. At Enghien, the baths are taken at the same time with drinking the mineral waters: at Eaux-Bonnes the baths are not used: at La Raillère, *semi-baths* are used, that is to say, baths limited to the inferior half of the body.

It may be asked, at what period of phthisis ought we to have recourse to mineral waters? What is the object of the thermal treatment, and what are its inconveniences and its advantages during the first evolution of tubercles or during their softening, or when their elimination is being effected? What is the signification of hæmoptysis, fever, and active congestions of the lung, as counter-indications? M. Durand-Fardel draws the following conclusions in answer to these questions.

The progress of phthisis is generally not continuous. The disease is sometimes arrested for a longer or shorter period, and during these intervals the development of the tubercles appears to be less active or to become stationary. At such times, the thermal treatment may be adopted, and it will be the more efficacious in proportion as it is combined with the reparative tendency of the organism, a tendency which may always be supposed to exist to a certain extent, and in the absence of which the thermal treatment itself would be fruitless. The mineral waters must never be employed in the active periods of phthisis, but only when the disease appears to be stationary. Mineral waters have been recommended by some, and deprecated by others, either in the first stage of phthisis, or in the stage of softening of the tubercles, or in the last stage of the disease; and both parties may appeal to facts in support of their views. But perhaps they are wrong in regarding exclusively the anatomical stage of the evolution of the tubercles, instead of inquiring whether there were not some conditions common to these different stages, from which they might deduce indications and counter-indications. If it be true that tubercular consumption is susceptible of cure, and if it be true, on the other hand, that the indications for the mineral waters have been clearly explained, then it may be affirmed that these waters may be usefully employed in phthisis, either to retard its progress or to decide its cure. "Undoubtedly," says Dr. Durand-Fardel, in concluding his paper, "in so formidable a disease, the beneficial influence of any kind of treatment must be rather limited. But I am convinced that when applied according to the principles I have laid down, that is to say, at the proper times, the mineral waters may be administered without danger, and with some benefit, in the great majority of phthisical cases."

VI. *On the Use of Injections of Chloride of Zinc in the Treatment of Urethritis.* (L'Union Médicale, May 10th, 1859.)

A new treatment of urethritis, by injections of chloride of zinc, has lately been introduced into practice, and it would be an interesting point to determine the value of this kind of medication. The plan consists in injecting once a day a very weak solution of the chloride in cases of simple and acute urethritis, and a stronger solution in chronic and obstinate cases. Fifty patients were subjected to this treatment in the Val-de-Grâce, under the care of Professor Legouest. Of this number 21 were affected with simple urethritis, 12 with the acute, and 17 with the chronic disease. In the cases of simple urethritis a single injection was thrown in every morning, and kept for three or four minutes in the canal, and in three cases only did the application give rise to some slight pain, which, however, disappeared in a few days, and then allowed the treatment to be recommenced. The acute cases were subjected to the same treatment, namely, to one injection daily. In general, the injection relieved the pain and occasioned only a slight itching; but in one case the injections were suspended, in consequence of the supervention of rather acute pain. The minimum period of treatment was four days, and the maximum forty-one, the medium being 13·5. The chronic cases were all treated with the injection, and no bad symptom supervened. The results drawn from these cases prove that injections of chloride of zinc ($\frac{1}{1000}$ to $\frac{1}{500}$) are not generally painful; that they seldom cause any bad symptoms; that they rapidly diminish the discharge; that in the greater number of acute cases they relieve the inflammation and pain; that they are less successful in simple urethritis (in which they possess no superiority over the ordinary methods of treatment) than in the well-marked acute and the chronic cases; and, lastly, that it is in the latter, especially in the very old chronic cases, that the treatment appears to be truly and remarkably successful.

VII. *On the Medical Knowledge of the Inhabitants of Tahiti.* By Dr. Schwarz. (Zeitschrift der Gesellschaft der Aerzte zu Wien, August 22nd, 1859.)

The Tahitian practitioner of medicine treats only the members of his own family and his friends, and receives no substantial remuneration for his cures; but he is open to the reproach of concealing his knowledge from others. He goes alone to seek for his medicinal plants in the forests and ridges of the hills, and no one must be present at the preparation of the drugs; the patient receives the medicine at the hands of the doctor himself, who gives only one dose. When the treatment is once undertaken, the Tahitian doctor exhibits excessive zeal, and if after a certain time the expected operation does not ensue, he again goes away, prepares new drugs, and makes other combinations, which are very complicated, and he himself gives the fresh medicine to the patient. But strangers also, that is to say, Tahitians from distant districts, may become partakers of the beneficial operation of the treatment; but they must visit the doctor, as he never goes to them. Hence whole families, in which a relative is dangerously ill, leave their huts and their village in order to visit the place of residence of a celebrated doctor, so that the latter often resides in a kind of village hospital, in which huts are brought in and erected one day and broken upon the next.

A short time ago, all medicines in Tahiti which were taken internally were called Cocoa-medicines, an expression which seemed to attribute some peculiar virtues to the cocoa-palm. Such, however, is not the case; but the water of the young nut is the saccharine and sweet-tasting vehicle of the vegetable extracts, and draws forth their medicinal powers; it possesses no more efficacy in itself than any other saccharine solution. Every medicine is carefully pressed through a filter prepared from the fibres of a certain cyperaceous plant before it is administered to the sick person, with mysterious ceremonies and prayers for the blessings of the gods and spirits. Another therapeutical practice is the external application of certain plants and barks, which are bruised, pounded, and boiled in water, and then laid on in a hot state for certain skin diseases

Dr. Schwarz enumerates the principal diseases known among the inhabitants of Tahiti, and also the native remedies which they are in the habit of employing. The number of the latter is rather scanty, and those which are described are not much known in Europe. The remedies are divided into mild purgatives, comprising the Ficus prolixa, Balanophora fungosa, Ophioglossum vulgatum, Adenosma fragrans, Polygonum imberbe, and Convolvulus turpethum; the drastic purgatives are the Daphne fœtida and the Cerbera manghas; the emetics are the Daphne fœtida and Morinda citrifolia. Some plants are considered antiblennorrhagic, as Polygonum imberbe and Geophila reniformis. The emmenagogues are Polygonum imberbe and Adenosma fragrans; the expectorants are Terminalia glabrata, Killingia monocephala, Vandellia crustacea, and Jambosa malaccensis. Some medicines are called vulneraries, as Poa latifolia, Cardamine sarmentosa, Cyperus cinctus, &c.; and there are others which are considered specific in ophthalmia and pains in the bowels.

VIII. *On the Treatment of Blennorrhagia by Vinum Colchici and Tincture of Opium.* By Dr. Eisenmann, of Würzburg. (Bulletin Général de Thérapeutique, May 15th, 1859.)

Dr. Eisenmann states that he once had occasion to prescribe a combination of vinum colchici and tincture of opium for an officer affected with rheumatic conjunctivitis, and a few days afterwards he was informed that the medicine had cured not only the ophthalmia, but also a blennorrhagia, of which no mention had been previously made. He was surprised at this result; but he resolved to profit by it, and to try the same treatment in other cases. He therefore prescribed the medicine for a girl affected with blennorrhagia, and was again surprised that a permanent cure was effected in a few days. Nothing was ordered externally, except frequent applications of tepid water. Subsequently, several cases of blennorrhagia in the male presented themselves, and were treated in the same manner. The dose employed was eighteen to twenty drops, three times a-day, of a mixture consisting of twelve grammes of vinum colchici with two grammes of tincture of opium; milk was ordered as the principal article of food, and absolute rest was enjoined. All the cases of blennorrhagia thus treated were cured without exception in a few days, especially when the treatment could be adopted at the commencement of the affection, and none resisted longer than a week. The observations of Dr. Eisenmann have been confirmed by those of M. Collin, of Dresden, who treated ten cases of blennorrhagia with the greatest success by the mixture of vinum colchici and laudanum. The patients did not recover so rapidly as those treated by Dr. Eisenmann; but the latter physician attributes the difference to the probable inferiority of the drugs employed, and to the fact that the patients did not consult a medical man at a sufficiently early period.

IX. *On the Employment of the Pulp of Raw Meat in the Treatment of Chronic Diarrhœa in Children.* (Bulletin Général de Thérapeutique, May 30th, 1859.)

The plan of treating the diarrhœa of children by raw meat, as proposed by M. Weisse of St. Petersburg, has already been described; but Trousseau has lately introduced it into practice in Paris with great success, notwithstanding the natural opposition to such a system of treatment. Although the substance employed is raw meat, yet its administration must be accompanied by certain precautions, so as to render it palatable and digestible. The meat, in fact, must undergo a peculiar preparation, consisting in the complete separation of its fibres and the removal of all the cellular, fibrous, and tendinous parts which might offer obstacles to its solution in the gastric juice. The lean of beef, mutton, or poultry may be employed; but the first is far preferable. After having cut the meat into very small pieces, it is pounded and reduced to a thick pulp. This pulp is placed upon a sieve with small holes, after being stirred and pressed until the red and fleshy part can pass completely through the holes. Then the red strained matter is collected and mixed with sweatmeats, of which little balls are made for the children to swallow. Thus prepared, the pulp of raw meat has not the taste of raw flesh, which, indeed, cannot be recognised; still, if the children continue to refuse it, the pulp is mixed with chocolate, and a new kind of aliment is obtained, the taste of which is more palatable. The quantity of raw meat thus administered to children ought not to be considerable at first, because they may dislike it, or suffer from indigestion. The dose for the first day may be ten grammes (four drachms) given at four separate times; the next day twenty grammes; the day after, thirty grammes, and so on in succession, until as much as 400 grammes may be reached; and then when the diarrhœa has ceased, the quantity of raw meat may be gradually diminished, to give place to other nutriment, such as broth, eggs, &c. From the commencement of the treatment, all accessory nourishment is interdicted, and only mild demulcent drinks are allowed. If the stools are examined on the first day, it is usual to find the meat in the same state as it was swallowed, and the fæcal matters, which are excessively fœtid, are composed of colourless fibrine, a little cellular tissue, and mucus. The treatment must nevertheless be continued, and a slight increase of strength is soon perceived; the child resumes its cheerfulness, plays about, and is soon entirely restored to health. When once accustomed to this kind of food, it sometimes happens that the children do not wish for any other; and often when flesh almost bloody is presented to them, they seem ardently to desire it.

It is difficult to explain how the pulp of raw meat is more easily digested by a diseased alimentary canal than meat cooked and prepared according to the refined processes of modern cookery; but the fact, however empirical, is no less certain. The pulp of raw meat is not only applicable to cases of chronic diarrhœa in young children, but to others occurring at more advanced years; and it has been successfully employed to effect an improvement in the general health of young persons. Still, it is in the former cases that this treatment has been most signally successful, and two cases are recorded as having recovered under this plan at the Hôtel Dieu, under Professor Trousseau.

In opposition to the opinions of M. Wiesse, it has been found in France that raw meat may be administered successfully to adults in certain cases of chronic diarrhœa. The cases in which it is probably most successful are those of the same nature as the infantile diarrhœa in which the beneficial effects of raw meat are most marked; namely, when the disease is accompanied by great debility, but without organic alteration of the structures.

X *On a Case of Acute Chorea treated successfully with Arsenic.* By Dr. Ronzier-Joly. (Bulletin Général de Thérapeutique, Oct. 15th, 1859.)

Dr. Ronzier-Joly was induced to try the effect of arsenic in chorea, from seeing an article recommending that treatment by M. Aran. The case was that of a boy, twelve years old, who was bitten by a dog, and who was suffering also under acute rheumatism. The latter disease yielded to ordinary remedies, but when he was convalescent he began to stammer and to perform irregular movements with his body. The arms were continually in motion, and the legs were almost as actively employed. The movements of the heart were regular, but frequent; the pulse 90, and compressible. Soon afterwards the patient was seized with an attack of true epilepsy, which lasted for a quarter of an hour. Dr. Joly then prescribed a mixture consisting of arsenious acid and water, the doses to be taken in increasing quantities, beginning with $2\frac{1}{2}$ milligrammes of arsenious acid on the first day, $3\frac{3}{4}$ milligrammes on the second day, and 5 milligrammes on the third day. At this period a little improvement was observed in the agitated movements of the patient. On the fifth day the dose of arsenic was raised to 6 milligrammes, and on the eighth to 8 milligrammes; after which the dose was again diminished, and continued at the rate of 4 milligrammes a day. On the twenty-sixth day from the commencement of this treatment, the boy articulated words, swallowed liquids with facility, and put out his tongue naturally; the irregular movements had completely disappeared, and the gait was less vacillating, notwithstanding the extreme weakness of the lower limbs. The boy's mother perceived at this time that some favous crusts on the head were diminishing in size and extent. The arsenious acid was continued in the dose of 3 milligrammes a day for some time after the disappearance of the symptoms. About three months and a half from the first attack of chorea, the report states that all medicines had been suspended for a long period, that there was no further sign of chorea, but there were a few rheumatic pains, and the favus had not altogether disappeared.

XI. *On Chinese Medicines.* By Dr. Schwarz. (Zeitschrift der Gesellschaft der Aerzte zu Wien., Sept. 19th, 1859.)

The Chinese apothecary prepares roots, barks, leaves, fruits, seeds, resins, oils, alkaline earths, metals, crystals, animal bodies and their several parts, especially their secretions and excretions, into infusions, decoctions, powders, pills, extracts, secret preparations, salves, plasters, &c. The mixtures, decoctions of plants, solutions of salts, &c., acquire, by the addition of brown sugar, and of mucous and gelatinous substances, a pretty uniform appearance and a similar taste. The soluble substances are often prepared before the patient in an enormous quantity of infusion of tea, and he drinks the medicine and the vehicle in the hot state; the powders are sold in small porcelain and stone jugs, and the stoppers being unscrewed, have on their inner surface a little bone spoon, with which the medicine is drawn out: the pills, which are uniform, and very beautifully rolled and often gilded, are packed in air-tight white transparent wax globes, containing one or two doses. The plasters and salves, usually spread upon red cloth, have a variously-coloured paper envelope, written over with explanations and praises of the remedy.

The Chinese divide their remedies into two great classes—namely, those which produce fat, and aphrodisiacs. A large paunch is considered a great title to admiration, and the devotion of this extraordinary people to the fair sex is well known.

XII. *On the Chemical Composition and the Medical Employment of the Oils from the Liver of the Cod, the Skate, and the Dog Fish.* By M. DÉVERGIE. (Bulletin Général de Thérapeutique, May 15th, 1859.)

M. Dévergie's paper is a Report to the Academy of Medicine of Paris on a memoir by Dr. Delattre, of Dieppe, who has written on the chemical and medical properties of certain oils from the livers of fishes.

M. Delattre resides at Dieppe, and has therefore abundant opportunities of obtaining a perfectly pure oil, but up to the present time the purest oils have been procured in contact with the air. M. Delattre, however, has devised an apparatus for isolating the oil from the influence of the atmosphere. This object is effected by expelling the atmospheric air from the vessels in which the oil is extracted from the livers, and replacing the air by carbonic acid. By this process the operator avoids the formation of the oleic, sulphuric, and phosphoric acids, which would otherwise be formed. M. Delattre having thus obtained pure specimens of oil, he made twelve analyses of each kind, and he tabulates the quantitative and qualitative results, from which it appears that all the oils contain a very large proportion of oleine, with some margarine, and some very small quantities of chlorine, iodine, bromine, sulphur, and phosphorus. M. Delattre also ascertained that the iodine, bromine, chlorine, phosphorus, and sulphur are not in combination with the potassium and sodium, as was formerly supposed, but are in a free state. Another important fact was ascertained by MM. Delattre and Girardin—namely, that in the spring of the year cod-liver oil does not contain a particle of iodine. It is also ascertained that the livers do not yield an equal quantity of oil at all periods of the year; that the quantity increases from June to November, and then diminishes from November to March, when it is at its minimum. In comparing the chemical composition of the oils from the cod and the skate, it is found that the proportion of iodine is less by half in the latter oil, and that that of sulphur is less by a fourth; but on the contrary, that the proportion of phosphorus is greater by about a third. As to the dog-fish oil, it is richer in phosphorus and iodine than cod-liver oil, and contains rather less bromine and sulphur. The increase of iodine is double the loss of the bromine. Compared with the skate-oil, it contains two-and-a-half times more iodine, and only a fifth less of phosphorus. Chemically, therefore, it is richer in inorganic elements than the cod and skate oils, except as to the proportion of phosphorus in the latter. M. Delattre has extended his researches to the chemical properties of the different varieties of cod-liver oil, and has analysed, respectively, the pure, the amber-coloured, the light, the brown, and the black oils. From these analyses he draws the conclusion (which has already been established), that in passing from the purest to the black oil, there is a decreasing progression in the quantity of the inorganic constituent.

Those who explain the mode of action of cod-liver oil by reference to its chemical elements, attribute especial efficacy to its iodine, bromine, and phosphorus; but the fact is, that all the varieties of oil differ very slightly in the proportion of these ingredients. A physician, therefore, who employs the various kinds of oil, will find very little difference in their operation; for it is of little importance whether, in twenty-seven days, a patient takes $6\frac{1}{2}$ grains or $6\frac{1}{4}$ grains of iodine, or 4 grains or $3\frac{19}{20}$ grains of phosphorus, and so forth. M. Dévergie cannot agree in the views of those chemists who propose to supply the fish-oils by artificial oils; not because he overlooks the therapeutical importance of iodine, bromine, phosphorus, and sulphur with cod-liver oil, but because he thinks that the curative effect does not reside *solely* in those chemical elements. It is to the association of elements by nature that the special action of medicines is due, and these effects cannot be obtained when the elements are in an isolated state.

M. Delattre, in treating of the medical properties of the oils from the cod, skate, and dog-fish, arrives at the following conclusions:—1. That the physiological action of the fish-liver oils is the same, whatever may be the kind of oil employed. 2. These oils may be considered as succedaneous to one another, and may all be employed in the treatment of scrofulous, cutaneous and rheumatic affections. 3. There are affections which more particularly require the employment of some one oil. Thus, the cod-liver oil is more efficacious in scrofulous phthisis than the skate or dog-fish oil. The skate oil effects more rapidly the cure of serous diarrhœa, and of mesenteric engorgement in children during dentition; indeed, this is the only remedy employed by M. Delattre in such cases, which are very frequent at Dieppe. The skate oil also succeeds better than the other oils in the treatment of cutaneous diseases and of chronic rheumatism. 4. The dog-fish oil appears to exercise a special action upon alterations of the bones, and in all cases it may be advantageously substituted for cod-liver oil. M. Delattre does not even hesitate to give it a marked preference in the treatment of scrofulous affections. In reporting upon these views of M. Delattre, the Commission offers no decided opinion upon their validity, as time and experience will be necessary to confirm or confute them; but it has endeavoured to solve one of the propositions—namely, whether dog-fish oil can be advantageously substituted for cod-liver oil, and if it may not even be preferable to it in some cases. This question is of the more importance because the cod fishing sometimes fails, while that of the *squalus catulus* (the dog-fish) never fails; and because the cod is a fish of a certain value, always meeting with purchasers, while the dog-fish is of no value at all, and is usually a source of annoyance rather than profit to the fisherman. The dog-fish oil sent to the Commission was very limpid, of a clear yellow colour, of a less powerful smell than that of cod-liver oil, and of a less disagreeable taste. Its effects were tried upon twenty patients in the Hôpital St. Louis, and to all of them the dog-fish oil was administered for a week instead of the cod-liver oil: two only of the number gave the preference, as to taste, to the brown cod-liver oil. A patient in whom the use of the cod-liver oil was suspended at several intervals and then relinquished altogether, was able to bear the dog-fish oil in a large dose until he was cured. This was not an isolated case, for in another instance a patient was able to bear the dog-fish oil, although he could not endure the cod-liver oil. Out of twenty patients who took dog-fish oil at the same period, eighteen preferred it to cod-liver oil; and, on the other hand, some patients who could not tolerate the cod-liver oil, were able to take the dog-fish oil; but still some persons could not tolerate either the cod-liver oil or the dog-fish oil. With regard to the therapeutical properties of the dog-fish oil, M. Dévergie, as the result of his observations, arrives at the conclusion that this oil produces all the effects of cod-liver oil, and cures with the same rapidity, so that it may be regarded as equally valuable with the latter oil. But further observations by other physicians have not altogether confirmed this view, and therefore the evidence before the Commission is at present insufficient to justify the formation of a definitive judgment as to the real value of the dog-fish oil, and the more special indications which it is calculated to fulfil; but it is sufficiently established that this latter oil may be substituted for cod-liver oil, a fact of considerable importance, since cod fish is often scarce and dear, while the dog-fish is always too abundant and very cheap.

XIII.—*On the Therapeutical Action of Mineral Waters in reference to their Chemical Composition.* By M. DESCHAMPS. (Bulletin Général de Thérapeutique, December 15th, 1858.)

The question proposed by the author is the relationship existing between the chemical composition and the therapeutical action of the mineral waters. Although the effects are by no means doubtful when the waters contain distinct principles in considerable quantity, yet when these agents are almost or wholly wanting, it is very difficult to explain the results which are observed. It is alleged that artificial mineral waters do not possess the properties of the natural waters, and yet when they are closely examined, it is found that a certain number of natural waters are in reality only artificial waters, and can exercise no more power than the latter. In fact, what is a sulphuretted water unless it be an artificial water? All sulphuretted waters are produced by an alteration of the sulphates held in solution in the water, the sulphates being decomposed by organic matters. Whatever may be the case, however, the inefficacy of the artificial waters cannot be admitted as a principle, for those which contain specific agents, such as gases, purgative matters, soda, &c., positively produce medicinal effects. These effects are not to be compared with those of the waters taken at the springs, but they are analogous to those of the waters taken at a distance from their source. Now if a comparison be drawn between the effects produced by artificial waters, or natural waters drunk at a distance from their source, on the one hand, and the effects of natural waters drunk at the springs on the other, it will be found that in the first case the treatment is solely medical, while in the other it is general as well as medical. In the first case, the patient's habits are not altered, and the hygienic conditions remain the same; but in the second case, everything is changed and modified, and the senses are variously affected. The appearance of strange places, locomotion, amusement, forgetfulness of business, and open air, exercise a great influence upon the organism. A patient, in addition to drinking the waters, uses daily baths to stimulate the skin, he is shampooed, reposes after bathing, &c.; and all these accessories contribute very materially to the beneficial effects of the waters.

Thus the efficacy of the natural mineral waters when drunk at the spring may be easily explained, and it is thus evident how the small quantities of chemical agents often contained in these waters may stimulate and excite the bodily organs, and give them power to resume the functions which they exercised before the commencement of illness. But it follows from this view that the natural mineral waters need not always contain much saline matter to act on our system, and that their action is not always in a direct ratio to the chemical reagents which they contain, for these agents alone constitute the therapeutical instruments of cure. As to the substances contained in the waters in infinitesimal proportions, it is doubtful whether many of them ought to be taken into consideration.

The author of this paper wonders why physicians who prescribe sea-water baths, do not also recommend the internal use of sea-water, since it is undoubtedly one of the most natural, efficacious, and abundant saline waters which can be procured or employed. Many properties are combined in this water, and yet it is neglected for waters which do not possess nearly so much value, and which are sought for only because they are distant, isolated, and difficult to be obtained. The administration of sea-water is very simple. Its use should be commenced by taking a very small quantity, either alone or mixed with common water, and increasing it gradually until the bowels are opened, but it should never be drunk to such an extent as to produce really purgative effects. The aperient result is a certain method of ascertaining the limits of the quantity to be taken. By combining the internal use of this water with bathing, the efficacy of sea-water baths would be much increased, and great advantages would be obtained in all respects.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

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I. Diseases of the Nervous System.

A Case of New Formation of Grey Cerebral Matter. By Dr. C. Tüngel, of Hamburgh. (Archiv für pathol. Anat., &c., Band xvi., hefte 1, 2.)

A female, aged thirty-one, was brought into the Hamburgh Hospital on the 26th November, 1858, in a state of sopor; she only gave short replies when repeatedly questioned, and without having understood the questions; she occasionally uttered brief exclamations. When attempts were made to open the eyes, she closed them convulsively; attempts at moving any of the limbs were resisted, but this was easily overcome, except the spasmodic contraction of the right hand. There was a dislocation of the right arm inwards. Respiration was accelerated, but no disease could be discovered in the lungs. As the patient was considered to be moribund, no treatment was adopted, except the application of a blister to the chest. She died six hours later, and it was afterwards ascertained that she had been subject to dislocation of the arm, that she had been indisposed and peculiar in her manner for four weeks previously, during which time she had been seen only once by a medical man, who considered her hysterical. There were various rickety distortions in the trunk and lower extremities. The vessels of the dura mater and pia mater and the cerebral tissue contained much blood; the arachnoid was not opaque, and there was a considerable amount of reddish watery exudation under it. The lateral ventricles contained a little fluid of the same kind. At the point where the upper wall of the lateral ventricle bends down, there were, on the outer side, between the middle and end of the posterior horn, several hemispherical tumours projecting into the cavity, varying in size, and on section apparently identical in hue and consistency with grey cerebral matter. These deposits extended into the medullary tissue, so as to form circular tumours, which were separated from one another by intervals of white matter. The deposit was greatest at the end of the posterior horn, and here the consistency of the brain was most developed. Both lateral ventricles presented the same appearances. There was no other abnormity in the brain. The microscopic examination of the new formation exhibited a fine granular mass with granular cells; nerve tubes were not seen. From the remainder of the autopsy, we merely note that the uterus was divided into two compartments by a septum, that there were two cervices and two vaginæ, with a single circular hymen.

On Diphtheritic Paralysis. By Dr. Maingault. (Archives Générales de Médecine, October, 1859.)

Various French authors have drawn attention to the fact that paralytic affections occur as sequelæ of diphtheria, or rather of the disease to which Bretonneau has given the name diphthérite. Dr. Maingault discusses the subject fully, and adduces numerous cases in illustration of his remarks. The following is a brief summary of the account which he gives:—Two or three weeks after all throat affection has disappeared, the first symptoms of paralysis show themselves; they are developed slowly; the patients may even have made considerable progress towards recovery before they occur. The first thing noticed is a paralytic affection of the soft palate, characterized by a difficulty of deglutition and a nasal speech—phenomena that may entirely disappear when the general muscular weakness shows itself. In some patients there is sudden emaciation. Vision becomes imperfect, and even complete blindness may supervene. The strength fails gradually; formication occurs in the extremities, accompanied by more or less severe pains in the joints. Walking becomes more and more painful, until the upright position is impossible. The paraplegia is then complete. The upper extremities partake in this weakness, the head becomes too heavy and sinks on the chest, the muscles of the trunk are incapable of sustaining the weight of the body. Strabismus, distortions of the face, dribbling, defective articulation, and paralysis of the bladder and rectum also supervene. There is an entire absence of fever, the pulse is small, and is reduced even to fifty, at the same time the heart's action is tumultuous, and there are anæmic murmurs. With these and other symptoms of defective innervation, the intellect remains intact, but the mental powers are sluggish. The disease may proceed to a fatal termination, or if it terminates favourably, the patient's strength returns gradually, and a cure is effected in a period varying from two to eight months.

Dr. Bouillon-Lagrange, in one of a series of articles on angine couenneuse, contained in the 'Gazette Hebdomadaire,'* also draws attention to diphtheritic paralysis, of which he adduces four cases. He regards it as a complication occurring mainly in the adult, that its duration is from two to three months, that the effect of treatment is very doubtful, and that it depends upon a serious alteration of the blood, the restoration of which is essential to recovery. Dr. Bouillon-Lagrange asks why this important complication has scarcely been noticed in previous epidemics, and is inclined to attribute it to the modifications in the epidemics of 1857 and 1858 by the continued dryness of the atmosphere which accompanied their development and progress.

II. Diseases of the Vascular and Respiratory System.

Tubercle in the Heart. By Dr. F. v. Recklinghausen. (Archiv für pathol. Anat., Band xvi., hefte 1 and 2.)

This is probably a unique case of miliary tubercle under the endocardium, occurring in a person aged twenty, who died of arachnitis, and exhibited acute miliary tuberculosis in the lungs, pleura, liver, spleen, kidneys, thyroid, and prostate. About twenty small, opaque, circular or elliptic nodules were found under the auricular and ventricular endocardium, embedded in the muscular tissue, and varying from one to one and a-half line in diameter. Under the microscrope they were found to consist of moderate-sized corpuscles, generally with a granular nucleus, and a dark, finely granular substance. The adjoining muscular fibres were atrophied.

On the Position of the Heart in Emphysema of the Lungs. By Dr. J. Klob. (Zeitschrift der Gesellsch. der Aerzte zu Wien, 1859, No. 6.)

In a paper in which Dr. Klob discusses the opinion that in emphysema the apex of the heart is felt at the epigastrium, owing to the organ being pushed into the middle line, he makes some remarks on the post-mortem changes in the position of this viscus which deserve notice, not only because they appear to

* Gazette Hebdomadaire, 24 Juin, 1859.

vitiate conclusions arrived at in reference to the heart by pathological anatomists, but also because they are suggestive of the necessity of caution in regard to similar questions affecting other viscera.

The rigor mortis fixes the muscles in the position which they occupied in the corpse; but long before the extremities have lost their rigidity, the thin diaphragm has become flabby, and owing to the early distension of the intestines, is pushed upwards, so as to induce a change in the heart's position. In order to determine this point more accurately, Dr. Klob percussed the heart of numerous corpses three hours after death, and then inserted a long needle into the apex, while the rigor mortis was still well marked. Before the rigor appeared to diminish in the muscles of the face or of the extremities, the position of the needle was altered, the external end of the needle moving downwards, and distinctly towards the right,* so that in some cases Dr. Klob inferred a movement of the heart's apex of from one and a-half to two inches. He therefore concludes that in every corpse the upward movement of the diaphragm causes the heart to assume a horizontal position at a very early period, and before other phenomena of putrefaction occur.

Dr. Klob does not deny that emphysema may induce a horizontal position of the heart, and that the epigastric pulsation felt in these cases may be due to the impulse of the right ventricle; but he does not consider the fact to be proved by autopsies, on the ground above-mentioned. In the cases of emphysema which have fallen under his notice, he has found that the heart occupied a horizontal position, so that the right wall of the right ventricle was in contact with the anterior wall of the thorax in the triangular space of the anterior mediastinum. But where he had marked the point of maximum impulse of the heart during life, he found at the autopsy neither the apex nor the right ventricle; but the latter above, the former to the left of the point that had been marked.

Inquiry into Sweating of Blood† *and Neuropathic Hæmorrhages.* By Dr. Jules Parrot. (Gaz. Hebdom., Nos. xl., xli., xliii.)

The occurrence of hæmorrhage on the surface of the body without solution of continuity and from internal causes, is so rare that a well-authenticated case deserves all the attention and analysis that Dr. Parrot has bestowed upon the one that has fallen under his observation. The following are its prominent features:—Mad. X., born in 1832, when seven years old was affected with scrofulous ulcers of the righth and, which cicatrized after two years' treatment; later on, the cicatrices were the seat of a sanguinolent exudation, occurring without pain, and often without appreciable cause. One day, under the influence of severe grief, her tears were coloured with blood, and from this time the knees, thighs, chest, and the margin of the inferior eyelids, exhibited the bloody sweat at irregular intervals. At times the blood suddenly inundated the face, so that the patient looked as if she had been assassinated. The menses occurred at the age of eleven years, and for a time the symptoms were in abeyance, but soon returned with increased force. The hæmorrhage was commonly due to mental emotion, and was associated with a temporary loss of motor and sensory power. She married at fifteen years of age; but the attacks became more severe, lasting at times one or two hours. They disappeared during the first pregnancy, and for a year after. Mad. X. appeared to be improving at the beginning of 1858; but after the severe illness of her child, she was seized, on the 1st of April, with

* This evidently means to the right of the observer.

† For an account of most of what is known on this subject the reader is referred to Dr. Copland's Dictionary, vol. ii. p. 72; Gendrin, Traité Philosophique de Médecine, vol. i. p. 276; and Grisolle, Traité élémentaire de Pathologie, vol. i. p. 644; and Grandidier, die Hemophilie. Leipzig, 1855.

a severe attack of unconsciousness, and hæmorrhage from the face, from which time Dr. Parrot was called in to attend her. He found her suffering from agonizing pains, alternately affecting the epigastrium, the inguinal and vulvar regions, the thighs, head, and thoracic parietes. On the 25th, the lady came to Paris: her period was somewhat behindhand, and she had lancinating pains in every part of the lumbar region. Towards 4 p.m. they attacked the inguina, thighs, breasts, head, hypochondria, and epigastrium, and on these disappearing under the influence of chloroform, she had three epileptic fits. A circumscribed spot on the scalp then became painful, and Dr. Parrot saw the blood exuding from there, and drying up immediately after; subsequently all the painful points became the seat of bloody sweat. It formed a chaplet round the roots of the hairs, and flowed in sufficient quantity from the lower eyelids to allow of several drops being collected. Both before and after the discharge, the skin presented its normal appearance, not exhibiting any injection or spot. After several bilious vomitings, sleep was induced at 11 p.m. by a full dose of muriate of morphia. The day after the catamenia appeared, and the patient gradually improved, the attacks becoming less frequent, till they disappeared entirely. Four similar attacks, wwere witnessed by the author subsequently to the one just described, on the 28th Sept., 1858, the 17th Nov., 1858, the 25th and 28th January, 1859. During the intervals, the patient enjoyed perfect health, looking well and healthy. Her intellectual faculties continue unimpaired, and even after the severest epileptic seizures she suffers none of that prostration often witnessed after epilepsy.

Dr. Parrot's treatment has been directed towards combating the neuralgia during the seizures, and the strumous and chlorotic diathesis during the intervals. The former object he obtained best by chloroform inhalations, the latter he pursued by the exhibition of preparations of iodine and iron. After detailing the above case, the author goes with some minuteness into the history and pathology of the affection: he concludes with regard to the latter, that it it is truly a secretion of blood from the sweat ducts, as evidenced by microscopic examination of the liquid, and close watching the surface from which the exudation takes place. He quotes several cases, the best and most complete of which is given by Professor Huss.* We can only make room for one remark as a warning, that these cases are quite distinct from those known as "bleeders," and characterized by the occurrence of hæmorrhage, which it is almost impossible to arrest, from any part of the body on the slightest abrasion.

On Congenital Vesicular Malformation of the Lungs, with Remarks on Cyanosis resulting from Pulmonic Disease. By Professor MEYER, in Zurich. (Archiv für pathol. Anat., Band xvi., hefte 1 and 2.)

This is a description of two preparations contained in the Pathological Museum of Zurich, of a kind not before noticed by recent morbid anatomists. The first was taken from a girl, aged one year, apparently healthy to her fifth month; then she presented symptoms of asphyxia, with sibilant respiration, irregular and tumultuous action of the heart, aggravated by lying on the right side, and accompanied by blueness of the face and hands. The attacks occurred in paroxysms with increased frequency and violence, in one of which the child died. The discoloration of the skin remained during the intervals. The heart was found to be normal, the ductus arteriosus closed, but the foramen ovale sufficiently open to permit the passage of a bristle. The right lung presented the usual lobar divisions. On the inner surface towards the heart some large vesicles projected above the surface, varying in size from a hempseed to a pea. The left lung presented two main divisions into lobes; the lower one con-

* Hémophilie, cas de maladies rares, etc. Archives Générales de Médecine, Août, 1857, p. 165.

sisted of three tongue-shaped lobules, their parenchyma being throughout pervious to air. The upper part of the left lung formed a large fibrous sac, with very thin parietes, the pulmonary tissue being cut off abruptly at its margin. The sac was full of air, lined with a smooth mucous membrane, which presented full and prominent folds; the largest folds were found near the root of the lung, where they were found to overlay the orifices of the bronchi. There was some chalky deposit on the posterior wall of the sac. The sac measured vertically 111 millimetres, transversely 93 millimetres (4·36 by 3·65 inches). The second case occurred in a fœtus of 5–6 months, and presented a similar malformation, though in a less degree.

The author regards these cases as resulting from a vicious development, though he will not determine whether this be attributable to an arrest or to a morbid process occurring in the fœtus. Among the older authors, Professor Meyer has only succeeded in finding one case which truly resembles his own, and is given by Bartholinus in a paper entitled "De Pulmonum Substantia et motu Diatribe," which in its turn is contained in the works of Malpighi. (Leyden edition, 1687, vol. ii., pp. 3 and 9.)

III. Diseases of the Organs of Digestion.

A Case of Pancreatitis. By Timoteo Riboli. (Schmidt's Jahrbücher, Band 102, No. 5; and Gazz. Sarda, 11, 1858.*)

A robust female, aged fifty-four, came under the writer's observation, suffering from gastric disturbance, emaciation, loss of appetite and strength, horripilations, slight catarrh, eructations in the morning, with vomiting of a thin, sometimes viscid fluid of a saline taste, with a white tongue, and night sweats. The exhibition of magnesia and bismuth restored the patient so that she was able to go into the country for the harvest. On returning to town she again complained of her former symptoms. The epigastrium now proved to be distended, inducing a supposition that the disease was a circumscribed inflammation of the left lobe of the liver, which was supported by the presence of slight icterus, and a dull, deep-seated pain. Numerous remedies were tried successively and in vain; death ensued at last from gradual exhaustion of the vital powers from arrest of nutrition. The autopsy showed the pancreas to be in a state of complete suppuration; the gall-bladder was turgid with bile, the liver was gorged with black blood, the spleen normal, the intestines anæmic, the uterus and its appurtenances atrophic, the heart and lungs healthy, the surface generally pale and icteric. The case was observed in 1837, but not published earlier because the author wished to collect more cases of a similar kind. Of all his colleagues who were consulted about the above case, Tommasini alone had made a correct diagnosis.

On Fatal Steatosis of the Liver and Kidneys. By Professor Rokitansky. (Zeitschr. der Gesellsch. der Aerzte zu Wien, 1859, No. 32.)

This paper is published by the learned Professor to prove the existence of a form of fatty kidney which he regards as consecutive upon fatty degeneration of the liver, and as distinct from that which is commonly set down as one variety of Bright's disease. Death occurs suddenly in these cases, with symptoms of uræmic poisoning. After analysing three cases in detail, which Professor Rokitansky brings forward in illustration of his views, he sums up thus:—In subjects who are inclined to an excessive formation of fat, we meet with fatty degeneration of the liver, with which, sooner or later, fatty degeneration of the kidneys is associated; both, gradually and imperceptibly, attain so high a degree

* See also Gazette Hebdomadaire, Sept. 2, 1859, where a few similar cases are quoted.

of development, that at last the biliary and renal secretions cease, and death ensues speedily after the occurrence of slight icterus, from uræmia and hæmorrhagic decomposition of the blood. The three cases resemble one another so closely in their pathological aspects, that it will suffice for the author's meaning to adduce one of them here. We may mention, however, that they were all females, and respectively of the ages, twenty-three, thirty-eight, and eight. Franciska Gily, aged thirty-eight, was admitted into the Vienna Hospital on February 26th, 1859, having suffered for eight days in consequence of mental excitement, from constipation, vomiting, heat, and headache. Latterly there had also been spasms in the face and extremities. When admitted, she presented an anæmic appearance, pulse 60, tenderness in both hypochondria on deep pressure; in the night delirium supervened, slight icteric colour on the following morning; in the afternoon, sopor and death. The autopsy was made on the following day.

"The body was well fed, fat, feebly icteric. But little blood in the meninges and brain. The lungs pale red anteriorly and above, behind and below full of blood. The sheath of the pulmonary veins of the left side presented a reddish-black suffusion. The pleura, mediastina, and peritoneum extensively suffused. The pericardium contained 1½ oz. of yellow serum. The heart collapsed, yellowish-brown, friable, containing a small quantity of fluid dark red blood, with a few scanty flocculi of fibrin. *The liver was large, pale yellow, bloodless, pulpy, soft, fatty, with a slightly icteric colour in the centre of individual lobules; the gall bladder contained some drops of grey mucus.* The spleen was enlarged, swollen, dark red, pulpy. The stomach contracted, marked with a few hæmorrhagic erosions at the fundus, containing some dark-brown fluid. The intestines were distended with gas, and pale; the colon contained dark brown consistent fæces. *The kidneys presented a pale yellow cortical substance, with delicate white spots, and dotted red, with the injected Malpighian tufts; the pyramids were pale red. The renal calices and pelves, the ureters and bladder, contained a turbid mucous fluid.** The uterus was 3½ inches long, 5 lines thick, friable, its inner surface covered with a red granulating layer, with numerous Nabothian vesicles in the cervix, a contracting cicatrix above the os tincæ. The uterus contained a small coagulum of blood. The ovaries cicatrized and contracted, the Fallopian tubes normal."

We would draw our readers' attention to the observations on scrofulous enlargement of the liver by Dr. Budd,† where an analogous relation is shown to exist between the hepatic and renal affection as that maintained by Professor Rokitansky in his cases. Dr. Budd's cases were all very chronic, and the deposit manifestly different from that observed in Vienna; the microscopic appearances in the liver and kidneys in the latter being essentially those of well-marked fatty degeneration, while in the former the deposit in both organs was mainly albuminous. As cases of acute fatty degeneration of the liver, or, as they are also called, acute yellow atrophy, are rare, we may briefly allude to a well-marked instance‡ which occurred in the wards of Dr. Uytterhoeven, at Brussels, and where death ensued within six days from the first commencement of the illness. The patient was a young woman aged twenty-four, enjoying excellent health up to the time of the seizure, for which no other cause could be assigned than an altercation with her brother a few days previously. The febrile symptoms were followed by icterus, cephalalgia, and extreme prostration, with tympanitis and pain in the hepatic region. The day after she was brought to the hospital—the fourth day after the first seizure—she looked as if suffering from puerperal intoxication, still she was sensible, and relished her food. The diagnosis of acute softening of the liver

* The italics are the Professor's.

† On Diseases of the Liver. By Geo. Budd, M.D., F.R.S. Third Edition. London, 1857.

‡ Annales de la Société Anatomo-pathologique de Bruxelles, No. 2, 1859.

was verified by the autopsy, which showed the liver of normal size, its convex surface adherent here and there to the diaphragm, with spots of inflammation which extended into the parenchyma; the latter was of a peculiar deep yellow, softened and diffluent. Under the microscope it presented itself as an almost colourless liquid, in which floated enormous globules of emulsified fat. Scarce a trace of the normal tissue was to be found. Treated with ether, nothing remained but an interlobular fibrous tissue, and when put into water, the liver floated. In this case the kidneys do not appear to have been seriously diseased; they are described as congested, the capsule thickened, hyperæmic, and scarcely at all adherent to the organ.

We only draw attention to the fact that although these cases are promiscuously called acute fatty degeneration, acute yellow atrophy, or acute softening of the liver, atrophy is by no means uniformly met with. It did not exist in the case just quoted, nor in those of Rokitansky. It is not improbable that different morbid conditions may as yet have been classed together, which a more intimate knowledge of the subject may distinguish.

Pathological and Clinical Researches regarding Ulceration and Perforation of the Appendix Vermiformis. By E. Leudet, Titular Professor, &c. (Archives Générales, Août, Septembre, 1859.)

Ulceration of the mucous lining of the appendix has been met with by Professor Leudet in 18 cases, and he is of opinion that phthisis pulmonalis is the most frequent cause of this disease. He is also disposed to discredit the ordinarily received opinion that perforation of the vermiform process most frequently results from the presence of foreign bodies. Here, too, phthisis plays an important part, inasmuch as out of 13 cases of perforation occurring under Leudet's observation, 6 were consecutive upon phthisis pulmonalis. The author has not met with one case of perforation of this part resulting from typhoid fever. The consequences of perforation vary: they are, general or local and circumscribed peritonitis, iliac abscess, communication between the perforated appendix and the neighbouring organs, as the small intestine, the cæcum, rectum, bladder, or internal iliac artery; inflammation of the portal vein, &c.; circumscribed peritoneal inflammation has been noticed most frequently by our author; it occurred in 11 out of the 13 cases of perforation, a circumstance that renders the prognosis less unfavourable than it would otherwise be, as it may lead to a limitation of the disease by the formation of adhesions and a cyst. The cæcal perforation is not generally rapidly fatal. Its diagnosis is difficult; we may suspect it when partial peritonitis of the right iliac fossa supervenes in subjects who have been previously well, or in the course of phthisis or chronic enteritis.

IV. Diseases of the Blood.

Clinical Lecture on Typhoid Fever. By Edward Goodeve, M.B. (Indian Annals, January, 1859).

We here receive a further confirmation of the fact which we have more than once had occasion to bring before our readers,* that typhoid fever is a disease occurring in India as an idiopathic disease, and quite distinct from those febrile conditions of an adynamic type which are associated with cholera, asthenic pneumonias, and the like. The cases reported by Dr. Goodeve bear out his views, though he has been unable to give any post-mortem account owing to the hospital patients having recovered, and the fatal cases attended in private not having afforded an opportunity for making autopsies. The cada-

* British and Foreign Medico-Chirurgical Review, Oct. 1857, p. 536, and Oct. 1859 p. 325.

veric evidence has, however, been supplied by Dr. Scriven in the papers already referred to.

Rheumatism and Allied Diseases. By C. A. Gordon, M.D. (Indian Annals, January, 1859.)

In an interesting paper on rheumatism as occurring in India, Dr. Gordon brings forward evidence to disprove the opinion which has hitherto prevailed, that the disease is not frequent in that country. The rate of its occurrence in the home-service is stated at 6·89 per cent. of mean strength, whereas the author finds it to be in India among soldiers 10·32, among officers 10·26, among women 2·74, and among children 0·17, per cent. of mean strength. Besides atmospheric alternations, which the author admits to be the most frequent cause of rheumatism, he considers endemic influences to exercise a material influence, because the rate of occurrence varies so remarkably at different military stations. Dr. Gordon admits with Dr. Morehead that acute articular rheumatism is not so common in India, the acute rheumatism which is met with so frequently being, we presume, the diffuse rheumatism of the fibrous tissues of the limbs. Dr. Gordon differs from Dr. Morehead in thinking that cardiac inflammation is really rarer in India than in Great Britain, and that this circumstance is not to be accounted for by the army surgeons overlooking the symptoms of heart-affection. He regards it as a corroboration of this view, that the cases of heart-disease bear a very inconsiderable ratio to the numbers admitted for rheumatism, and that the men invalided for disease of the heart in India is not a tithe so large as in the United Kingdom.

V. Diseases of the Integuments, and Miscellaneous.

Note on Aden Ulcers, compiled from Official Documents. By W. C. Coles, M.D. (Transactions of the Medical and Physical Society of Bombay, No. IV., New Series.)

The Aden or Yemen ulcer is a disease common in the place from which it takes its name, among the native poor and destitute. Dr. Craig, the medical superintendent of Aden, is of opinion that it is not, as has been hitherto supposed, a form of scurvy, but another form of Beriberi, dependent upon cachexia, or general weakness of the muscular and circulating system, produced, or at all events accompanied, by a more or less watery or other abnormal state of the blood, whereby the reparative power is weakened. It is remarkable that the Jews appear exempt from the disease, a circumstance attributed to their circulation being more vigorous, and to the fact that although poor, none are destitute. We are not favoured with an exact account of the ulcers; but it appears that they affect the legs, putting on a very ugly appearance, sometimes surrounding the whole limb, and frequently sloughing. Dr. Craig was of opinion that an accumulation of these cases in the hospital was one great reason of the ulcers proving so serious, in fact, he regards hospital gangrene as the real cause of the mortality which has been noticed at Aden. "The hospital," he says, "was imperfectly ventilated and badly situated, immediately on the high-water mark, each receding tide leaving an extensive surface of mud, mixed with much decaying marine animal matter. The floors of the hospital were damp—sufficient of itself in cold weather to convert healthy ulcers into foul. The walls, constructed of matting, acted as a sponge for sucking up putrid and other emanations, becoming in the end a focus of infection." Accordingly Dr. Craig suggests that ulcers should no longer be received into hospital, but treated in dispensary style, a system which appears to have proved beneficial, and to have arrested the disease.

On Eczema. By Professor Hebra. (Prager Vierteljahrsschrift, xvi. Jahrg. 1859; and Allg. Wien. Med. Zeitung., 1859, 6 et seq.)

In order to simplify the conflicting views entertained with regard to the nature and appearances of different forms of eczema, the author suggests that we should establish a type by an artificial process which induces an eruption resembling eczema in its characteristic form. For this purpose he selects croton oil, which, as is well known, when rubbed on the skin commonly induces a vesicular eruption. When, however, applied on several persons at the same time and in the same manner, certain differences are observed in the effect: at first, there is a diffuse reddening of the surface, and if the skin is delicate there will be vesicles after a few hours, the type of an incipient eczema; in others, there will only be some prominent papulæ; again, in others there are only red dots, and in one or two cases no effects whatever will result. The papulæ and vesicles, as we have ourselves frequently noticed, are to be found at the orifices of the hair-follicles, and are caused by hyperæmia and exudation of the part. If croton oil be repeatedly rubbed in there will be vesicles in all after a time; they become larger, more numerous, and confluent, the epidermis at last bursts, owing to the increased distension by its fluid contents. This gives rise to a new series of appearances, and we now have to do with a moist surface, studded with red spots, the basis of the former vesicles. In accordance with these effects, Professor Hebra establishes four varieties:—1. Pityriasis rubra. 2. Eczema papulosum, lichenoides sive lichen eczematodes. 3. Eczema vesiculosum (Willan's eczema solare). 4. Eczema rubrum sive madidens. If the rubbings are now stopped, the nodules or vesicles collapse and the epidermis desquamates, or the moist parts form crusts. If the rubbings are continued, pus forms in the clear fluid, which becomes opaque and thick, the pus dries and forms crusts. In this way we obtained an analogue to the fifth variety of eczema, eczema impetiginosum or impetigo eczematosa.

Variations are also observed according to the locality in which eczema appears; which will be easily understood by a consideration of the anatomical relations and a reference to the above principles.

Case of Herpes Zoster occurring a second time. By J. Forrest Kennedy, M.D. (American Journ. of Med. Sci., July, 1859.)

A farmer, aged sixty-six, came under Dr. Kennedy's treatment for a well-marked case of herpes zoster, and stated "that he had had an eruption similar to it about twenty-five years ago, and that his physician then called it shingles." In the present instance it occupied exactly the left half of the body, from the linea alba to the spine. Except that the attack was preceded and followed by more than the usual amount of pain, it ran the usual course. Although the evidence as to the first attack having been herpes zoster may not be quite satisfactory, we put Dr. Kennedy's statement on record, because the occurrence of the disease more than once in the same individual is certainly exceptional and has been questioned.

Researches into the Development and Propagation of the Trichocephalus and Ascaris Lumbricoides. By Dr. C. Davaine. (Journal de la Physiologie, Numero VI., Avril, 1859.)

As a result of numerous examinations, Dr. Davaine states that the trichocephalus dispar, a parasite which he estimates to affect the cæcum of every other inhabitant of Paris, is not developed from the ovum in the intestine, but that the ova were always expelled in the same condition in which they were laid. He tried to induce their development by placing them in water, but failed for a long time. At last he succeeded in the following manner: the ova were washed (Sept. 1857) repeatedly for several successive days, until the water was colourless and odourless. This water was renewed from day to day, and the ova examined by the microscope every week. It was not till six months after (April, 1858) that symptoms of development presented themselves in those of the ova which had preserved their vitality. The yolk then assumed the form of a rounded mass, and became more consistent. A few days later the yolk in some of the ova divided into two, and then into four parts. Each part subsequently underwent further division, so as at last to have a mulberry shape. This appearance continued until the 12th June, when a well formed embryo resembling the adult became visible, and showed distinct movements. Dr. Davaine satisfied himself equally that the ova of the ascaris lumbricoides are not hatched in the intestine, and that when treated like those of the trichocephalus dispar, they require six and more months, according to the temperature to which they are exposed, to induce the development of the embryo.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *On Tibio-tarsal Amputation.* By Professor Michaux. (Presse Médicale Belge, 1859, No. 33.)

M. Michaux, Professor of Clinical Surgery at the University of Louvain, recently presented to the Brussels Academy of Medicine an interesting memoir upon tibio-tarsal amputation, as compared with supra-malleolar amputation, and amputation at the place of election. The following are the conclusions:—

1. Tibio-tarsal amputation is an operation properly admitted into surgical practice. 2. It should replace supra-malleolar amputation whenever the lesions which render it necessary are limited to the constituent parts of the foot, to the upper part of the malleoli, or even to the articular surface of the tibia. 3. It should only be performed when a flap can be formed of the soft parts of the heel, so that these may constitute a mattress for the bones. 4. If the soft parts of the heel are destroyed, or have undergone such alteration as to be unfitted for the purposes of a flap, the tibio-tarsal amputation should be renounced, the supra-malleolar operation being, as a general rule, substituted for it. 5. The best mode of performing tibio-tarsal amputation is by Syme's procedure, as modified by Jules Roux, of Toulon, taking, however, the precaution of preserving less integuments at the external side of the articulation (a modification of the author's) than is done by the latter surgeon, cutting the tendo-Achillis sufficiently high, and excising the posterior tibial nerve, as recommended by M. Verneuil. 6. Pirogoff's operation may be had recourse to when the calcaneum is entirely healthy, or has undergone but little alteration at its anterior part. 7. Tibio-tarsal amputation is more difficult, more dangerous, and more slow in its after-treatment than supra-malleolar amputation, but standing and walking being performed directly on the stump, are better executed after the former than after the latter. 8. The prothesis after tibio-tarsal amputation is far more simple and less expensive than that employed after supra-malleolar amputation. Syme's boot constitutes an excellent apparatus. 9. The best procedure for supra-malleolar amputation consists in forming an anterior flap, which, falling by its own weight, covers the bones. This is the elliptical operation with anterior flap of M. Poupart. 10. A procedure similar to that described by Baudens for disarticulation of the knee, should be adopted in amputation at the place of election, whenever the lesions admit of the preparatory mode being chosen. 11. Amputation at the place of election is more difficult and far more dangerous than supra-malleolar am-

putation, but walking is more easily performed after the former than after the latter. 12. To sum up. (a.) Whenever the pathological condition allows of the surgeon choosing the place of amputation, he should prefer the tibio-tarsal in young, strong subjects endowed with a tolerably good constitution, whatever may be their position, sex, or profession. (b.) Supra-malleolar amputation is suitable for aged and enfeebled subjects, who are called upon by their position to walk but little, and can obtain a good artificial limb which assumes nearly the normal form of the extremity. (c.) Amputation at the place of election should be preferred to the supra-malleolar for young subjects, and especially children, who almost always recover after amputation. Also in strong subjects of a good constitution, to whom it is of greater importance to walk well than to disguise their mutilation, and whose position in life obliges them to undertake laborious occupations.

II. *On the Mechanism of Sinuous Ulcers.* By Professor ROSER. (Archiv fur Physiol. Heilk., 1859, pp. 218—225.)

Professor Roser observes that the practice of cutting away undermined portions of skin, although an old one, is far from being resorted to so frequently as it deserves to be, and is scarcely alluded to in the most recent text books. Those who do resort to it do so for the most part as a mere empirical procedure; and can give no account of why these undermined portions of skin will not heal, or why an ulcer, which may have remained for months unhealed, will take on reparative action only a few days after excision of such portions has been performed. So, too, there is wanting the means of determining the cases in which the undermined portions should not be cut away, and the healing brought about by means of suitable compression. In this state of things it is Dr. Roser's object, in the present paper, to explain the mechanism of sinuous ulcers, in order to furnish something like a scientific explanation of the mode of treatment which has been found practically so useful.

Sinuous ulcers are most frequently met with in the scrofulous suppuration of the cervical glands. By the prompt opening of such abscesses we may anticipate the undermining process, and do much to hasten healing, and prevent disfiguring cicatrices. And when even the skin has become undermined, if we but cut it away, we may, with few exceptions (as when the infiltrated glandular substance is laid bare), still secure a quickly-formed and proportionally well-looking cicatrix. But if we leave these abscesses quietly to themselves, waiting for spontaneous perforation, the undermining suppurative process leads to a gradual thinning and atrophy of the skin, one hole opens after another, and after tedious discharge, a wrinkling of the skin takes place, and hideous irregular scars result. The undermined skin, in a condition of atrophy from disturbed circulation and innervation, and from venous stasis, is not in a condition to undergo the healing process. On cutting such portion of skin away, it is found remarkably void of blood (as indeed its blue colour already indicated), a small portion of dark venous blood alone flowing away. Its sensibility, too, is almost entirely lost. It is true that after a very long period these ulcers will heal of themselves, the undermined skin first retracting at its base, converting the irregular into a simple ulcer, which then cicatrises from its circumference. The tedious part of the healing is the retraction of the undermined skin, and the process is immensely hastened by the removal of this part, which after all is lost, whether removed or left.

If, however, the removal of this skin is advantageous in simple subcutaneous abscess, which by perforation or dividing in the middle has become a sinuous ulcer, it is of far greater utility in undermining abscesses having numerous perforations. Such multiple perforations are common in the undermined and atrophied condition of the skin, and they are sometimes even advantageous, as the fusion of the several holes into one will promote the gaping of the opening and the retraction of the undermined tissue. But in a great number of cases these multiple perforations give rise to the production of half-isolated, bridge-formed, or tongue-like slips of skin, which are quite incapable of undergoing the process of healing. When they do not entirely disappear under the influence of progressive ulceration or atrophy, they give rise to a peculiarly ugly form of cicatrix, termed by the author the "bridge cicatrix," and the "lappet-cicatrix." The skin cicatrising behind these bridge-like slips of integument, they remain stretched over it as projecting cords, and are especially observed in the cervical and inguinal regions. Their disagreeable appearance renders it necessary to cut them smooth off with the scissors, an operation which, on account of their bloodlessness and little sensibility, gives rise to but little inconvenience. In like manner when the small tongue-shaped lobules of skin following multiple perforation are not removed by the knife, they give rise to the production of small projecting lappet-cicatrices, attached by a pedicle, and resembling warts, or having a broad basis. These also must be cut off with the scissors.

What has already been said will indicate the circumstances under which the undermined skin may still be considered as capable of adhering again. When it has become thinned and blue, its nutrition may be considered as arrested; but as long as any of the *panniculus adiposus* remains, it is capable of healing. Frequent division is the best treatment for threatened integument, the edges of the wound retracting and becoming thicker, and the nutrition and circulation being facilitated. When the undermined skin continues well nourished, the healing may, under favourable circumstances, be brought about by compression; but these cases are rare, as various complications, such as exposure of bone or fasciæ, infiltration of the cellular tissue, the retention of decomposed pus, or dyscrasis, may furnish contra-indications. Compression, too, is in some regions difficult of application.

What has here been said of the skin will also apply to the *mucous membrane*. Undermining of this certainly is a much rarer occurrence; but it is met with, as in the case of the rectum, and more rarely the entrance to the vagina; and if the undermined portions are not removed, endless suppurations and the formation of the bridge-cicatrices will be the result. Many cases of fistula in ano are nothing else than this undermining process, affecting the skin and mucous membrane in common; and to secure rapid healing, excision of the atrophied membrane—not a mere incision—is required. When there is a mere fistulous tract without undermining, simple incision is alone required.

III. *On the Detachment of the Mucous Membrane in Laryngotomy.* By PROFESSOR PITHA. (Zeitschrift der Gesellsch. der Aerzte zu Wien, 1859, No. 11.)

Among the accidents which may arise during the operation of tracheotomy, the detachment of the mucous membrane at the moment of opening into the air-passages is one that deserves the greatest attention, and that not only because of the great increase of danger it confers upon the operation, but also from the confusion and perplexity it causes to the operator just when he has most need of all his presence of mind. When Dupuytren confesses to an error arising from this cause, how easily an inexperienced operator may be deceived, and passing a canula into a wound which does not penetrate into the trachea, may only aggravate the asphyxia which it is his object to relieve. The rapid rectification of the error is only possible to those who have been already warned of the nature of the accident by their own or other's experience.

The following case is in point. A soldier was brought into the Clinic, suffering from impending asphyxia, the consequence of some affection of the larynx. Sugillated leech-bites rendering the opening of any other part somewhat difficult, a free transverse incision was made into the crico-thyroidean ligament. A canula was easily introduced; and, although with so superficially-placed an aperture no suspicion of a false passage arose, no air issued, and the patient sank lifeless. Without an instant's loss of time, an incision was carried from the middle of the transverse wound downwards, though the cricoid cartilage and three rings of the trachea, and the canula passed through the gaping wound, the thorax being at the same time powerfully compressed. After some minutes the artificial respiration restored the patient to consciousness. We need not pursue the details of the case, it sufficing to say that the man, rallying at first, died on the fifth day after the operation, in consequence of pneumonia, with œdema of the lungs. There had been frequent obstruction of the canula, and somtimes difficulty arose in its re-introduction, from the presence of a kind of valve at the upper part of the wound. At the autopsy, the vertical portion of the T-shaped wound was found to gape considerably; but the horizontal first-made incision was almost entirely covered by a thick cuneiform wedge. The wound had penetrated into the larynx, but the mucous membrane of this had undergone an extraordinary amount of thickening (more than three lines) throughout its whole circumference. It had become rigid and hard, and much resembled a thick fibrous membrane. Over a full square inch this membrane had become detached from the thyroid and cricoid cartilages and intervening ligament, and was turned downwards and backwards, so as to form a thick cuneiform valve, closing the cavity of the trachea from above, and almost completely separating the upper wound from the lower perpendicular one. The cartilages whence this had become separated were in a condition of necrosis. Through the thickening of the mucous membrane, the cavity of the larynx was so narrowed that its walls were almost in contact.

This detachment is an accident which occurs in laryngotomy much more frequently than would be expected from the little mention made of it in published cases of operation. It may always be supposed to be present when chronic ulceration or stenosis of the larynx becomes complicated with perichondritis, a complication which especially renders bronchotomy necessary, by converting a supportable chronic condition into one of impending suffocation. The separation of the mucous membrane which ensues may easily give rise to mistakes in operating, either through the fact of the knife not penetrating it, or the canula getting entangled in it, and perhaps only causing its still further detachment, in place of passing into the cavity of the tube. The nature of the occurrence is so little understood, that it has been usually attributed to some fault on the operator's part; but in fact an artificial detachment of a normally adherent mucous membrane is entirely impossible.

IV. *On Ligature of the Lower Portion of the Popliteal Artery after Amputation of the Leg.* By M. Verneuil. (Archives Générales, Août, 1859, p. 156.)

M. Verneuil concludes his memoir with the following propositions. 1. After amputation of the leg at the place of election, the tying the ends of the arteries at the surface of the stump may be rendered difficult or impossible from various causes. 2. The causes are, retraction of the arteries, which render them invisible; their relation to surrounding parts making their seizure difficult; and a changed condition of their tunics, which render them too feeble to bear the constriction of the ligature. 3. Nevertheless, in consequence of the size of the vessels, the ligature is the only hæmostatic to be relied upon. The other means are not only faithless, but they may aggravate the condition of the patient, by giving rise to alarming inflammation. 4. The retraction and vicious relations of arteries may usually be met by suitable *débridements* and the mediate ligature: but arterial friability is a much more serious occurrence, calling for a more radical procedure, such as the ligature by Anel's method, which indeed may be employed as a last resource in all cases of difficult hæmostasis, whatever may be its cause. 5. This method has the advantage over the ordinary one of not leaving numerous threads, or a large foreign body in the wound. It exerts no compression on nerves, muscles, or veins, and offers no obstacle to primary union. 6. Anel's method applied to these cases scarcely predisposes to gangrene, as has been feared. The surface of the stump is only moderately inflamed, and its healing takes place with regularity. 7. After amputation of the leg, this distant ligature might be applied to the femoral artery at the ring of the third adductor, or to the popliteal at its upper, middle, or lower third. These four operations would be equally efficacious, but the three first would present some difficulty in execution, and would add considerably to the dangers of the case. 8. The procedure of M. Marchal de Calvi (tying the lower third of the popliteal), as proposed in his thesis in 1837, is easy of execution, and theoretically exposes less than any other to the occurrence of gangrene. The wound necessary for reaching the vessel does not do much injury to the surrounding parts, and becomes confounded with that of the amputation itself. 9. It would be especially easy to tie the vessel in the double flap-operation, a simple vertical detachment of the skin enabling the vessel to be reached. In the circular operation, it would be preferable to making an incision where it would be required in the entire limb, to cut sufficiently high through the parts, uniting the supplementary incision by sutures.

The author adduces one case, in which, on account of friability of the arteries, this operation was performed with success.

V. *On the Duties imposed on Practitioners by Lithotrity.* By M. CIVIALE. (Moniteur des Hôpitaux, 1859, No. 94.)

M. Civiale took the occasion of two favourable lithotrity cases to make the following observations to his class at the Necker Hospital:—

As long as cystotomy constituted the sole surgical resource in a case of stone, the conduct to be followed by the surgeon was distinctly marked out. The rule was, at least as regards the adult and the aged, to delay the operation as long as life continued supportable. It was founded upon the fact that any cutting operation on the bladder, independently of the circumstances under which it may be resorted to, gives rise to real perils, and that under the most favourable conditions, both as regards the size of the stone and the state of the patient, the hopes of the practitioner may be belied. Under these circumstances, a prudent and experienced practitioner, suspecting the existence of stone, pursued a judicious course in not communicating his suspicions to the patient as long as the pains were slight and of short duration, and capable of being rendered very bearable by the use of internal remedies: and a very great number of facts prove—1st. That the stone may remain stationary, and many patients who would have succumbed to an operation performed at an early stage, have lived for a long period without suffering excessively; and 2nd. That the operations performed at a later period, when functional disturbances have rendered them necessary (and always before the condition of the patient has become seriously deteriorated), are not followed by notably more unfavourable results. This rule constituted the basis of all rational practice, and has received the sanction of experience and the consent of the greatest practitioners. No serious arguments can be opposed to it; and some exceptional cases or isolated opinions, founded on an insufficient experience, do not possess this character.

Since lithotrity has become the general method of treating stone, this rule of conduct has undergone a change; for all is different, both as regards the manner of proceeding and the result obtained. The operation succeeds with greater certainty in proportion to the small size of the stone, a few days' treatment then securing the patient an easy and durable cure, unattended by unfortunate consequences. All calculous patients are in these conditions at one part of their malady, and may then rely upon the benefit of treatment. Lithotrity, too, presents the invaluable advantage of saving the patient from the suffering from stone, and especially of preventing the development of the organic lesions of the bladder, which constitute a long series of complications, involving the operator in uncertainties and mistakes.

Looking at these general results, it might naturally be expected that every enlightened and conscientious practitioner would make it a rigorous duty to carefully study the early rational signs of the presence of stone, and have recourse to the new means of exploration which art has furnished for establishing an exact diagnosis. It is much to be regretted that this is not the case, the same line of conduct being now pursued with regard to lithotrity which formerly was properly applicable to lithotomy. Practitioners of high repute may be daily found not making a stand at the early symptoms, and without assuring themselves as to the presence of stone, merely palliating these by the use of sedatives. Such means succeed all the better, inasmuch as the symptoms of stone are often interrupted, especially at an early period: and when these return, the same means are again prescribed, the patient is sent to Vichy, or appropriate regimen is directed—the idea of stone never being raised for fear of alarming the patient or his friends. Every practitioner is aware that, in order to establish the diagnosis of a calculous affection, a direct exploration is essential; but on the patient exhibiting any signs of fear, this is indefinitely adjourned: and thus both patient and practitioner live in ignorance of what really exists, both seeming to fear recognising the true condition of things, and remaining in a state of deceptive calmness and security. In M. Civiale's 'Traité de l'Affection Calculeuse,' he has related a great number of curious facts, each more melancholy than the other, which only too plainly exhibit the deplorable consequences of this mode of procedure, which leads the patient fatally to his end, and involves the practitioner in the most painful errors. In the present paper he adduces additional instances of the mischief accruing from this temporizing practice—the stone in some of these having acquired such a magnitude as to be no longer amenable to lithotrity, while in other cases death ensued upon operative procedures too long delayed. M. Civiale finally observes that it is impossible to relieve a practitioner of the responsibility of events, the occurrence of which he might easily have prevented, had he made or caused to have been made a careful exploration at the period of the first appearance of the symptoms. In some instances the patients have been nearly on the point of bringing this point of medical responsibility before the legal tribunals.

VI. *On Sciatica and Conjunctival Neuralgia.* By M. JOBERT. (L'Union Médicale, 1859, No. 77.)

In a recent clinical lecture, M. Jobert made some interesting observations upon these affections, some of which we reproduce.

Sciatica.—The pain constantly follows the track of the nerve, never proceeding towards its origin. It is always centrifugal, and is sensibly increased on pressure, especially when this is made opposite to the point of the exit of the nerve from the pelvis. The pain is felt over a varied extent, being in some cases limited to the thigh itself, and in others coursing along all the branches of the nerve. Sometimes continuous, in other cases there are very marked intermissions. Certain patients complain of a peculiar sense of heat along the track of the nerve, and occasionally there may be observed a kind of papular, or even a vesicular eruption, but it is very rare to meet with a marked redness along the course of the nerve. In some cases the limb at last presents almost a tetanic condition, and more or less lameness is almost always present, amounting in some cases, owing to the excessive pain, to positive impossibility of walking. When the neuralgia is old, wasting with muscular debility ensues, and as even after a cure this does not always disappear, lameness may become a permanent condition.

The treatment should not always be alike. In some cases antiphlogistics succeed, and in others either simple blisters, or these followed by the use of morphia. When we wish to employ the endermic method, long and very narrow blisters should be employed along the track of the nerve, and in very susceptible persons *datura stramonium* may advantageously replace morphia. When the intermissions are very marked, quinine may be of service. In a large majority of cases a far more energetic and efficacious mode of treatment, which, however, should not be resorted to at first, is *transcurrent cauterization*, performed by irons heated to whiteness, and applied so lightly as only to affect the epidermic surface. The pain caused by it is very slight, and rapidly disappears, and in a week the carbonized portions of epidermis have entirely fallen off. The application should be first made at the most painful part, and then continued along the course of the nerve, cold compresses being afterwards placed over the eschars. Sometimes the pain persists or returns, and then the cauterization must be again resorted to. It is a heroic remedy when the sciatica is not symptomatic, and even then it gives great temporary relief. When the cauterization fails, and the affection continues to inflict terrible torment, we must not give up the case always as hopeless. M. Jobert has in such a case divided the nerve; but the success of the operation has not been such as to induce him to recommend it. In the event of his again meeting with a case in which the dreadful character of the suffering compels the surgeon to do something, he would, after exposing the nerve, act directly upon it by means of the actual cautery. He has practised various experiments upon animals, with the result of proving that the nerve may thus be deeply acted upon without destroying it. The movements of the limb disappear at the same time as the pain at the time of the cauterization, but they entirely return again, which is not the case after division of the nerve. In a case in which he thus cauterized one of the branches of the facial nerve after exposure, a very great amelioration ensued.

Conjunctival Neuralgia.—Ocular and periocular neuralgia are common enough, but M. Jobert calls attention to a form but little known and ill-described, which he terms *conjunctival*, and which was described by Lisfranc under the name of *nervous ophthalmia*. It is very often met with in infants at the breast, and in children from two to five years of age. It is also pretty common in the adult; but is never seen in the aged. It generally affects persons of nervous constitution. It is usually quite suddenly developed, damp, cold, or a current of air often acting as the determining cause. In most cases both eyes are affected. The pain is the predominant symptom, and is insupportable. In persons otherwise quite well, a most remarkable amount of photophobia is observed, the patient seeking complete darkness, and shrieking on the admission of light. There is tenderness on pressure of the eyelids, and these are sometimes infiltrated: but the most important fact is that, with all this, the media of the eye continue quite transparent and the cornea is unaffected, the conjunctiva only presenting a slight rose tint. There is always discharge of tears, but none of the signs of inflammation exist in these cases. The neuralgia may disappear after five, six, or seven days, and sometimes suddenly; but when it is more persistent, it may lead to a real conjunctivitis. The most varied means of treatment have been resorted to, as electricity, electropuncture, opiates, antiphlogistics, purgatives, and antiperiodics. In the patient who formed the subject of the lecture, the disease had shown great persistence; and the occlusion of the eye, by means of charpie dipped in white of egg well beat up, which M. Jobert has known of use in several instances, was here of no avail. He has entirely given up as useless, in these cases, setons, blisters, and the like. The excision of a small portion of the conjunctiva gives relief, but only for a time. In some cases periorbital cauterization has been followed by amelioration. Two important points are to be borne in mind:—1st. The utter inutility of antiphlogistics; and, next, that by far the most efficacious means, and which almost constantly succeeds, especially in the case of young children, is dropping some tincture of opium or henbane, together or separate, several times into the eye daily. In young children the pain is thus relieved with a truly remarkable rapidity.

VII. *On the Local Employment of Chloroform in the Reduction of Dislocations.* By M. Orliac. (Moniteur des Sciences Médicales, 1859, No. 27.)

M. Orliac, a French provincial practitioner, relates two cases of recent dislocation of the shoulder, in which rapid and painless reduction was accomplished. This result he attributes to having surrounded the shoulder with, and placed in the axilla compresses imbibed with ten or twelve grammes of chloroform, these being applied two or three minutes prior to, and during the attempts at reduction. In this way, he observes, assistants may be dispensed with (an important matter in country practice), and pain be prevented, without any danger being incurred.

VIII. *On Syphilitic Tumours of the Tongue.* By M. Lagneau, Jun. (Gazette Hebdomadaire, 1859, Nos. 32, 33, 35.)

In this paper M. Lagneau brings together the particulars of 10 cases, some having been already recorded and others having fallen under his own observation.

Etiology.—The affection has only been met with at an advanced stage of constitutional syphilis, some of the subjects being, nevertheless, otherwise in excellent health. Excepting one instance, all the cases occurred in males.

Pathological Anatomy.—The tumours are sometimes seated deep into the muscular substance of the organ, and at others very superficially; and their development commences most frequently at the base of the organ. The tumour may be single or exist in considerable numbers. Sometimes several isolated indurations in the process of growth become confounded together so as to constitute a single tumour. The size has varied from that of shot to that of a small walnut, and in a case occurring to M. Cloquet the tongue became so enlarged as to descend three inches below the chin. In general the form of the tumour is more or less rounded, and its colour is grey or white, although this is not always the case. Of an almost cartilaginous hardness at first, as the tumour approaches the surface it presents more of a pasty or gummy consistence, and on bursting gives rise to ulceration. As these tumours do not prove fatal, their texture cannot well be judged of, but it is to be supposed that they do not materially differ in this respect from syphilitic tumours developed in the muscles and cellular tissue of other parts of the body. The excavated ulceration which follows the opening of the tumour is of variable depth, according to the position of the latter. It is of a more or less elongated form, with irregularly sharp cut edges, and presents a greyish bottom, covered with pseudo-membranous exudation, and bleeding easily from contact of the teeth. Induration at first surrounds its base, but this gradually somewhat diminishes. When several of these ulcers become joined together, a considerable portion of the substance of the tongue is destroyed, and a serious deformity remains even after cicatrization. When, however, the syphilitic tumours become arrested by suitable treatment while still in the condition of nodosities, they gradually lose their consistency, and leave behind them no signs of their former existence.

Symptoms.—Many of these have been already alluded to in detailing the appearances furnished by the tumours; of course there is difficulty of speech and deglutition proportionate to the size of the tumours. The pain is usually but slight, or even does not exist, especially in the early stage. Although the occurrence is quite exceptional, the submaxillary glands sometimes undergo enlargement on the breaking out of ulceration. When there is great tumefaction and projection of the tongue, salivation is one of the consequences. The evolution of this affection of the tongue is eminently chronic, its commencement being often referred to a period many months distant.

Diagnosis.—This is of the highest importance, for the affection has often been confounded with other lesions. Among these, *cancer* is pre-eminent. Several patients, cured of frightful-looking syphilitic ulcers, have been regarded as instances of recovery from cancer of the organ, while the tongue has been amputated for reputed cancerous ulcerations which would have yielded to iodide of potassium. In distinguishing between the two affections we are greatly aided by the fact of the pre-existence or co-existence of other syphilitic symptoms, a fact which prevailed in nine out of the ten cases here given. Cancer of the tongue is attended with lancinating pain, and its preferential seat is not the base but the point or edges of the tongue. It is usually single, while syphilitic tumours are mostly multiple. Cancer usually first shows itself as a hard, circumscribed, warty tumour, which is not the case with the syphilitic tumour, which assumes a more regularly roundish form. Consecutive glandular swellings are rare in the syphilitic affection; but cancer, when somewhat advanced, implicates the surrounding glandular tissue. The tendency of the syphilitic tumour is towards the surface, where it softens and ulcerates, while cancer involves the deep-seated as well as the superficial tissues. The ulceration from cancer is generally single and more or less fungous, while the ulcers following syphilitic tumours are generally multiple and excavated, with sharp, irregularly cut edges, and a greyish, diphtheritic, partially gangrenous bottom. The early induration of their base gradually disappears as the plastic matter becomes softened and excreted at the surface of the ulcer. Cancerous engorgement is persistent, for in proportion as the morbid product is destroyed by ulceration, the disease spreads to deeper and deeper parts. Both forms of ulcer bleed from contact with the teeth; but the syphilitic is less fungous and less vascular than the cancerous. (2.) *Tubercle* going on to ulceration is not very uncommon in the tongue in phthisical or scrofulous subjects; and in such cases error may arise without much difficulty. These steatomatous tumours have usually less consistence than have the syphilitic. (3.) *Callosities, hypertrophy of the epithelium, tylosis linguæ.* These terms indicate a condition described by MM. Ullmann and Buzenet as occurring on the tongue of inveterate smokers. The mucous membrane becomes adherent, and beneath it is deposited a plastic exudation, presenting a firm, whitish, insensible surface. This layer of plastic deposit and epithelial cells cracks and becomes detached in fragments, bringing into view a painful, irregular ulceration of greyish aspect. But, although this appearance somewhat resembles that caused by those syphilitic tumours which induce a whitish projection at the surface of the tougue, these latter are not found at the surface, but in the substance of the tongue. The ulcers which follow the detachment of these plastic deposits do not at all resemble the deep excavations which follow the syphilitic tumours. (4.) *Hypertrophy* of the tongue, and (5) *glossitis* are easily distinguished from syphilitic tumour. (6.) *Primary chancre of the tongue* is usually easily distinguishable by the history of the case.

Prognosis.—This, in the case of suitable treatment being had recourse to, is usually favourable; but if this be too long delayed, the destructive ulceration which ensues will be followed by more or less permanent deformity.

Treatment.—Various forms of mercurial preparations have been used by different authors with success; and although the author believes that iodine is useful in some cases in which mercury has been already employed in vain, he does not agree with M. Ricord in the propriety of proscribing the latter, or even, as a general rule, in substituting iodide of potassium for it.

IX. *On the Stricture of Herniated Intestine.* By Professor PALASCIANO. (Presse Médicale Belge, 1859. No. 14.)

M. Palasciano, Professor of Surgery at Naples, concludes a memoir upon this subject in the following terms:—1. The great frequency of stricture of a herniated intestine has been proved by numerous and authentic facts, and well deserves the attention of the surgeon. 2. It is possible that in a great number of cases nature alone, without any intervention of art, effects the dilatation of the stricture; but, nevertheless, it is of daily occurrence that these strictures, left to themselves, give rise to the death of the patient. 3. The stricture may be brought about by the same description of causes, and the mechanism which ordinarily give rise to strangulation, and without doubt the aponeurotic rings play a great part in its production. 4. Small, irreducible hernias, or hernias which are reduced and badly kept up, are those which are most disposed to this occurrence. 5. In hernias of this kind the mild chronic form of strangulation, unaccompanied by notable nervous accidents, should induce the careful examination of the seat of constriction of the strangulated hernia during the operation: for it is there, in all probability, that the stricture exists. 6. The production of the stricture depends primarily upon the forcible approximation of the walls of the intestine, with compression and sometimes rupture of the internal coats. Intra-intestinal plastic exudation and ulceration of the internal fissures are occurrences which take place subsequently to the mechanical action, and often persist after simple reduction. 7. The stricture may vary from the slightest amount to complete obliteration of the canal; and, as organic modifications follow, it is not easy to determine by the simple view of a stricture to what degree the consecutive obliteration may extend. 8. We are not in possession of any precise fact demonstrating how far the powers of unassisted nature are capable of re-establishing the natural calibre of the intestine after reduction. 9. Simple reduction, adopted in all cases of stricture which have not reached total obliteration, is a fatal procedure, to which are due all the deaths produced by the effects of the stricture in strangulated hernia operated upon in opportune time. 10. The rule of dilating by invagination every intestinal stricture during the operation for hernia is the sole practice capable of assuring success under these circumstances. It is always easy of execution, and is itself attended by no serious danger. 11. Even in the case of total obliteration, it should be attempted; for the organization of the plastic matter, which alone could enable the obliteration to resist mechanical force, has not had time to become complete. 12. When even this artificial dilatation proves impracticable, an important diagnostic criterium will have been obtained, establishing the necessity of other proceedings. 13. The excision of the whole of the strictured part of the intestine is the indispensable operation which is to be preferred to all others, whenever the stricture will not admit of artificial dilatation. 14. Its excision should be immediately followed by the suture; but if there exists any doubt as to immediate union, in place of a suture, an artificial anus should be established, by fixing the two ends of the divided intestine externally to the neck of the sac.

The dilatation of the strictured portion is directed to be thus performed:—After the operation for hernia has been performed, the intestine is to be gently drawn out, and if a permanent fibrous stricture is observed, the portion of the intestine extracted is to be suspended with one hand, while with the small or index finger of the other, the upper part of the intestine is forced into the stricture. The dilatation is then to be effected by the distension of the intestine by means of the fingers of the one hand placed on the other which is invaginated, or by the separation of the two fingers that have been introduced.

X. *On the Incautious Employment of the Ophthalmoscope.* By M. Desmarres. (Gazette des Hôpitaux, 1859, No. 67.)

M. Desmarres in a recent lecture observed, that with the recent great extension of the employment of the ophthalmoscope, and the employment of instruments possessed of so much stronger a reflecting power than those originally devised, there may be sometimes danger of inducing a mischievous congestion of the eye, which may arouse disease which had continued stationary, or may cause, in the case of disease successively invading the two eyes, its premature development in the second. Of this latter occurrence he gives an instance in which the patient suffering from confirmed glaucoma on the right side, had the left eye submitted to repeated ophthalmoscopic examination on one occasion, which, however, gave rise to no pain or uneasiness. Nevertheless, the incipient glaucoma, which was there detected, and which, according to the general rule, might have taken months or years to arrive at its acme, immediately took on a very acute form, so as in two days to necessitate the performance of iridectomy.

The precautions which should be taken to avoid similar accidents are simple. The eye should be sufficiently illuminated to see, but not more than is necessary, and the illumination should vary in intensity. The examination should be often suspended, so as to give the patient rest. In the great majority of cases, M. Desmarres only employs, as the source of light, a wax candle held on the shoulder by the patient himself; and by placing the eye in the focus of the mirror or a little beyond, the illumination is varied. Except under very special circumstances, the pupil should not be dilated by belladonna, for not only is the eye too much illumined, but the patient attributes the artificial mydriosis not to the belladonna but to the instrument, and becomes averse to a repetition of the examination. The hand-ophthalmoscopes answer their purpose best, the fixed ones requiring too much light, too prolonged an exposure to it, and a constrained position of the patient. They are, moreover, complicated and expensive. The hand-ophthalmoscopes often require long and troublesome practice in order to acquire their dexterous management. Such management should be well acquired by repeated practice before any attempt is made to examine the eye of a patient.

XI. *Simultaneous Dislocation of both Ends of the Clavicle.* By M. Morel-Lavallée. (Gazette des Hôpitaux, 1859, No. 33.)

In this communication, addressed to the Paris Surgical Society, M. Morel-Lavallée observes that the circumstances must be very exceptional to admit of a bone losing all its articular relations. The astragalus is the bone which has furnished most examples, and some of the other short bones are susceptible of analogous displacements. The ileum is the only flat bone in which the lesion has been found, an instance of this having been recorded by the author in his Thesis on 'Compound Dislocations.' With the exception of the lower jaw, the lesion is very rarely met with among the long bones. Boyer met with it once in the fibula; but the author has only been able to find two examples of its occurring in the ulna, and one in the humerus. Of the double luxation of the clavicle only one other example besides the present has been recorded, and that very imperfectly.

The author's patient was forty years of age, and the accident occurred through his shoulder becoming compressed between the angle of a pile of wood and the advancing wheel of a carriage. On admission next day to the hospital, his attitude most markedly exhibited injury to the clavicle. On examination the supra and subclavicular hollows were found entirely effaced, while a tumour, the size of a walnut, just below the sternal depression, denoted a dislocation of the sternal extremity of the clavicle forwards. Passing the finger along the bone, the external extremity was found exactly in the middle of the space which separates the shoulder from the side of the neck, the bone seeming to have assumed an antero-posterior position, perpendicular to that which is natural to it. The extremity projected so distinctly under the integuments that its form could be easily traced, while the clavicular projection on the acromion was quite lost. The portion of the trapezius inserted into the clavicle displaced posteriorly, constituted in its relaxed state a softish tumour the size of half an orange. The distance from the acromion to the sternum seemed diminished, but was found to measure nineteen centimetres on both sides. The internal dislocation was found to be reducible and maintainable by bandages; but no effort that was employed exerted any effects upon the external extremity.

During the discussion on the case, M. Chassagnac described the procedure he adopted in a case of complete dislocation of the external extremity of the clavicle. The operator pressing the sound shoulder against his chest, passes one arm in front and one arm behind the patient's chest, crossing his hand below the elbow. In this way so decided an elevation of the point of the shoulder is produced that the extremity of the luxated clavicle resumes its normal relations. M. Morel-Lavallée explained that this manœuvre failed here because the clavicle being not retained at the luxated sternal extremity, follows the outward movement of the shoulder, and still retains its abnormal relation to the acromion.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D., F.R.C.P.
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I. Physiology and Pathology of the Unimpregnated State.

1. *On the Uterus and its Inflexions.* By Rokitansky. (Allg. Wiener Med. Zeitung, No. 17, 1859.)
2. *On the Origin of Uterine Flexions.* By Virchow. *Opere citato.*
3. *Spontaneous Elimination of an Uterine Fibroid the Size of a Child's Head, with Termination in Recovery.* By Hugo Ziemssen. (Virchow's Archiv, 1859.)
4. *A Polypus-shaped Desmoid of nearly Three Pounds' Weight extirpated by Breaking-up.* By Dr. Gustav Simon. (Monatsschr. für Geburtsk., July, 1859.)

5. *Spontaneous Emptying of an Ovarian Cyst into the Bladder.* By ULLRICH. (Monatsschr. f. Geburtsk. 13 Bd. 3 Heft.)

6. *On Ovariotomy.* By Mr. SPENCER WELLS. (Dublin Quarterly Journal, Nov. 1859.)

1. Rokitansky communicates some interesting observations on the flexions of the uterus. He says, the vaginal portion of the uterus of a multipara and the connected vaginal roof is formed out of a duplicature, or folding-in of the vagina, in which the lower end of the uterus takes a part. So soon as the uterus has passed into the vagina, the vagina surrounds it like a ring, the appearance being like an intussusception. In front the doubling is shorter, and is attached by loose cellular tissue. On this account the anterior lip of the vaginal portion is thicker, the vaginal roof is more shallow, and the anterior lip is, besides, deeper. After repeated labours, the sharpness of these distinctions is lost. On section of the uteri of young persons, it is seen that the vagina, after it has formed the duplicature constituting the roof, is continued into the uterus. The mucous membrane of the vagina grows into the brawny mucous membrane of the cervix, the muscular layer follows it, and runs over the cervix into the fleshy mass of the uterus. A second, outer, loose, muscular longitudinal fibrous layer of the vagina goes, on the contrary, not into the duplicature, but passes over it outside, and spreads above over the mass of the body of the uterus. In more mature uteri, and in those which have been pregnant, there is interposed between the brawny mucous membrane of the cervix and that longitudinal muscular layer a richer mass of uterus, which, by so much the more that the duplicature is unfolded, ends more plainly in a point in the anterior lip of the vaginal portion. The shortening of the longitudinal muscular layer strengthens the inversion of the uterus in the vagina. At the anterior side of the uterus runs the round ligament, separating into two muscular bands; the upper run together at the fundus, the lower run together under an angle in the neighbourhood of the os uteri internum; so that, from these four bands on the forepart of the body of the uterus, a kind of lozenge-shaped space is marked out. At the seat of union of the lower bands there strikes, in uteri of this description, a band about an inch broad in the form of a bow, the pillars of which run down close by the edge of the cervix to the vagina, and spread out on it. These fasciculi fix and especially enlarge the duplicatures. On the posterior wall of the uterus the ascending band is simply continued over the cervix into the vagina; or there proceed, also, from the end of this band, in the neighbourhood of the os uteri internum, two strips of the form of a sharp bow to the vagina. The stiff, thick, strong mucous membrane of the cervix, and the remarkably thicker connective tissue on the posterior wall, does not terminate at the os uteri internum, but continues, becoming thinner, on to the body of the uterus. This stiff stratum forms the support of the mass of the body of the uterus, and the foundation of its normal upright position, and shares essentially in flexions.

Inflexion of the uterus forwards or backwards always falls in the region of the os uteri internum, where the stiff connective-tissue mass is renovated, and appears as the submucous connective tissue of the mucous membrane of the uterus. Flexions of the cervix but seldom happen. The stratum of connective tissue is always found less thick, looser, thinner, and even wasted away. For this reason anteflexion is far more frequent and in lesser degrees, or it grows to infraction; retroflexion is less frequent, but oftener in extreme degree, and very seldom grows to infraction. Anteflexion, moreover, most commonly appears in the virgin uterus, or at least it is apparently in no relation with labour; retroflexion, on the contrary, hardly even arises but after repeated labours (or abortions), and is in essential relation to this state.

2. Virchow sums up his researches on the flexions of the uterus as follows: He believes that anteflexions are more a mechanical than an organic phenomenon. 1. At the seat of infraction there is found no primary alteration of tissue. 2. Simple relations of pressure, distinguished from actual tumours, cause no anteflexions, but mostly retroflexions. 3. The fillings of the bladder and rectum cause, as one can be convinced by experiment on the dead body, distinct changes of position of the uterus. 3. These changes of position are, in like manner, no longer possible when the body of the uterus, and especially its fundus, is fixed in a certain height and on a certain level; still more, one may be satisfied, by artificial filling of the bladder, that, in proportion to the distension of its walls, a stronger bowing, and even a true bending takes place. 5. In original shortening of a lateral ligament there is found in childhood only lateral dislocation and lateral inflexion; in persons beyond puberty, anteflexions. 6. Anteflexions are more frequent in normal, retroflections in pathological conditions of the uterine walls.

From the foregoing considerations, Virchow draws his therapeutical deductions:—1. In the history of flexions there is a period of simple predisposition, one of simple flexion, and one of flexion complicated with various inflammatory processes: 2. The predisposition is frequently given by partial forms of peritonitis, which appear with colicky attacks, and is apparently very difficult to mitigate: 3. Long retention of the urine and of fæces favours the formation of flexions, especially at the time of menstruation, of childbearing, &c., and is therefore carefully to be avoided: 4. Enlargements of the uterus, especially when united with relaxation, quickly cause flexions, and the removal of these enlargements, *e.g.*, in chronic endometritis, may materially alleviate them; hence the antiphlogistic treatment of uterine catarrh, and most careful watching of the menstrual and puerperal processes are necessary: 5. A complete removal of anteflexion seems, in the highest degree, doubtful, whilst, in retroflexion, it may be expected: 6. When flexion is connected with consecutive processes, as endo- and peri-metritis, a careful local treatment is necessary. Endometritis may by this means be removed; but perimetritis causes adhesions of the uterus, which the longer they endure the more they fix the flexion and hinder the improvement of position of the organ.

Virchow follows up his own researches on flexions of the uterus by some remarks on the views of Rokitansky, related above. The atrophy at the seat of infraction is, according to Virchow, not primary or essential, for in anteflexions of infantile and maiden subjects there is not the slightest alteration of the uterine wall to be found. First, menstruation, childbed, abortion, catarrh, commonly bring the changes. Of all things, flexions favour the conditions of atrophy and atomy; but in this respect further researches are wanted.

Farther,—the anatomical description of the uterus, as Rokitansky gives it, is not in all points correct; for example, the mucous membrane of the cervical canal cannot be called callous, for it is here relatively thin; although it is still thinner in the body, it is in the cervix very rich in cells, and more resembling a granulation-tissue than callus. The fibro-muscular tissue of the uterus is found as well in the body as in the neck: it contains more muscular fibres and vessels in the body, and more fibrous connective tissue in the neck. Towards the mucous membrane the muscular fibres in both places cease, and there is found a distinct, apparently thick, but in the normal state by no means thick, submucous layer.

3. Dr. Ziemssen's case of spontaneous elimination of an uterine fibroid tumour is of great interest in reference to the question of the removal of uterine tumours by operation.

A woman forty years old had borne one child, and had menstruated regularly. She observed a swelling in the abdomen in 1855, which gradually increased.

In the autumn of 1858, when examined by Dr. Ziemssen, the tumour was of the size of a child's head, very resisting, and resembling a gravid uterus of six months; it was very slightly moveable; it did not distress either bladder or bowel. The brim of the pelvis was quite filled by the tumour; the vaginal portion of the uterus was directed backwards, and so high that it was reached with difficulty; the os was closed. The patient had suffered neither hæmorrhage nor pain; she was in good health. But in December hæmorrhage from the uterus took place, with labour-like pains. This recurred in January, 1859, with more violence; strong pains, great tenderness of the abdomen on pressure, a febrile condition, and presently great weakness followed. On the 12th of February, hæmorrhage, followed by great anæmia and faintings, and attended by great pains, occurred. There was also irritative fever. In the beginning of March the discharge became offensive and discoloured, and in a few days this so increased as to empoison the room. One day the tumour seemed suddenly to have become softer. On the next morning several fatty, foul pieces of tissue, from the size of a walnut to that of an apple, were expelled by strong uterine contractions. Similar pieces were expelled in the following days. On section they appeared shining and reddish, resembling muscular tissue. Professor Grohé, who examined the structure microscopically, found it to consist of connective tissue, and smooth muscular fibres, both in a state of commencing decomposition. During this process the swelling in the abdomen lost considerably in bulk and firmness. The last piece was expelled on the 27th of March. On the 30th of April the patient had returned to work. The uterus remained somewhat above the normal size, but was of normal form. Menstruation returned, and a chronic uterine catarrh seemed the only remains of the recent affection of the womb.

4. Dr. Gustav Simon's case of desmoid polypus is interesting, both on account of the structure of the growth and the operation for its removal. A lady, aged fifty-one, had been known to carry a polypoid growth for four years, but had refused operative measures. The pelvis seemed almost filled by the tumour; and six months before Dr. Simon's treatment an attempt had been made to seize the tumour with the obstetric forceps, in order to bring it within reach of other instruments. This attempt was abandoned. It seemed to have had the effect, by compression, of lessening the bulk of the tumour, and the voiding the bladder and rectum became easier for a time. The patient was much emaciated, and confined to bed. The tumour quite filled the pelvis; no os uteri could be found. By palpation between the vagina and the abdomen an immense tumour was made out, having its apex at the vulva and basis at the umbilicus. Near the umbilicus was a smaller tumour the size of a hen's egg, which was made out satisfactorily to be the uterus. It being considered impossible to bring down the huge tumour with the obstetric forceps or with Museux's instrument, the lower part of it was held by the latter instrument, and funnel-shaped pieces were cut out, so that its circumference was lessened; then the obstetric forceps was applied, and by compression with this the polypus was drawn partly out of the vagina. On account of the weakness of the patient this was done without chloroform. After two hours' painful operation, four large pieces were cut out of the apex of the tumour by the scissors, with inconsiderable bleeding. But soon the hæmorrhage increased so as to bring on syncope. The hand was forced up to the vagina, and a stalk being felt, the tumour was dragged down by the hand so as to bring the stalk externally. This was transfixed by a needle carrying a double thread, tied on both sides, and cut through. The patient had recovered in twelve days. The following is a summary of the description of the tumour:—The investing substance was of uterine tissue, which had been distended by the growth within it of desmoid balls projecting out of the cervical cavity, and becoming enormously

hypertrophied. These desmoid structures were of all sizes, from that of a pin's head to that of a child's head. On the surface of the mucous membrane covering the tumours, especially on the part where the broad basis was, were numerous vesicles filled with grey transparent mucous contents, and between these were equally numerous funnel-shaped openings, of various sizes, leading into roundish, smooth-walled empty sacs. These open sacs and closed vesicles, which lay in the deepest strata of the mucous membrane, and not seldom communicated, were nothing else than the enlarged, perhaps multiplied, mucous crypts, and the enlarged, partly burst and partly unburst, ovula Nabothi of the normal cervical mucous membrane. The second constitutive part of the tumour consisted in desmoid balls of various sizes. They were mostly round, of very thick concentrically-disposed fibrous layers, strongly resisting section by the knife. They were surrounded by the investing uterine substance, and loosely united by connective tissue.

5. Dr. Ullrich's case of ovarian cyst. A woman, aged thirty-five, had suffered eleven years before from inflammation of the abdomen, which was followed by great distension, and occasionally pains, with difficulty in passing stools. In November, 1857, the patient was attacked with irritative fever from abdominal inflammation. At this time several tumours larger than a child's head were observed. After several weeks the pains were suddenly much increased, and a great quantity of thick fatty matter, which was pure elain, was discharged by the bladder with much relief. The discharges went on for several days, so that the entire quantity amounted to several quarts, the swelling of the belly diminishing meanwhile. During the next four weeks the urine contained only drops of fat, but always a copious purulent sediment; then an exacerbation of pain occurred, and a pint of decomposed pus was voided by the bladder. From this time the patient improved, but the urine contained from time to time a purulent sediment and fat-drops.

It is concluded that here an ovarian disease was cured by a communication with the bladder, since only the fat glands in a desmoid cyst of the ovary could produce so copious a fat-formation.

Mr. Wells relates eight cases of ovariotomy performed by him, all since the commencement of the year 1858. Two of the patients died; one thirty-two hours, one forty hours after the operation. A third patient, who had so far recovered as to have returned to her home in the country, becoming "robust and strong," died ten months after the operation. In the two first cases, peritonitis was found; but Mr. Wells is disposed to think that the more active cause of death was the shock that seems to be a peculiar attendant upon operations upon the uterus and ovaries. In the last case, death was due to the progress of abdominal cancer. The relation of the cases is followed by very practical observations on the conditions indicating operations, and the modes of procedure. All we can do here is to give a brief reference to the paper.

II. Pregnancy.

A Case of the Death of the Fœtus in the Seventh Month, in consequence of over-intoxication of the Mother by Carbonic Oxide Gas. By Dr. M. B. Framb. (Monatsschr. für Geburtsk., July, 1859.)

A strong woman, aged twenty-two, primipara, was delivered of a dead putrid child in the Lying-in Hospital of Breslau. The mother believed she was carrying a dead child for fifteen days. She narrated that one day the chamber which she occupied with a child and another woman became filled with fumes of burning charcoal through a stoppage of the chimney; but she did not feel compelled to leave the room or to ventilate. After a few hours, the child had vomiting and diarrhœa, and both women complained of headache. Towards evening the pregnant woman had a long shivering fit, and vomited. From this time she felt no more of the fœtal movements; she felt as if carrying a foreign body; her breasts shrank; the shivering returned daily. After lying-in, she had perimetritis, but recovered.

III. Labour.

1. *Case of Cæsarean Section in which both Mother and Child were saved.* By W. F. McClelland, M.D. (North Amer. Med.-Chir. Rev., July, 1859.)
2. *A Cæsarean Section with Unsuccessful Result.* By Dr. Gustav Simon. (Monatsschr. für Geburtsk., July, 1859.)

1. Dr. McClelland was called to a woman, aged thirty, in her second labour. There was a cicatrix of the vagina obstructing the access of the finger to the os uteri. The patient had been delivered the first time by instruments. The cicatrix was divided by a bistoury, when the os was felt expanding; but the brim very much contracted, the antero-posterior diameter being estimated at one inch five-eighths, and the transverse one inch and a half. (?) The head could not enter, and the crotchet was deemed useless. The Cæsarean section was performed under chloroform. The child was born alive and lived. On removal of the placenta, a tolerably profuse hæmorrhage took place; but by the application of sponges wrung out of cold water, the uterus was soon made to contract, and all bleeding ceased. The external womb was brought together by interrupted sutures and adhesive straps. A full anodyne was then given. On the third day there was considerable fever; pulse 106; tenderness and tympanitis of abdomen. She had twenty grains of calomel with twelve grains of Dover's powder, and fomentations. The symptoms soon subsided; and in five weeks the patient had fairly recovered.

2. A woman, aged twenty-four, of small stature, was in labour on the 14th of January, 1858. After thirty hours' labour, the head of the living child was still at brim. At the end of forty-eight hours, a consultation was held; the patient was somewhat exhausted; pulse 110. The conjugate diameter measured from two inches and a quarter to two and a half. Pains had ceased. The sounds of the fœtal heart were no longer to be heard. The question lay between gastrotomy and embryotomy; although the probability was against the child's being alive, fearing every moment a rupture of the uterus, the Cæsarean section was resolved upon. An incision five inches long was made in the uterus; blood instantly streamed out in all directions; the upper part of the insertion of the placenta had been wounded. A dead child and the placenta were immediately extracted. The upper part of the uterus contracted well; but the lower part, against which the child's head had lain so many hours, did not contract, and blood continued to flow freely from this open part. It was some time before this could be stopped. The operation was done under chloroform. The patient died, apparently of exhaustion, forty-eight hours after the operation.

Dr. Simon takes occasion from this case to suggest that the incision in the uterus should be made not in the lower part of the uterus, which has become affected by the pressure of the child's head, but as much as possible towards the upper part. [May we take the liberty of suggesting that in all probability the mother's life might have been saved had the Cæsarean section not been performed at all; but the head lessened and contracted by crotchet, craniotomy-forceps, or turning? There certainly seems a wide discrepancy between the indications for Cæsarean section in this country and in Germany.—Reporter.]

IV. The Puerperal State.

1. *A Contribution to the Microscopic Investigation of the Blood in Puerperal Fevers.* By D. Schulten. (Virchow's Archiv, xiv. 5, 6.)
2. *The Pathological Anatomy of Puerperal Fever.* By Buhl. (Monatsschr. f. Geburtsk., July, 1859.)

1. Dr. Schulten has undertaken the difficult but useful task of tracing the influence of the poison of puerperal fever on the blood. The following is the case that gave rise to his researches. A strong woman, twenty-four years old, was taken thirty-six hours after her second labour with a violent shivering, followed by heat and profuse sweating. On the fourth day, renewed shivering, heat and sweat, after which restlessness and delirium; face red, eyes glistening, speech quick. Pulse 140-145; abdomen painless on pressure, but somewhat distended. Lungs and heart free; no enlargement of liver or spleen; great thirst, swimming in the head, anxiety. She had several doses of calomel, nitrate of soda, and was cupped. The symptoms became aggravated. She had quinine; the pulse fell to 95-100; the shiverings ceased. Under the use of quinine recovery progressed.

The first examination of the blood was made three hours after being drawn. The cupping was performed four hours after the second shivering-fit, before the use of any remedy. Before the serum had completely separated, a drop of blood was put under the microscope. The serum was a little turbid. The blood-globules were scanty. On the other hand, the whole field was covered with those little ball-like, yellow-coloured corpuscles which are but seldom seen in healthy blood. Without forming *rouleaux*, they lay thickly together; between these imbedded were white corpuscles, in great number and of various sizes; but the smallest were scarcely half the size of the blood-corpuscles, whilst the largest were twice or three times the size.

Two days later, when the impetus of the fever had already remitted, a second blood-test was made. The serum was quite clear; the blood-globules were of normal size, and formed sharp-outlined rouleaux.

White corpuscles were still frequent in bundles of 5-8; but their size was no longer various. Of brood-cells no more were to be seen. On the other hand, there appeared in the inside of most of the corpuscles small fatty vesicles, reflecting the reddish colour of the surrounding fluid. Single white corpuscles appeared to have passed into fatty substance.

Three days later another sample was examined. A drop of serum showed a crowd of fat-globules of every size. White globules were not in greater abundance than in healthy blood. The blood-globules were normal, but had a tendency to shrivelling. After fourteen days, another examination of the blood and milk was made, to determine whether the child might be suckled without danger. Still there appeared single fat-globules, but nothing else abnormal could be found. Even the tendency in the blood-globules to shrivelling had ceased.

Analysing these observations, we discover two interesting appearances. First, the great increase of white corpuscles in the blood of a person before in health, in whom there was no indication of leukæmia. These could not be regarded as pus-globules. We are driven to conclude that, in this case, there was a special *disease of the normal white corpuscles of the blood*. Secondly, the appearance of fat-globules in the later tests, which, as they were wanting in the first examination, exhibited only traces in the interior of the white corpuscles in the second examination, and presented in abundance in the third examination, contemporaneously with disappearance of the white corpuscles, was probably the result of a change of the white corpuscles in the course of the disease. Schulten has recognised similar appearances in three out of four other cases in which he has repeated his examinations.

2. The practice of hospital lying-in, so prevalent abroad, supplies an experience of childbed-fever on a scale which is fortunately impossible in this country. Dr. Buhl has the unhappy privilege of relating the autopsies of fifty patients who died in the Munich Hospital in the three years 1854 to beginning of 1858. He says there is one constant and characteristic appearance—a pulpy, dirty red or black-brown mass, which here and there has a mildewy, and here and there a putrid smell. This condition leaves no doubt that in it is to be found the starting-point of the development of puerperal fever. It is an infection-disease, and the infecting poison lies in the inner wall of the uterus. On the cause of this putridity or decomposition we are not clear. Whether it be the immediate conveyance of a poison into the womb, or whether it be the preceding empoisonment of the blood by miasmata, which produces a secondary decomposition in the womb. In preventive or curative therapeia the distinction is important. There are two principal forms of the disease—1, puerperal pyæmia; 2, puerperal peritonitis. These forms are clinically distinct in prognosis and in treatment. The usual terms, oophoritis, uterine croup, uterine dysentery, uterine putrescence, metrophlebitis, lymphangioitis, phlegmasia alba, and so on, may all be classed under the two heads named.

In the cases observed, *puerperal pyæmia* killed usually not before the ninth day, and sometimes even not under three weeks. It appeared most frequently when there was no epidemic—or, at least, but a slight epidemic; and in eighteen of the fifty cases, the path of infection was the veins. The pyæmic form is characterized by not sequestrated, purulent plugs in the veins of the placental seat of the uterine walls, in one of the pampiniform plexuses, or in one of the spermatic veins. We never found both spermatic veins plugged; and only once was the inferior vena cava filled with adherent coagula. In two cases (lasting three and six weeks), the pus in the vein-plexus was cheesy. The so-called metastatic deposits were found fresh in the lungs in one case, cheesy in one case. The pleuræ mostly exhibited ecchymoses. Three times purulent exudation was in the pleural cavity without pyæmic infarction of the lungs. The kidneys in one case showed purulent deposits; and twice fructiform ecchymoses. In two cases there was hypopyon; three times pus in joints; and twice phlegmasia alba.

Puerperal peritonitis was more frequent, more violent, and killed more rapidly than pyæmia; out of 32 cases, only two ended fatally, after six and eight weeks. There was always purulent exudation. In 18 cases, pus was found in the Fallopian tubes of one or both sides; 14 times there was sub-serous pus in the uterus, especially in the vicinity of the neck; also in the lumbar-glands. Disease of the veins has no relation to peritonitis. Peritonitis may be etiologically and anatomically discriminated—1. In cases in which through the direct passage of the poisonous material out of the womb through the tubes, peritonitis is set up; and 2. In those cases in which, through the reception of the poison from the inner surface of the womb into the lymphatics, the peritonitis has been excited. Peritonitis through tubal-pus is much the more ready, purely and primitively inflammatory form; the other, on the contrary, is the more severe, and occurs chiefly during epidemic diffusion. The principal changes observed were—Œdema of the ovaries; the spleen (ten times) enlarged; liver always pale; the kidneys pale; the peritoneal exudation mostly in small quantity; the intestinal walls mostly œdematous, their canal filled with gases and watery contents; œdema of the lungs and hypostatic blood-filling; twice fibrine on the pleura; one, hydrothorax; often pleural ecchymoses; three times capillary bronchitis.

Pyæmia and peritonitis had the following properties in common:—1. An almost constant slight swelling and watery infiltration of the retro-peritoneal, inguinal, and mesenteric glands. 2. Osteophites in the inner table of the skull. 3. In most cases, especially in those of pyæmia and lymphatic resorption, a swelling of the capsules of the kidneys, and an acute stage of Bright's disease.

V. Physiology and Pathology of Infants at time of Labour.

Some Observations on Dead-born Children. By Dr. August Breisky, Assistant in the Lying-in Clinic at Prague. (Vierteljahrssch., 1859.)

Dr. Breisky's paper on dead-born children is a valuable contribution to a subject which, on many grounds, demands extended and careful investigations. We will give first a condensation of his observations, and then a summary of his conclusions.

Case I.—*Prolapsus of the Funis: Aspiration of Liquor Amnii.*—A woman with a well-formed pelvis was in labour with her second child. Head in first position; a small knot of funis prolapsed within the thin membranes. In endeavouring between pains to replace cord, pulsation was felt. The indication was taken to be that the membranes should be ruptured to afford a better opportunity of replacing the funis and rescue the child from the threatening danger of interruption of the placental circulation. The rupture of the membranes let out a great quantity of turbid fluid, much discoloured with meconium. The funis was carried down in a long loop; it was pulseless. The pains were strong, and a dead boy was born. [We regret that we are unable from the author's history to fix the lapse of time from the cessation of pulse in the cord to birth.—Rep.] The *dissection* showed—Paleness of the skin, with cyanosis of the face and extremities; bloodvessels and heart cavities full of dark fluid blood; atelectasis of the lungs; the lower part of the trachea, the bronchi and their branches, as far as they could be followed, were filled with fine mucous, yellowish-brown contents, which under microscope was determined to be a mixture of mucus, meconium, and liquor amnii.

Case II.—*Meconium in the air-passages.*—A woman with a roomy pelvis in labour at term. Head presenting; *funis not prolapsed.* At 3·30 a.m. liquor amnii discoloured with meconium escaped, and quickly thereafter a fully-developed girl was born. *Dissection* showed—Paleness and light cyanosis of the skin; paleness and relaxation of the muscles; formation of skull normal; the kidneys without trace of uric-acid infarction; much dark blood in the large veins; both lungs atelectasic; the mucous membrane of nose, mouth, and laryngo-tracheal canal covered with a thin yellow layer; the bronchi, down to the smallest ramifications, filled with a yellow mucous fluid, determined under microscope to be meconium mingled with liquor amnii.

Case III.—*Meconium in air-passages; slight dropsical effusion in serous cavities; inter-meningeal extravasation; laceration of the left tentorium cerebelli.*—The dead child of a primipara, born after twenty hours' labour. Head lay in first position; the fœtal heart had been heard during labour; green-coloured and somewhat offensive liquor amnii discharged; funis came down. *Dissection:* An unusually large child; skin pale, and slight cyanosis of face and extremities; the end of the funis discoloured yellow; between the skull and pericranium were small extravasations; the sinuses were uninjured in their walls, and gave vent to dark fluid blood; the tentorium cerebelli on the left side was slightly torn; there were small fresh ecchymoses near; brain and meninges full of blood, and infiltrated with watery fluid; the large veins of the neck, thorax, and abdomen filled with fluid dark-red blood; the heart cavities also contained dark blood; in the pericardium, pleuræ, and in peritoneum, was some clear bright yellow serum; the lungs contained a few small lobuli filled with air, but the great mass was in a state of complete atelectasis; a small lump of meconium was found in the trachea, and in the minute bronchi was a slimy mixture of liquor amnii and meconium; no trace of uric-acid infarction.

CASE IV.—*Narrow pelvis; large child; natural delivery after thirteen hours; rent of the left tentorium cerebelli and of the sinus transversalis; intermeningeal hæmorrhage; meconium in the air-passages.*—A primipara, aged twenty-nine, with a contraction of the pelvis, was in labour in the hospital; the fœtal heart was heard; the liquor amnii tinged with meconium; but still the heart was heard; the head-swelling was very large; the child was born apparently dead after strong labour. The heart and funis continued to pulsate, but all attempts at resuscitation were fruitless.—*Dissection:* Slight cyanosis of face and finger-ends; the head was lengthened in the direction of the diagonal diameter; under the external periosteum, in the course of the sagittal suture, were flat dark-red extravasations. In the arachnoid cavity, especially in region of the left hemisphere and at the basis, was a very dark fluid extravasation; several ecchymoses on the tentorium of the right side; on the left side, near the falx, was a rent; the transverse sinus was opened. The brain was much infiltrated, and full of blood. The bronchi contained a thin yellow, somewhat frothy mucous mixture. The lungs were small, deep-sunk against the spine; the edge of one lobe contained a little air, and gave out a little frothy fluid on cutting. The pulmonary vessels, the venæ cavæ, and heart cavities were filled with dark fluid blood. In the pericardium was a little serum; no ecchymosis. Kidneys without trace of uric-acid infarction. Under the microscope the bronchial contents showed the yellow-coloured elements of the meconium, some cholesterine crystals, and the epidermic cells of the vernix caseosa.

In his commentary on these cases, Dr. Breisky observes that the presence of liquor amnii in the air-passages, in the first case, was the effect of premature intra-uterine inspiratory effort, excited by the interruption of the placental circulation. We may also learn from this observation that the cause of the first respiration of new-born children consists in the breaking off of the placental circulation, which gives rise to the *besoin de respirer*. Connected with this is the indication always to tie the cord immediately after the birth of the child; this is the more urgently required in the case of apparently dead and weakly children, in order by suddenly cutting-off the intra-uterine respiration to compel an inspiratory effort.

The etiology of the respiration in the remaining three cases in which there was no compression of the cord is not so clear; but here also the author, relying on Schwartz's experiments, maintains that it was produced by an interruption to the interchange of elements between the maternal and fœtal blood. He believes that the indraught of liquor amnii is facilitated in head-presentations, where some liquor amnii is always ponded up behind the head, allowing freedom for the chest to expand; and he says he has found this not to be the case in breech-presentations, which allow the liquor amnii to run off.

THERAPEUTICAL RECORD.

*Digital Compression successfully employed in a Case of White Swelling.**—The success of digital compression of the principal artery in limbs attacked with inflammation has suggested to Dr. Giachich the idea of adopting this plan in order to allay some very acute pains produced spontaneously in the knee affected with white swelling. At the end of a quarter of an hour, during which the femoral artery was compressed, the patient ceased to suffer. This rapid improvement induced the physician to continue the experiment, and for twenty days this plan was adopted night and morning, the result being that the patient became enabled to move the joint, which had up to that time been entirely motionless.

Powder for Chronic Coryza.†—M. Monnerat has for some time observed that subnitrate of bismuth might be employed with advantage as a local application in the acute stage of coryza. When the affection has become chronic, it no longer yields to the bismuth salt employed alone. In such cases, Dr. Sobrier states that he has found it useful to add the iodide of sulphur. His formula consists of four grammes of subnitrate of bismuth, eight grammes of powdered liquorice, and thirty centigrammes of iodide of sulphur. He prescribes ten or twelve pinches or more during the day, according to the results obtained.

Formula for the Eau Saint Jean, a topical resolvent application.‡—The name of *Eau Saint Jean* has been given to an old preparation used as a local application in cases of traumatic lesions, with or without division of tissues. MM. Corbet and Rouget, after having tried this application on a great number of patients, consider it to be much superior in its effects to the other liquids prescribed in the same circumstances, such as aromatic wine, Goulard water, camphorated spirit, &c. The following formula for the *Eau Saint Jean* is given in the Swiss 'Echo Médical:'—Sulphate of zinc three grammes, sulphate of copper one gramme, dissolved in a quart of water; then twenty-five centigrammes of saffron are to be added, together with fifty centigrammes of camphor dissolved in alcohol. The ingredients are to be left to macerate for forty-eight hours, then filtered and preserved in a close vessel for external use. This preparation is employed in lotions and fomentations, either pure or combined with water. It diminishes suppuration, masks or destroys the fetor of contused wounds, and is very serviceable in the treatment of sprains, dislocations, and fractures. To the other advantages of this liquid must be added its extreme cheapness, which must especially recommend it in the treatment of the poor.

On the Use of Garlic and Lemon-juice in Cases of Membranous Angina.§—Dr. Cazin, of Boulogne-sur-Mer, gives an instance of membranous angina (diphtheria) successfully treated by garlic. The patient was a young lady, aged fifteen, who presented an extreme degree of swelling of the throat; the whole of the pharynx was covered with a membranous exudation, which was only partially diminished by caustic; the nasal fossæ were affected and poured out an abundant secretion (a symptom always considered unfavourable in the Boulogne epidemic), the pulse was small, the anxiety very great, and the strength was almost exhausted. The citro-alliaceous wash, and draughts of

* Giornale Veneto, Dec. 1858. † Bull. Gén. de Thérap., Oct. 30th, 1858.
‡ Ibid. § Ibid.

the same kind, were immediately prescribed. From the first day of this treatment there was a marked improvement, the pulse became stronger, the local symptoms were relieved, the membranous exudations were gradually detached, the swelling of the neck was reduced, the throat and nasal fossæ became free, and convalescence was established at the end of five days.

*On the Treatment of Croup (Tracheal Diphtheria) by large and rapidly repeated doses of Tartar Emetic.**—The epidemic of croup still continuing its ravages in Paris, a variety of measures are employed to subdue this terrible disease, and M. Bouchut relates the history of three cases in the Hôpital Sainte Eugénie, where emetics were given with success. In all these cases the emetic was administered in the following form:—Gum water 100 grammes, syrup of poppies fifteen grammes, tartarized antimony fifty to seventy-five centigrammes. Half a tablespoonful to be given every hour. After relating the details of these cases, the reporter comes to the conclusion that the cure was unquestionably due in each instance to the use of the tartarized antimony. But it is not enough to prescribe the emetic in the dose of five to ten centigrammes, as is usually done; the salt should produce a powerful dynamic shock, often repeated. For this purpose it is necessary to give it at first in the dose of fifty centigrammes to one gramme (a gramme is about fifteen grains troy) with a little syrup of poppies, and to repeat it every half hour in teaspoonfuls; by this plan the children vomit frequently, and thus have a chance of recovering. If, on the contrary, the tartar emetic is digested, there is superpurgation, and the salt only throws the patient into a state of dangerous prostration. To these remarks it may be added, that in one of the cases related, the nurse in the ward seeing a fit of suffocation ensue, and which she supposed to be due (as it really was) to the presence of a laryngeal false membrane, doubled the dose of the tartar emetic at this critical period, and made the patient also swallow two glasses of tepid water. Under the influence of this prompt measure, the child made a violent effort, and threw up a membranous tube of the length of about two inches.

On a Method of Preserving Sulphate of Iron.†—To preserve the crystallized protosulphate of iron from superoxidation and transformation into persulphate, M. Hoorn advises the following process:—The salt is dissolved in a small quantity of water, is slightly warmed, and stirred until the complete cooling of the liquid. The crystals are then collected and dried rapidly at the ordinary temperature, or the crystalline powder thus obtained is compressed at different interval between folds of blotting-paper, and then placed in a vessel hermetically closed and filled up to the neck. M. Haakman points out another method which succeeds in preserving the protosulphate of iron for an indefinite time without alteration; this consists in placing the crystals in alcohol of medium density, and when they are required for use, it is sufficient to wipe them with a cloth and press them between folds of blotting paper.

On the Combination of Iodine with the Extractive Principle of Plants.‡—It is generally considered necessary to make use of an excipient containing tannin when it is desirable to make iodine enter into any preparation. But some recent researches have proved to M. Chaix that all vegetable substances possess the singular property of assimilating iodine, and forming a true combination with this metalloid. If this observation should prove to be correct, the practitioner would not need to inquire, when choosing vegetable extracts, whether they do or do not contain tannin, but would merely prescribe those

* L'Union Médicale, April 5th, 1859.

† Bulletin Général de Thérapeutique, March 30th, 1859.

‡ Ibid.

which are the best indicated by the nature of the disease. M. Chaix has published some formulæ for iodine preparations in the 'Gazette Médicale' of Lyons, as syrup of iodized Curaçoa, iodized syrup of walnut-leaves, &c., and he states that syrup of iodized hop, of iodized gentian, of iodized quina, and of iodized sarsaparilla may be prepared in the same manner.

*Treatment of Nervous Headache by Hydrochlorate of Ammonia.**—Hydrochlorate of ammonia has been lately recommended by some English physicians in certain cases of headache and obstinate neuralgia, and Dr. Barrallier, physician of the navy at Toulon, has made some observations confirming the beneficial effects of this salt in some of the painful affections of the head. He met with varying success, however, some of the cases seeming not to be amenable to the treatment. The conclusions he has drawn are :—1. That the mixture containing hydrochlorate of ammonia almost constantly dissipates the fits of idiopathic headache, and of that which is consecutive to an unusually abundant menstruation. 2. That it is powerless in relieving fits of hemicrania dependent on irregularity or suppression of menstruation. 3. That its use has been attended with pretty good results in cases of cranial pain dependent on functional derangement of the stomach, and also in accidental nervous headache. 4. That it has relieved headaches consecutive upon reiterated attacks of intermittent fever, such as are observed at the decline of severe fevers, and in the course of the period of irritation of typhus. 5. That its action is not remarkable, except when it is given at the period when the pain is most intense.

BOOKS RECEIVED FOR REVIEW.

The Climate of Brighton. By William Kebbell, M.D. London, 1859. pp. 187.

A Guide to the Treatment of Diseases of the Skin. By Thos. Hunt, F.R.C.S. Fourth Edition. 1859. pp. 246.

Homœopathy and Hydropathy impartially appreciated. By Edwin Lee, M D. Fourth Edition, enlarged. London, 1859. pp. 155.

Sur la Réunion des Fibres Nerveuses Sensibles avec les Fibres Motrices. Par G. Gluge et A. Thiernesse. (Extrait des Bulletins de l'Académie Royale de Belgique.)

Annales de la Société Anatomo-pathologique de Bruxelles. Bulletins No. I. et II. Bruxelles, 1859.

Lectures on the Diseases of Infancy and Childhood. By Charles West, M.D. Fourth Edition, revised and enlarged. London, 1859. pp. 755.

L'Ami de la Religion. Nouv. Série. pp. 85.

Phthisis and the Stethoscope; or, the Physical Signs of Consumption. By R. P. Cotton, M.D. London, 1859. pp. 80.

On Diseases and Injuries of the Joints. By Thomas Bryant, F.R.C.S. London, 1859. pp. 273.

A Practical Account of General Paralysis. By Thos. J. Austin, M.R.C.S. London, 1859. pp. 225.

The Indian Lancet. Nos. 1—4. 1859.

The Christian Examiner and Church of Ireland Magazine. Jan. 1859.

Cleveland Medical Gazette. No. 3.

Statistics of Smallpox and Vaccination in the United Kingdom. By W. Moore, M.B. Dublin, 1859. pp. 14.

Aerztliche Brichte Des K. K. Krankenhauses Wieden, vom Solar Jahre 1856 und 1857. Wien, 1858–1859.

Guy's Hospital Reports. Third Series. Vol. V.

Om Uræmi. Akademisk Afhandling af S. Gustaf Troilius. Stockholm, 1858. pp. 56.

Researches on the Intimate Structure of the Brain, Human and Comparative. First Series, on the Structure of the Medulla Oblongata. By J. Lockhart Clarke, Esq., F.R.S. London, 1858. (Phil. Trans.)

The Journal of Mental Science. Edited by Dr. Bucknill. October, 1859.

Report on the Pathology, Therapeutics, and General Ætiology of the Epidemic of Yellow Fever at Lisbon, during the latter half of the year 1857. Blue Book. By Dr. Lyons. London, 1859.

On the Electric Condition of the Egg of the Common Fowl. By John Davy, M.D., F.R.S.

Archives of Medicine, No. 4. Edited by Lionel S. Beale, M.B., F.R.S.

A Manual of Operative Surgery on the Dead Body. By Thomas Smith, F.R.C.S. With Illustrations. 1859.

A System of Instruction in Qualitative Chemical Analysis. By Dr. C. R. Fresenius. Fifth Edition. Edited by J. L. Bullock, F.L.S. London, 1859. pp. 310.

* Bulletin Général de Thérapeutique, April 15th, 1859.

Infantile Mortality and the Establishment of Hospitals for Sick Children. By W. Moore, M.B. Dublin, 1859.

American Medical Monthly. July, August, 1859.

The Surgeon's Vade Mecum. A Manual of Modern Surgery. By Robert Druitt, Licentiate of the Royal College of Physicians. Eighth Edition. London, 1859. pp. 791.

Illustrations to How to Work with the Microscope. By Lionel Beale, M.B., F.R.S. London, 1859.

Illustrations of the Use of the Ophthalmoscope. By W. Martin, F.R.C.S. London, 1859.

Throat Ailments, more especially the Enlarged Tonsil and Elongated Uvula. By James Yearsley, M.R.C.S. Seventh Edition. London, 1859. pp. 112.

Charleston Medical Journal. September, 1859.

North American Medico-Chirurgical Review. September, 1859.

The Morningside Mirror. Vol. XIV. Morningside, 1859. pp. 96.

Statement of Facts relating to the British Opium Trade and Revenue. By the Edinburgh Anti-Opium Committee.

Reports and Tracts from the Ladies' Association for the Diffusion of Sanitary Science. London, 1859.

Third Annual Report of the Board of Works for the Hackney District. 1859.

The Epidemics of the Middle Ages. From the German. By J. F. C. Hecker, M.D. Translated by B. G. Babington, M.D., F.R.S. Third Edition. London, 1859. pp. 360.

Observations on the Structure of Nerve Fibres. By Joseph Lister, Esq., F.R.C.S., and William Turner, M.B. (Reprint.) London, 1859.

An Expository Lexicon of the Terms, Ancient and Modern, in Medical and General Science. By R. G. Mayne, M.D. Part IX. London, 1859.

On the Comparative Value of certain Salts for rendering Fabrics Non-inflammable. By Fred. Versmann, F.C.S., and Alphons Oppenheim, Ph.D. London, 1859. pp. 22.

Description of a New Hysterotome for the Cure of Dysmenorrhœa. Invented by Dr. O. A. White. Charleston, 1859.

Blood Disease. By J. W. Hughes, M.D. London, 1859. pp. 132.

Notes on the Dispatch of Troops by Sea. By C. I. Kirwan, Esq., L.R.C.S.I. Calcutta.

The Facts and Laws of Life. By J. Russell Reynolds, M.D. London, 1859. pp. 39.

Report on the Jails of the Lower Provinces of the Bengal Presidency, for 1858-59. By Fred. J. Mouat, M.D. Calcutta, 1859. pp. 135 and Appendices.

The Nature and Treatment of Gout and Rheumatic Gout. By Alfred Baring Garrod, M.D., F.R.S. London, 1859. pp. 601.

An Introduction to Practical Pharmacy; designed as a Textbook to the Student and as a Guide for the Physician and Pharmaceutist. By Edward Parrish, Graduate in Pharmacy. Second Edition. Philadelphia, 1859. pp. 720.

A Practical Treatise on the Diagnosis, Pathology, and Treatment of Diseases of the Heart. By Austin Flint, M.D. Philadelphia, 1859. pp. 473.

The South of France. 'Notices General and Medical of Hyères, Cannes, Pau, and the Pyrenean Baths. By Edwin Lee, M.D. London, 1859. pp. 114.

The Pathology and Treatment of Pulmonary Consumption. By John Hughes Bennett, M.D. Second Edition. Edinburgh, 1859. pp. 221.

The Manchester Flora. By Leo H. Grindon. London, 1859. pp. 575.

Für Chemischen Constitution des Knorpelgewebes. Von Alex. Friedleben, Dr. Med. Zu Frankfurt-am-Maine.

On the Curative Treatment of Paralysis and Neuralgia, and other Affections of the Nervous System, with the aid of Galvanism. By Harry Lobb, L.S.A. Second Edition. London, 1859. pp. 152.

Phthisis Pulmonalis and Tubercular Phthisis. By C. J. Humphreys, Surgeon. London, 1859. pp. 32.

A Contribution to Urology. By W. Roberts, M.D. (From Memoirs of Manchester Philos. Soc. 1859.)

The Practice of having Wet-nurses. A Paper contributed to the Public Health Department of the National Association for the Promotion of Social Science. London, 1859.

The Influence of the Microscope upon the Progressive Advance of Medicine. By Cuthbert Collingwood, M.B. Liverpool, 1859. pp. 20.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

APRIL, 1860.

PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *Notes on Hospitals.* By Florence Nightingale. *London*, 1859. pp. 108.
2. *On the Defects with reference to the Plan of Construction and Ventilation of most of our Hospitals for the Reception of the Sick and Wounded.* By John Roberton, Esq., Surgeon. (Reprint.) 1856. pp. 17.
3. *A few Additional Suggestions, with a view to the Improvement of Hospitals.* By John Roberton, Esq. (Reprint.) 1858. pp. 26.
4. *Report of the Commissioners on Regulations affecting the Sanitary Condition of the Army.* 1858. pp. 605.
5. *Report on the Formation and General Management of Renkioi Hospital, on the Dardanelles, Turkey.* By E. A. Parkes, M.D. 1857. pp. 64.
6. *Regulations affecting the Sanitary Conditions of the Army.* Published by the War Department. pp. 249.

There is, perhaps, no topic which has received less specific attention from the great bulk of the medical profession than that which relates to the site and construction of hospitals appropriated to the cure of the injured and the sick. Hygiene has not, until very recently, been systematically taught in our schools of medicine. With the honourable exception of the London University, we do not know where hygiene is even recognised as taking part in the curriculum of a medical education. Can we wonder, then, if the great bulk of our profession do not find themselves up to the mark in all that is now being so successfully brought before the public in the important department of sanitary science? Nay, more, we are sorry to see some of our profession actually doing their utmost, by their writings, to make the public believe that sewage emanations and other no less savoury decompositions are not only harmless but even salubrious.

Particular details of topics relating to hygiology have only now and again been studied by us, and then only when we have been placed in circumstances which rendered some acquaintance with the great facts of hygiene an absolute necessity.

A knowledge of all that relates to the site and construction of hospitals embraces an intimate practical knowledge of hygiene; and when an hospital for the sick is about to be built, we know that those who desire to see it carried out are generally at "sixes and sevens" as to where and how it should be erected. We search in vain in our systematic treatises on medicine, or in our cyclopædias of medical science in the English language, for anything which can enlighten us on this most important subject. One exception in recent days we must, however, make to those sweeping statements. That exception is the article "Hospital," written by the late Sir George Ballingall for the 'Cyclopædia of Practical Surgery.' In the course of lectures which he gave in the University of Edinburgh, from the chair of military surgery which he held there, an account of the construction of military and naval hospitals formed a part of his regular course of instruction. He further tells us that to everything connected with the situation, construction, and internal arrangements of hospitals, he gave more than twenty years' habitual attention—"an attention," he remarks, "for which I take to myself no sort of credit, because it is rendered compulsory by the wise regulations of the service in which I passed my earliest years." If we may judge, however, from the examples of hospitals constructed under the influence of the wise regulations of the service referred to, we do not think the doctrines or principles of construction that have been embodied in such monuments of architecture are either worthy of admiration or of being followed as models. The published monograph of Professor Ballingall does not add much to our knowledge regarding hospitals and their construction; and it will not redeem our profession from the charge of having neglected, to a very great extent, this important subject. The topics of the essay are huddled together without a plan, and the subject is left very much as the writer found it. He fails to elicit, to educe, or to establish any general and comprehensive principle upon which the construction of hospitals ought to be regulated. Indeed, Sir George Ballingall found the material to write from so deficient that he acknowledges his desire was to ascertain, rather than to propound with authority, any doctrine or fixed principle.

Such a premeditated abortive intention is greatly to be regretted, inasmuch as few men had more opportunities of knowing what an hospital ought to be than the late Professor of Military Surgery in Edinburgh, for he served personally in no fewer than "three-and-twenty different hospitals, civil and military." No one, therefore, was more entitled to speak from experience than Sir George Ballingall; yet, when he quotes no better model than the great hospital at Plymouth for future generations to copy, we cannot think that his observation and experience led him in the right direction—that is, to the establishment of such sound principles as we think ought to determine the choice of a site for an hospital, or which ought to regulate its construction. We fear, indeed, that the prejudices and superstitions which entangled medical observation and experience a hundred years ago are scarcely yet cleared away. Nay, more, we shall presently show that there were men (and these army men) a hundred years ago who really knew and acted on the knowledge that the best and cheapest of all medicines was a boundless profusion of fresh air; that they would rather watch, shelter, and treat the sick soldier behind a hedgerow, under the canopy of heaven, than consign him to the reeking wards of a military general hospital.*

We believe that the treatment of the sick and wounded in an hospital, or in large numbers together, is a SPECIALTY requiring much more careful study and attention than has been yet given to the subject. Houses intended for this purpose must be designed for the special end in view. The form and construction of ordinary buildings are not suited for such a purpose.

Holding these opinions, therefore, we transcribe the titles of some monographs on hospital construction which have been recently published, with other documents, in all of which much valuable material exists of immense interest to the profession and importance to humanity. From these sources we desire to lay before our readers those doctrines and principles which we believe ought to guide us in the choice of a site for an hospital, and which ought to enable us to follow out in the architectural construction of such a building all those requirements which we now know to be conducive, if not absolutely essential, to more successful and certain results in the treatment of the sick than have hitherto characterized our best intentions.

It is with the greatest difficulty that any important principle can be made to attract the attention of the public, or even of any considerable number of individuals. Doctrines must be embodied in some material and substantial form before they make any impression on the general community. So far as the construction of hospitals is concerned, this statement is most true; for if we consider the very early age of the Christian era, when hospitals are said to have been founded, and the length of time they have been in existence as sanctuaries for the cure of the sick, we find that it has taken centuries to develope any fixed principle which can be considered as fit to determine the construction of hospitals. And since that principle was first enunciated, nearly a century elapsed before it became embodied in a substantial form. At a time when great wars were being carried on in every quarter of the globe—when "Commerce, for the first time was united with, and was made to flourish by War" under the auspices of the greatest war minister that England ever saw—the great principle to which we refer was not only enunciated but embodied on a small scale by some of our army medical men. Their efforts, however, were unavailing; their position was not sufficiently elevated to give their opinions that importance they deserved; and consequently their power to *prevent* disease has always been feeble

* See account of Pringle's Campaigns; also article in Builder, Feb. 4th, 1860.

and inefficient. So far as the construction of hospitals is concerned, recent attempts to construct them indicate retrogression rather than progress; but now that we have a War Minister—Mr. Sidney Herbert—who is justly regarded as the very embodiment of military hygiene, and who has already done so much to better the condition of the soldier, we have confidence that hospital construction and management will date most substantial improvements from his time.

The origin of hospitals is involved in the obscurity of early ecclesiastical history; the circumstances and events which have led to their sanitary improvement are not difficult to trace, and an account of them is a comparatively modern story. It is a story of particular interest to us; and if told in the full minuteness of its melancholy details, it must be a story of thrilling interest to all. Hospitals for the relief of the poor and the sick are pre-eminently characteristic of Christianity. They are spoken of as commonly known at so early a period as the famous Council of Nice, A.D. 305. The first one of note usually referred to as similar to our institutions, is that of Cæsarea, A.D. 370–380, and which was richly endowed by the Emperor Valens. It is said to have been of immense dimensions. The hospitals of Chrysostom, at Constantinople, are next in the order of history; and by the ninth century it is said that there were twenty-four hospitals in Rome alone.* As for medical establishments during war for the care of the sick, little or nothing is said by the ancient Greeks or Romans, or even by the early historians of Christendom. Xenophon, Cæsar, and Polybius, who give the most minute details of war, and the first of whom even considers it essential to provide physicians to accompany his army, yet make no mention of hospitals for the reception of the sick or the wounded. The commentators on the writings of these men would lead us to believe that the sick or wounded soldier was treated in his tent. To the antiquary such research may have special interest; for us the investigation of the subject in this direction points to no result. We only express surprise that so universal and ancient a method for relieving the sick—so important a measure as the institution of hospitals—should have received so little notice in the history of literature and science.

When we look into the records of more recent times, we are still more astonished that no fixed principle seems ever to have regulated or determined the construction of hospital buildings, especially when we consider the large experience which medical men of all nations have had in the treatment of the sick in all kinds of buildings. The very early nations of antiquity give proofs in every page of their history of a great disregard for human life; and from the sad history of disease and mortality in more recent times, of undoubted nosocomial origin, we lament that even now, in this age of progress, we have not been less regardless than the ancients of the lives of our fellow-men. We have been slow to profit from the lessons of history; and yet our experience has been dearly bought. Hosts of suffering humanity have

* See Beckman's History of Inventions and Discoveries for the best account of the ancient history of hospitals; also on the Establishment of Hospitals, a lecture by J. M. Arnott: London Medical Gazette, 1839–40, p. 71.

walked through the deep and dismal Valley of the Shadow of Death, before we have even looked in the right direction to stay their slow but steady march to the grave. A knowledge of nosocomial influences as they affect the condition of the sick has not been derived from anything like correct statistical comparison; and, indeed, such influences are practically but little recognised. That such influences do arise, however, there can be no doubt; and any considerable experience in hospitals of various constructions and of various administration will soon enable one to detect those unwholesome conditions which are due to the site or construction of the place, and which exercise a powerful effect upon the aspect of the sick, the duration of the illness, and the termination of the case. Accurate and uniform hospital statistics really do not exist by which to compare different hospitals as to the amount of good they do. The sanitary state of an hospital cannot be inferred solely from its greater or less mortality.

"If," says Miss Nightingale, "the function of an hospital were to kill the sick, statistical comparison of this nature would be admissible. As, however, its proper function is to restore the sick to health as speedily as possible, the elements which really give information as to whether this is done or not, are those which show the proportion of sick restored to health, and the average time which has been required for this object. Hospital mortality statistics, as usually compiled, give us but very little useful information which can be compared year by year, or place with place; accordingly the mortality of different hospitals is the most fallacious test of the salubrity of an hospital that it is at present possible to use."

There are reasons, as some think, for believing that in hospitals where the severest disorders and injuries are admitted, a large mortality may be regarded as evidence of the goodness of the hospital—that the most judicious selection of cases had been made—that nevertheless many die—therefore that hospital is the best. Besides the character of the cases admitted, and the ages of the patients, we absolutely require other data before we can judge of the wholesomeness or unwholesomeness of hospitals. The Statistical Congress soon about to meet in London, could not do a greater service than that of drawing up and promulgating some uniform scheme by which to register hospital statistics. This topic meets us at the very threshold of our inquiry, and is so important that we would shortly draw the attention of our readers to the schemes proposed for registration. The first is that adopted in a very valuable paper by Mr. Thomson, in the sixth volume of the 'Edinburgh Medical and Surgical Journal' for 1843, and relates to the wholesomeness of Scotch Hospitals; others may be seen employed in papers on Hospitals in the fifth and seventh volumes of the 'Statistical Society's Journal.' The most recent is the scheme propounded by Miss Nightingale, in her 'Notes on Hospitals,' page 2.

Mr. Thomson's scheme, as he gives it, is unfit for use, for although it gives details of what ought to be got, it does not tell us how to get them; it takes no cognizance of sex or of difference of ages. As to Miss Nightingale's scheme, we heartily join in the wish so well expressed by her, that—

"If the hospitals of London, Paris (and we would add of the country generally), would give us the information contained under the following heads, so important would be the knowledge thereby conveyed, that it would be worth while to go back for many years to construct such tables, and to continue the same forms hereafter."

Her scheme is the following :—To obtain

"1. The numbers admitted for each decennial period of age for each sex per annum.

"2. The numbers, similarly arranged, remaining in hospital at the end of the preceding year.

"3. The numbers dead of each sex at each decennial period of age per annum.

"4. The numbers discharged cured, similarly arranged, per annum.

"5. The numbers discharged incurable (or, we would add, 'unrelieved'), similarly arranged, per annum.

"6. The numbers remaining in hospital at the end of the current year, similarly arranged.

"7. The diseases remaining, admitted, died, cured, discharged incurable and remained, arranged for each sex, and each decennial period of age per annum.

"8. The duration of cases similarly arranged."

This scheme seems to us to have only one defect. It does not "enter an appearance" for those who are discharged as "irregular" or improper cases, of which not a few every year find their way into hospitals.

Our knowledge regarding the comparative salubrity of hospitals is at present derived almost entirely from evidence of a circumstantial kind ; and there are now on record several instances which are painfully conspicuous in demonstrating the dire effects of pernicious nosocomial influences. The observation and recognition of these dire results mark out epochs in the history of those attempts which have been made from time to time to improve old hospitals, or to devise better plans for new ones.

It has been remarked that the practice of physicians, whether good or bad (according to orthodox medical views), does not materially influence the ultimate mortality of an hospital. If the mortality under three physicians in the same hospital is to be compared, although it would be an odious comparison, the one treating his cases on the expectant method, another on the tonic system, and the third by the eclectic system, no appreciable difference would be observed in the mortality, but the length of time which the disorder would continue, and the character of the convalescence of the cases of each, would be widely different.* Let us look now for a moment to the nature of the evidence on which the pernicious results of nosocomial influences rest, and which shows that certain fixed principles exist which ought to determine the site, construction, and administration of hospitals. Our historical narrative will strengthen the evidence.

In Russia, in 1811, it was found that the average mortality of the general hospitals, which contained above thirty patients, was one in nine ; but in those which contained a less number the mortality was one in ten.†

In Edinburgh, in the Royal Infirmary there, for the year ending

* Dr. Craigie's remarks on Mr. Thomson's statistics, loc. cit.

† Mém. de l'Acad. de Science de Petersbourg, tom. ix., quoted by Mr. Thomson.

the 1st October, 1842, the daily average number of patients was 303, and the mortality 12·53 per cent.; the cures 63·89 per cent.; the period of treatment twenty-nine days, with 5·09 cures to 1 death.

In the Glasgow Infirmary, for the year ending 31st December, 1842, the daily average number of patients was 228, the deaths, 9·63 per cent.; the cures, 77·70 per cent.; the length of treatment, twenty-three days; with 8·06 cures for 1 death.

In the Aberdeen Infirmary, during the year ending 31st December, 1842, the daily average number of patients was 113; the mortality, 4·64 per cent.; the cures, 80·02 per cent.; the residence in hospital, twenty-one days; with 17·15 cures to 1 death.

In other Scotch hospitals the deaths ranged between 4·36 per cent. in the Northern Infirmary at Inverness, and 6·84 in the county and city Infirmary of Perth.

In thirty English provincial hospitals the mortality for one year ranged between 2 per cent. in the Canterbury Hospital to 7·50 per cent. in the Manchester General Infirmary; the average of all the thirty hospitals being 4·46 per cent.

During the years from 1817 to 1842–3, a very great increase was noticed in the mortality of the Edinburgh and Glasgow Infirmaries—an increase from about 6 per cent. in both to 12·20 in the former, and 11·50 in the latter. With the exception of the hospitals of Chester and of Manchester, it does not appear from Mr. Thomson's records that any similar increase took place in the mortality of other hospitals; and Dr. Craigie, in his remarks on Mr. Thomson's statistics, comes to the inevitable conclusion, that the *ratio* of mortality in any given hospital increases whenever there is any great increase in the number of patients; that the more patients that pass through the same hospital the greater the ratio of deaths per cent.* And further, as Miss Nightingale shows, "there *is* even a *certain ratio* between the number of sick placed in a building and the amount of mortality." But it is found that some hospitals have better *recovering conditions* than others, while in other respects they seem to be the same.

It is now known that bricks and mortar disposed in the form of a building, 700 feet square and three stories high, can accommodate with safety 800 to 1000 patients (*e.g.*, Barrack Hospital, Scutari). But it is now known that the same materials could have built an hospital, differently placed and constructed, to have accommodated easily 3000 patients, *with good recovering conditions.* It is indeed ruinous to build hospitals after the former plan. The 'Report of the Sanitary Commission on the Army,' and the 'Builder' newspaper, have been the first to enunciate the principle in our day, or rather to propound the essential question for consideration in the construction of all hospitals. It is a question economical as well as sanitary. It is *to find that construction which will accommodate the greatest number of patients upon a given area, with the greatest facilities for economy, administration, and recovery in the shortest possible time.*

Pernicious nosocomial influences indicate the defective sanitary state

* British Medical Almanac, 1836–37; Hawkins' Elements of Statistics; Thomson's Statistics of Civil Hospitals.

of an hospital much better than its mortality returns. Medical men are beginning to recognise this fact;* but we do not think that such pernicious nosocomial influences have been sufficiently brought into notice. Indeed, it is often difficult to point out any single instance in any single hospital of special pernicious influences at work; but cumulative evidence of the same general character soon makes the seat of the mischief apparent to the intelligent observer. It is exactly a hundred years ago since Sir John Pringle recognised the existence of such pernicious influences, and ever since his time several illustrious men have joined in the crusade to improve the construction of hospitals. This band of philanthropists counts but few in number, and their praiseworthy efforts we will now endeavour to bring together in historical order. They have indeed been crusaders, for they seem to have promulgated their beliefs, and urged their suggestions, upon most stubborn unbelievers.

It was towards the first half of the eighteenth century† (A.D. 1700 to 1750) that numerous new edifices arose in Great Britain dedicated to the support of the poor under the severe afflictions of disease and want. General infirmaries were established in almost every considerable provincial town; and what were called "Houses of Recovery" were established in London and most large cities of England, Scotland, and Ireland. The influence of these large establishments on the treatment of the sick soon became a subject of observation and study; and when war was successfully waged abroad, and commerce flourished at home, abundance of opportunities were not wanting to judge of the effects of treating the sick in great numbers together. The sickness and mortality of our soldiers abroad soon made manifest a great fact—a fact which every one has verified who has had much hospital experience, and to which all who have written on the subject of hospitals have borne ample testimony.

In 1764, Sir John Pringle stated, in the preface to his great work 'On the Diseases of Armies,' that hospitals are among the chief causes of mortality in armies, "on account of the bad air and other inconveniences attending them." He was alive to the great necessity of preventing as much as possible the pernicious influence of a general hospital atmosphere. Hence he formed the idea of separating or isolating the sick in limited numbers; and hence also arose his scheme for the formation of "separate regimental infirmaries." They were conceived with the intention of saving the charge of a great hospital. But they effected a more important object; for as he distributed them they rendered the spread of an hospital atmosphere an impossibility, but which is the inevitable consequence of a large and crowded house—as hospitals are generally constructed. (p. 33). He considers large common or general hospitals as highly objectionable, on account of the great mortality which ensues when large numbers of sick are thus brought together. In every campaign he has recorded the fatal effects of

* Axenfeld: des Influences Nosocomiales, 1837.

† See History and Mortality of Hospitals, by Dr. Walker: Edinburgh Medical and Surgical Journal, vol. lxvii.

hospital fever, and directs attention to two conditions for its prevention —namely, (1) a due regard to the choice of hospitals; (2) the right management of the air in them. He is of opinion, also, that the effluvia from dysenteric fæces may not only propagate dysentery, but he believes that such excrementitious effluvia may even generate a fever peculiar to hospitals and gaols, when large numbers of sick are confined in them. He lays it down as a rule, that the more fresh air that can be let into an hospital the less is the danger to the sick and their attendants. One great principle he especially seizes hold of, and enforces with much energy—namely, the necessity for spreading or dispersing the sick in small numbers, so as not to crowd them into one hospital, or even into one village. A narrow compass may be more economical and easier for administering to the wants of the sick; but, practically, he is of opinion that every consideration ought to be sacrificed to secure the recovery of the sick. Their welfare must be paramount in all our arrangements. His experience led him to pronounce in favour of small hospitals instead of large general ones, with wards as large and as airy as possible; and to admit so few into each ward, that "*a person unacquainted with the danger of bad air might imagine there was room to take in double or triple the number.*" This general rule is remarkably interesting in an historical point of view; for it shows that the idea of abundant cubic space being necessary for sanitary purposes had possessed the mind of Sir John Pringle, although he could not express the amount definitely, but could merely give this vague mode of showing the proper proportion of beds to each ward. He recommends open fireplaces under a chimney, and condemns the use of stoves.

Dr. Richard Brocklesby succeeded Sir John Pringle as Physician to the Army. He was a Fellow of the Royal Society and of the College of Physicians; he was the friend and physician of Samuel Johnson, and his name frequently recurs in the narrative of the events of the later years of the great sage's life. From 1758 to 1763, he published his 'Medical and Œconomical Observations.' He was most successful in his arrangements for providing temporary accommodation for the sick, and he shows that huts for small numbers (forty to one hundred and twenty) are much better than large hospitals. He strongly condemns the infirmaries of his time, as they existed at Brompton lines, near Chatham, and states that in the small-pox apartment of one of them he found the ceilings little more than six feet high, and in each small room of this sort sixteen loathsome bodies were often crowded. The small lozenge casements were made without any openings, and no ventilation had been provided for any of the wards. The details of evidence recorded in the 'Report of the Commission on Regulations affecting the Sanitary Condition of the Army,' would lead us to believe that in 1857–58, the Chatham military hospitals had made little or no progress in sanitary reform since the days of Richard Brocklesby.*

* See questions and answers in Report: 2007, 2179, 2113–14, 3486–89, 2115–16, 2302, 2305–15, 2307–18, 2019–22, 2064–75, 2111, 1346–47, 1352–55, 1375, 2888–92; also Report, p. xxxiii.

In 1771, Mr. John Aikin, a surgeon at Warrington, published a paper entitled 'Thoughts on Hospitals.' We find him also observing that—

"Hospitals, instead of a blessing, have proved an additional misfortune to the afflicted. Every hospital (he says), without exception, may in some measure be considered as having its own peculiar disease within it. I am intimately acquainted with a county infirmary remarkable for neatness and apparent excellence of construction, and I have even there seen a slow depressing fever, the offspring of putridity, creep over the other complaints of the patients, and become the principal disorder, resisting every remedy that could be thought of, till dismission from the house produced a spontaneous cure."

About this time an enormous mortality in the great Hôtel Dieu of Paris arrested public attention, and the extensive origin of disease within its wards called for investigation. By the statutes of its foundation, "all applicants were to be admitted." The catastrophe that followed marks an epoch the most remarkable in hospital history, only surpassed by the unexampled mortality of Scutari hospitals. The discussion and inquiry to which it led on the part of benevolent and intelligent men, resulted eventually in the introduction into France of the greatest improvements in hospital construction and management which have taken place up to the present time. Then it came to be written that "hospitals are a curse to civilization;" may we hope now that the era has commenced when they will prove a blessing. In 1773, a Commission of the Academy recommended the entire reconstruction of a new Hôtel Dieu on a more salubrious and commodious site. In regard to hospital construction, they also recommended that each ward should contain only fifteen or sixteen beds, and with respect to large hospitals, it is remarked that they will be the most healthy which shall consist of *several separate pavilions,* according to the plan of the Academy, or like the hospital at Rochefort.* Still the question remained in abeyance, and again in 1785–86 we find another Commission of the Royal Academy of Sciences of date 22nd November, 1786, confirming and extending the suggestions of the previous Commission. They reported that the disposition of the wards led to general infection of the air of the hospital—in other words, an hospital atmosphere was common to every apartment in the building. The convalescents were mixed with the sick of all kinds, and the specifically contagious cases with the ordinary sick. They reported that small-pox cases were sometimes (22nd December, 1782) more than two in a bed. One ward, indeed, held seven sick over and above its complement for thirty-five beds. The whole hospital had but 1200 beds, yet these beds used to receive at the same time from 2000 to 5000 sick, and during epidemics as many as 7000 sick have been in the building at one time. From 20,000 to 30,000 sick passed through the hospital every year, and about twenty-five per cent. of them were carried to the grave. In the other hospitals of Paris, the mortality was about 12½ per cent. of the sick. The Commissioners reported on the unwholesome state of the surgical wards. They found three or four parturient women lying on a single bed, festering in corruption and humidity, and respiring the

* Hist. de la Soc. Royal de Médecine, 1777: Nouveau Plan de Constitution pour la Médecine en France.

foulest air. They especially called attention to the very great mortality from surgical operations, and attributed the enormous mortality in puerperal cases to the infectious vapours continually being evolved from the surgical cases. It had been observed and recorded so early as 1666, that the more surgical cases the greater was the tendency to epidemic fever, and now it is distinctly stated that the great mortality is due to the general infection of the air. As to the construction of the house, it is recorded that the wards were too low, and much lower than those of any other hospital in Paris. The eminent chemist, Lavoisier, had at that time stated that five cubic feet of air per hour were consumed by one man, but the dimensions of the Hôtel Dieu admitted only of one and a quarter. The Commission were struck with the great length of time which patients remained in the hospital, and ascribed the great mortality to vices of hospital construction. They concluded that the Hôtel Dieu was insufficient for the wants of Paris, that it was incommodious and insalubrious, that it was necessary to rebuild it in another place and on a different plan.* Much discussion, and the recommendation of many plans, followed the recommendations of these Commissioners, and ultimately led to the publication of a very valuable work by Tenon on the hospitals of Paris. He proposed also to publish a volume on the hospitals of other countries, but this he never accomplished. He was the friend and contemporary of Sir William Blizard, of the London Hospital, also an hospital crusader and reformer, and to him he sent a copy of his great book. He regrets that no work exists on the formation or distribution of hospitals.

At this time also the astounding revelations of John Howard† were made regarding gaols and hospitals. The successful efforts of this great philanthropist in reforming the condition of prisoners gave an impulse also to sanitary improvements in hospital construction, and he gives a detailed account of how an hospital ought to be constructed—an account to which we shall afterwards refer.

We next come to the writings of Sir Gilbert Blane, who is the first to compare hospital treatment of the sick with their treatment in private practice. He records twelve years' experience of St. Thomas's Hospital, from 1784 till 1794, and compares it with his private practice from 1795 to 1805.‡ He records the general law observable amongst the lower animals as well as man, that when large numbers are congregated in ill-contrived tenements, so that the exhalations and excretions from the living body are not completely removed, then disease is produced. Thus we have glanders among horses, originating in large stables; the distemper among dogs, originating in kennels; and the typhous fevers originating in gaols, hospitals, and ships. He observes that the site and construction of hospitals greatly modify the influences of any hospital for good or evil. In giving an account of St. Thomas's Hospital, he notices the original badness of the site, from the swampy nature of the ground, that ague was

* Extrait des Registres, &c., et Rapport des Commissionaires chargés par l'Académie de l'examen du projêt d'un nouvel Hôtel Dieu. 4to, Paris, 1786.

† Account of Lazarettos, Hospitals, &c., in Europe. 4to, 1791.

‡ Medico-Chirurgical Transactions, vol. iv. p. 106.

common, and that fever was often caught in the wards. That for the first time in his day whitewashing the walls was instituted with benefit, and also contrivances for ventilation. He noticed that the recoveries of all classes of patients were retarded by the impure air of the place, that the convalescence of patients with severe injuries, and those who had undergone the capital operations of surgery, were especially tedious. Howard had also recorded of St. Thomas's Hospital, that compound fractures rarely survived, and that the mortality among parturient women and their infants was remarkably great. Sir Gilbert Blane mentions the cubic space for each bed as being 700 to 1000 feet, and, from his experience of civil and military hospitals, he judges that 600 cubic feet is the smallest that ought to be allowed. He does *not* believe in the infectious qualities of fevers if proper arrangements are made for ventilation, attention being paid to cleansing the patient and keeping him clean. This view of the doctrine of contagion is precisely similar to that so successfully expounded and maintained by the late Professors Gregory and Alison of Edinburgh; and it is the one, also, which Miss Nightingale maintains at page 93 of her work on hospitals. Dr. Alison held that the doctrine of contagion, in the case of fevers especially, mainly rested on no stronger foundation than the observation and record of facts in badly-constructed and ill-ventilated hospitals and close rooms. In such places, the emanations from the sick play a corresponding part to the emanations from cesspools and other nuisances in producing fevers out of hospitals. Both classes of emanations become fatal, or remain innocuous, according to the extent to which they are diluted in pure atmospheric air. A few fever cases in a crowded, ill-ventilated ward may spread fever, but, in a well-ventilated hospital, with plenty of cubic space and rapid change of air, they certainly will not. It is lamentable to know how much our fever hospitals stand in need of improvement in this respect, for the mortality in some of the best of them is really enormous; and when the treatment of cases in them is compared (as it has been*) with the treatment of similar cases at their own houses, the comparison shows how egregious a mistake fever hospitals are, as at present constructed. When fever, erysipelas, or gangrene spreads in an hospital, or originates in its wards, such an event does not prove contagion; it merely means that the laws of nature, as to the abundance of fresh air, are not being carried out. It means that the ward contains more people than it ought to have; or that some pernicious nosocomial influences are polluting the place. When such conditions are detected and removed, then the apparent contagion will cease, but not till then. "Miasma" may be said, roughly speaking, to diminish as the square of the distance; and in fresh air is not found to extend much beyond three feet from the patient; although miasma from the excretions may extend a considerably greater distance.

"If it can be ascertained," says Sir Gilbert Blane, "that the preponderance of cases taken into an hospital are such as ought to recover, but that never-

* Dr. Adams: Monthly Journal of Medical Science, August, 1850; and Steele in Edinburgh Medical and Surgical Journal, No. 176.

theless they die or contract other diseases, something is certainly wrong with the site or construction of the hospital."

No account of the hospital mortality at St. Thomas's was kept till 1689, when the mortality is stated to have been 1 in 10. In 1721, the hospital was rebuilt, and no account is given of the mortality till 1741, when it is still about 1 in 10. In 1783, improvements were made as to cleanliness and ventilation; and for the ten years previously the mortality had averaged 1 in 14; but for the ten subsequent years it averaged 1 in 15·6; and for the ten years from 1803–1813 the average was 1 in 16·2. In the table of admissions from 1783–1794, some statements are very innocently made which clearly demonstrate the unwholesome nature of the place. We find it stated, for instance, in a special note referring to "erysipelas," that "many cases, besides" the one referred to in the table, occurred in the hospital supervening on other complaints.* A person was brought in suffering from simple vertigo, with headache: he took erysipelas of the face, followed by mortification of the hip, and died thereof. A patient was admitted with a disease of the eye; he took a continued fever, and died. Another admitted with scrofula is similarly disposed of; while a third is admitted with some urinary complaint, who goes the same way to the grave.

A few months ago the removal of St. Thomas's Hospital was mooted, but not for the first time. We have seen that about thirty years ago a proposal was made and printed—of date June 27th, 1832—by the medical officers of the hospital, in which they jointly recommended its removal from its present site to a more eligible situation. The following are the facts submitted to the Governors:

"1. The decayed state of the building—a considerable part of which is so dilapidated, that in all probability it will require to be rebuilt within a short time—and the yearly outlay for repairs contingent on such a state.

"2. The greatly improved architecture of hospitals, as regards their internal economy, ventilation, and salubrity, since the erection of the present building.

"3. The low, close, and confined position of the hospital, as it at present stands.

"4. The greatly extended surface and population of the metropolis requiring a more equal distribution of such charitable establishments.

"5. The greatly increased accommodation of Guy's Hospital within the last few years.

"6. The relief afforded by the London Hospital, and the establishment of the Dreadnought Hospital-ship for sick and wounded seamen.

"7. The notoriously smaller number and less urgent importance of cases applying for admission now than formerly; attributable, we presume, to the facts last mentioned."

This interesting and important document was signed by Robert Williams, John Elliotson, H. S. Roots, Benjamin Travers, Joseph Henry Green, and Frederick Tyrrell.

We come now to a most important treatise on the site and construction of hospitals. Iberti, a native of Italy, travelled over different countries, and paid particular attention to the sites of hospitals, to the abuses in the management of them, and to the essential faults of their

* Loc. cit. p. 133.

construction. About 1788 he published an octavo volume of observations, and we must regard him as having made the first attempt to put such topics into a practical shape.* His attention was drawn to the subject by observing the comparative mortality of different hospitals under apparently similar circumstances. Much discussion, as we have seen, had previously arisen regarding the Hôtel Dieu of Paris, from the large mortality and origin of disease within its walls; and now Iberti tells us how a conviction was gradually gaining strength among those who thought of such things, that the "relief of the sick poor was better and more successful by treatment at their own homes than in the hospital." But as in large cities many have no homes, therefore he recognised hospitals as necessary evils. He is of opinion that both methods of treating the sick poor should be combined; that out-door visitation at the residences of the sick should be associated as much as possible with hospital relief; and in as many cases as possible it should precede it. Rome, he says, first set the example in this direction; for from time immemorial that city has been divided into twelve districts, to each of which a physician, a surgeon, and an apothecary are provided to attend to the wants of the sick poor whose poverty is properly attested. This example has been followed in many countries, in Spain especially, and was recommended in France by a commission.† The plan of out-door attendance on the poor is also followed out to a great extent in Edinburgh and other towns of Scotland; and Iberti is of opinion that it is impossible to conceive a plan of relieving the sick poor more extensively useful or more economical than that of combining house visitation with hospital treatment. We know, also, that no plan is better than this for medical education.‡

Iberti had been attached to hospitals in Italy and Spain, and he soon became painfully aware that diseases often had their origin in those charitable institutions, whose insalubrity not only retarded the cure of the sick, but added diseases to those for which the patient had been admitted. He became impressed with the belief that on the proper disposition of an hospital and of its parts depended not only its proper ventilation, but in a great degree the success of its internal policy, the regularity and expedition of its service. With these views he projected a model hospital, and although much of his plan may not now be approved of, yet he has seized upon some of the most essential points to be observed in hospital construction. The first essential he notices is that an hospital should be so constructed that the air has free access to it on all sides, and the following conditions he enumerates as necessary to accomplish this end:—(1), separation from other buildings; (2), due exposure; (3), a proper distribution and fit correspondence of the different parts of an hospital. He attributes the defects in hospitals mainly to the following circumstances:—(1), The aim of the architect to

* Iberti: Observations générales sur les Hôpitaux, suivies d'un projet d'hôpital, &c. 8vo, London, 1788.

† Hist. de la Soc. Roy. de Médecine, 1777: Nouveau plan de Constitution pour la Médecine en France.

‡ See on this topic an excellent paper by R. G. Whitfield, Esq., of St. Thomas's Hospital, of date 1856.

economy and architectural elegance at all risks; (2), an attempt to accommodate the greatest number of sick in the smallest possible space; (3), the great boast of hospitals seeming to consist in the number of sick they receive. He objects to more than two floors, as rendering service difficult, increasing danger in case of fire, and preventing ventilation. He recommends that the ground-floor should be elevated above the level of the court, that it should be lofty, and entirely appropriated to the official residences and offices of administration. The patients are to be treated entirely on the second floor.

Since the time of Sir John Pringle and Dr. Richard Brocklesby we have had no further suggestions as to hospital construction from the medical department of the army; now, however, in 1791 we find the apothecary to H.M.'s forces, Mr. Stewart Henderson, at the Cape of Good Hope, giving the outline of a plan for military hospitals on a principle and construction different from any yet established in Europe.* He, like all before him, was painfully convinced of the bad construction of hospitals and the want of proper arrangements in them, and such circumstances he considers the principal causes of the ill success which attends the military practice of physic. He believed that such conditions greatly contributed to induce hospital gangrene, and nursed infection to that degree of virulence so fatally experienced in every quarter of the world where H.M.'s troops have been treated in hospitals. "A crowded hospital, badly situated, with want of cleanliness, ventilation, and *good nursing*, will certainly counteract the best effects of medicine." The plan he sketches out is for an hospital intended to accommodate 600 or 700 men. So convinced was he of the necessity of isolating the sick, of the necessity of preventing the possibility of a common infection or hospital air, that he carries the principle of isolation to the utmost possible extreme. But although his principle must be acknowledged to be good, the details of his plan are simply impracticable—and indeed, as we shall see, they are unnecessary. He considers that each patient should have a separate apartment 7 × 8 feet, and 10 feet high, with a window over its door and a window on the side opposite the door. These little hospitals are to be erected at four feet distance from each other, and to be arranged in street rows of twenty feet apart. For convalescents, the houses are to be 120 feet long, eighteen feet broad, and fourteen feet high, with separate doors to each, opening into the street.

He is the first also to suggest that a corps of hospital attendants for the army should be regularly and permanently appointed to hospitals, in the proportion of twenty to each 120 patients, and that there should also be a matron with ten female nurses, besides cooks and others.

In 1796, a very valuable little work was published in London by Sir William Blizard, F.R.S., and Surgeon to the London Hospital. He modestly entitles it 'Suggestions for the Improvement of Hospitals.'

His suggestions really meet many of the difficulties of the subject, and are given in the form of aphoristic dicta. He also had come to

* Medical and Physical Journal, vol. i. p. 461.

the conclusion that the number of patients admitted into an hospital does not indicate the number of lives preserved, the degree of misery lessened, or the source of benefit to the community. It is the proportion cured and relieved, or the time taken to accomplish these ends, in a given period, which will express the happy or unhappy consequences of hospitals to society; and that proportion will rise or fall according to the degree of purity of the air respired, and inversely according to the number congregated in a given space. His experience leads him to record that the pernicious influence of air strongly impregnated with effluvia, is strikingly remarkable in cases of compound fractures and fractured skulls. Under all circumstances it is hurtful, although its effects may not be distinctly obvious at the time. The powers of the body may be superior to its influence. He shows how the circumstances of disease are highly polluting to the surrounding air; how, during the progress of fevers especially, the air is very rapidly polluted, so great and rapid is the evolution of malignant effluvia from the bodies of the sick. At the time when he wrote (1796), he was of opinion that this limitation of the numbers in an hospital should be far below the average then existing. As to the site for an hospital, he points out the danger of its proximity to standing water, especially if it is laden with animal filth. Such a state of things proves a source of continuous mischief. He observed that the influence of sewage exhalation often made itself strikingly manifest, for its presence often brought about an unfavourable alteration in a few hours in the aspect of every sore, as well as in the state of nearly every patient in the hospital subjected to its pernicious influence. He suggests also various methods of judging of the purity of the air; by eudiometers, for instance, to determine the composition of the air; while the growth of plants or of trees he thought might also be made tests of the nature and degree of purity of the air in various situations; and he notices that in the wards of an hospital some plants grow with great rapidity.*

As to hospital construction, he suggests separate wards, hospitals, or pavilions, for the separate accommodation of delirious cases, contagious diseases, and for convalescence. For convalescents, he further suggests that buildings should be erected out of town, and so relieve the "mother hospital." As to the place and erection of hospitals generally, he thinks that reason and experience ought to guide us by some fixed principles; and on setting about to build an hospital, he inculcates the following doctrine:—(1) That a committee be appointed to obtain information from every source that may prove

* Means and instruments for testing the purity of the air or of the presence of organic matter in it have been from time to time devised. The determination of ozone is now well known, but apparently not of much use. An instrument known as "Granville's septometer" is noticed in the Medical and Physical Journal for 1826, vol. lvi. p. 198, from which it appears that chlorine was the agent used to determine the impurity. Recently, Dr. Angus Smith has devised a method which might be very useful if it could be simplified. The healing of a wound is not a bad septometer. A wound (by subcutaneous section, e.g.) not exposed to the air heals by a different physiological process from that of a wound exposed from the first to atmospheric influence (Paget: Surgical Pathology, vol. i. pp. 181, 2); and that again is variously affected by the pernicious influence of vitiated air.

useful to the undertaking.* (2) That the best possible site be fixed upon. (3) That the *plan* of the building should not be left to architects and surveyors, but they should be required to plan and to execute according *to a determined system.* As to the site and plan of building he gives the following advice :—The site should be elevated and out of town; the soil should be gravelly, or at least dry, with a fall towards a river or stream of water, convenient for drains and sewers. The surrounding country should be free from wood, marshes, or standing water; and a sufficient extent of ground ought to be secured, so that buildings or obnoxious manufactories of any kind should never come so near the hospital as to be detrimental to the air thereof. As to structure, he remarks that curvilinear plans are to be avoided, for the air moves in straight lines, and is resisted or deflected like other bodies in motion. He decidedly objects to having the buildings arranged in a square form, for the air will become stagnant in the enclosed area. Parallel wings he objects to, while high walls and close investments are also to be avoided. He roughly indicates, by means of printer's breaks or rules, the general forms of ground-plans which may suit different spots with the greatest advantage. He thus indicates that the wings should be divergent from a common central block, and in each design the hall, chapel, dispensary, and generally all the administrative offices, should be in this central part. The buildings for the patients are to consist of one floor only, or at most not to exceed two stories. In other details he follows the recommendations of Howard.† The wards are to be from twenty-five to thirty feet long, fifteen feet high to the ceiling, and as much space is to be assigned to each patient as answers to an ordinary private bedchamber. The disposition of windows and doors is to be such as will secure currents of air in every direction, while light and air are to accompany each other. Darkness in passages or other parts of a building produces an unfavourable impression on the healthiness of the place. Some of the windows ought to open from the ceiling to the floor; and if of one floor only, openings ought to exist in the ceiling for ventilation. If of two floors, similar openings are recommended in the ceiling of the upper ward, while capacious funnels from the ceiling of the lower wards are to be continued upwards through the ceiling of the upper ward. Each ward is recommended to have two doors, one of them iron latticed or canvassed. The windows ought to be lofty and opposite each other, and the fireplace in the middle of the long side of each ward; and the vaults, with water-closets, are to be on the outside. The first floor of the building must be raised four or five feet above the ground level; and the chemical laboratory, kitchen, wash-house, and such offices, ought to be detached from the main building. The kitchen ought to be lofty, and have a funnel from the ceiling to the air. He objects to the servants eating in the kitchen; separate dining-rooms ought to be provided for them.

At the beginning of the present century we find Dr. Flajani, the

* A suggestion afterwards reiterated in another form by Mr. R. Martin.—See Appendix to Army Sanitary Report, p. 461.

† On Prisons and Lazarettos, p. 141.

Professor of Surgery in the Santo Spirito Hospital at Rome, now directing attention to the important subject of hospitals. From 1798 to 1803 he published four volumes of surgical observations and reflections,* and a considerable portion of the work is devoted to the state of the public hospitals at Rome, with proposals for their reform addressed to the Pontifical Government. From alienation of their funds and neglect by directors, these institutions *had become insalubrious and fatal to those who entered them.* He supports his assertions by a comparison of the mortality of hospitals throughout a series of years, and he especially urges the following subjects for consideration: 1. The number of hospitals amongst a certain population; 2. The site and sanitary structure of hospitals; 3. The arrangement of the wards; 4. The separation of contagious diseases; 5. The great good to be obtained from the institution of officers of health; 6. The regulation of the diet for the sick; 7. The institution of convalescent establishments; and 8. The regulation of pharmacy.

During the first fifty years of the present century, with the exception of the monograph of Sir George Ballingall already referred to, and a paper by Dr. Steele in the 'Glasgow Medical Journal,' we find no important paper on hospital construction. Several new hospitals have been built during this time, and many of the committees directing these institutions generally consulted the opinions of eminent medical men on the subject. The New Dundee Hospital was not erected without much consultation, and the advice was especially asked of Professors Syme and Christison, of Edinburgh, who visited the place and wrote detailed reports on the subject. The additions to the Newcastle-upon-Tyne Infirmary also gave rise to much consultation and discussion,† and we could mention many more. Such discussions arose simply because no fixed principle seems ever to have been recognised by which hospitals could generally be planned; and we doubt not that if access to the archives of hospitals could be obtained, much curious, and perhaps valuable, matter might be collected on the subject of hospital construction. In 1847-48 an interesting paper was published in the 'Edinburgh Medical and Surgical Journal' (No. 177), by Dr. Robert Paterson, giving an account of the epidemic fever which prevailed in Edinburgh during that year, and giving also an account of the treatment of the cases in the hospital as compared with the treatment of similar cases in wooden sheds and tents. The treatment of fever in sheds and tents gave a much less mortality in them than in the hospital. In the hospital it averaged 15·42 per cent. for males, and 10·03 per cent. for females; while in the sheds and tents the mortality for males was 12 per cent., and that for females 7 per cent. The sheds were three in number, and all differently constructed, and it is curious to notice how the favourable results were the more apparent just as the position of the shed was more open to the influence of fresh air. The most successful treatment was in a shed one hundred and ninety-two feet long, which the managers of the hospital, at the wise suggestion of Dr. Paterson,

* Collezione d'Osservazioni e Riflessioni di Chirurgia di Guiseppe Flajani, M.D., &c.

† Appendix to Report of Sanitary Commission, p. 454, No. xl.

divided into three compartments, so as to make three sheds of it, separate from each other, with a separate door to each compartment. It was erected on an open space of ground; it had windows on both sides and opposite each other, with ventilators in the floor and roof. Thus there was perfect ventilation, and the mortality was only 9 per cent. In the large circular tents the mortality was only 5 per cent.; that of the hospital being 10 per cent. on the same class of cases. Dr. Paterson also suggests in this paper a plan for constructing such sheds, to which we must refer our readers.

Dr. Steele's paper was written when an extension of the Glasgow Hospital was under consideration. The principle, however, which he lays down as the rule to guide hospital construction is absolutely untenable. He proposes that the external form of an hospital should be arranged according to the amount and position of the ground at disposal. By all means make the most of the ground that can be got; but if the nature or amount of the ground were always to determine the form and construction of an hospital, we should never arrive at any sound principle of hospital construction of uniform or general application.

There is nothing, then, in our language of recent date which treats satisfactorily of hospital construction prior to the paper of Mr. John Roberton, read before the Statistical Society of Manchester in March, 1856. Soon after that the 'Builder' newspaper took up the subject, and in that excellent periodical a number of most interesting papers were written on the construction and ventilation of hospitals, from June, 1856, till now. For the first time general plans were designed after the principles we now advocate for the architectural arrangement of hospitals. Miss Nightingale has greatly matured the subject, and in her work before us she has brought a knowledge of those fixed principles which ought to regulate the construction of hospitals within the reach of all. We have to thank her and the editor of the 'Builder,' Mr. George Godwin, for some of the plans and woodblocks which we have used at the conclusion of this paper.

In the spring of 1857 the staff of the Middlesex Hospital forwarded to Government two memorials, containing some valuable remarks against the plan of Netley Hospital, and the same year Miss Nightingale delivered a body of written evidence of the most elaborate kind on this subject to the Commissioners appointed to inquire into the sanitary condition of the army, and which was published in a "Blue-book" report, of date 1858. In March of that year we again find Mr. John Roberton, of Manchester, calling attention to the subject, when he put into circulation a paper containing further observations on hospitals, the result of a long tour in Belgium and Germany, made during the preceding autumn. He gave in this paper a plan of the Blackburn New Hospital, which we reproduce on a small scale. On this paper Dr. Richardson made some excellent observations in the 'Sanitary Review' for July, 1858.* This country, then, is much indebted to Mr. Roberton, of Manchester, to the medical staff of the Middlesex Hospital, to the Army Sanitary Commission, to the 'Builder' newspaper, but, above all, to Miss

* See also British and Foreign Medico-Chirurgical Review, vol. xviii. p. 451.

Nightingale, for their advocacy of the pavilion system of hospital construction, in opposition to the corridor system, and for their enlightened labours in the cause of good hospital construction generally.

But why has the subject of hospital construction remained nearly in abeyance during the last fifty years? Simply, because we had ceased to notice hospital insalubrity. As we become insensible to the beauties of the stars by gazing on them long, as we are apt to overlook the blessings of civilization because we live constantly under its genial influence, so we become insensible to those many evils with which we are always more or less constantly surrounded, which often embitter our existence, and which slowly, but surely, undermine our constitution. It is only some conspicuously remarkable event which rouses our curiosity and excites our attention. An astronomical phenomenon may astonish us, perhaps, once in a lifetime; some great social change may scatter blessings among the inhabitants of a country which they had not known before, signalize the government of the State, and excite the gratitude, the confidence, and the love of a united people; while a remarkable, a conspicuous, and a dire calamity (like the loss of the finest army the world ever saw), will no less forcibly arrest popular attention, rouse the indignation of the people, and shake the confidence of a nation, but it will call forth the best exertions of philanthropists and statesmen to prevent for the future a similar result. Such has, indeed, been the effect of the losses experienced by our army in the hospital at Scutari during the Russian war of 1854–6, when the sanitary commissioners, with Dr. Sutherland as their chief, commenced their labours, and when the mortality in the month of February, 1855, had reached the incredible rate of 41·5 per cent. per annum. In the great catastrophe of the Hôtel-Dieu it never exceeded one-half that rate. But here, indeed, was to be recognised "an *ignoble* fellowship of death," and which has marked off two great epochs in the history of hospital hygiene. Out of evil good has come. The attention of the country has been once more effectually called to investigate the principles which regulate the construction and hygiene of hospitals; and the various monographs and reports with which we have headed this article have forthwith appeared. From them we hope the greatest permanent improvement in hospitals will take their date. Miss Nightingale, with Mr. Sidney Herbert and Dr. Sutherland, have led the van in the practical work of this great crusade; and we look forward with much interest to the forthcoming report of their doings in improving the military hospitals and barracks of the country.

We have seen, also, that within the last century and a half most of the hospitals in this country have been built; and the demand for them has been supplied. Once done, therefore, there they remain, and if bad, they are generally irremediable. As a profession, therefore, we have not many opportunities of projecting a new hospital, nor of interfering by our good advice in matters relating to hospital hygiene. Moreover, when we do interfere, our influence for good is generally overpowered by the majority in committees and boards of directors, or by the superior influence of quartermasters-general, or some such similar "heads" of departments. The Commissioners, in their Report, also most justly exonerate those of us whose fate connected them with army hospitals and their management. The evils of them, so much complained of, "have been the subject of constant, though fruitless, representations on the part of the medical officers."*

We now come to lay before our readers an account of the principles of hospital construction which the experience of the last century has gradually developed, as we have seen, and which are so clearly brought out by the inquiries of the Sanitary Commission, expounded in the columns of the 'Builder,' and brought within the reach of all in the 'Notes on Hospitals,' by Florence Nightingale.

Our object is to explain the nature of that kind of hospital construction which will accommodate the greatest number of patients upon a given area, with the greatest facilities for economy, administration, and recovery in the shortest possible time.

The first point to be attended to is undoubtedly the selection of the site. Climate here is the first consideration. We must seek to obtain a pure, dry air for the sick, to the exclusion, therefore, of damp climates. In the more damp localities of the south of England, for example, it is now well known that certain classes of sick and invalids linger on without recovering their health. The climate is intimately related to the nature of the ground. Clay is highly retentive of moisture, and where it forms the subsoil, it will keep the air over large districts of the country always more or less damp. Soils of this character are therefore obviously unfit for hospitals. Soils which extend to a considerable depth in gravel and sand, with a good foundation below of consistent marl, and which are so far self-draining, are the best possible for hospital sites. Valleys, marshy and muddy ground, ought, therefore, to be avoided. It may seem superfluous to state that an hospital should not be built over an old graveyard, or on ground charged with organic matter. Such, however, has been done again and again; and camps also have been pitched on similar spots. That such examples may never be repeated, we relate two prominent instances of dire disaster which resulted from such a course. Hospital diseases have invariably been the consequence. Sir James Macgregor relates how the sick in the hospitals of Ciudad Rodrigo suffered from dysentery, hospital gangrene, and tetanus; and it is also no less interesting to observe that tetanus is recorded to have disappeared after those sanitary measures were adopted which did away with the occurrence of other diseases. What was the state of Ciudad Rodrigo when it was occupied and made an hospital station? Why, shortly before that time 20,000 bodies had been buried within its walls within a few months; moreover, it was unhealthy independent of this circumstance.†

Again, when the First Division of our army arrived at Varna, on the 13th of June, 1854, they were healthy till they encamped at Aladyn,

* Report, p. xxxvii.
† Copland's Medical Dictionary, vol. i. p. 703.

on the 1st of July in that year. They unfortunately took up the *same site* which the Light Division had occupied there, and had just left to advance upon Devno. Mark the result—seventeen days after encamping here, cholera first broke out among the men, preceded by the prevalence of diarrhœa.* Again, on the 20th of October, 1854, the battle of the Alma was fought, and after it our army halted on the ground, where the imprudent choice of a site for the Fourth Division was soon made painfully obvious. They encamped on the heights above the Alma, on the very ground recently occupied by the Russians, the place being strewed with the straw and old materials left by them, while some dead bodies also lay there.† One would have thought the bitter experience of the second encampment at Aladyn, after occupation by the Light Division, would have been sufficient to have prevented this incautious step. It was not so; the ground was taken up, and cholera raged in the camp. We learn that many similar mistakes have recently been made in India, and protested against by our military medical officers. The position of our camps in Bulgaria generally, during the Russian war, exemplifies on a large scale the unfortunate selection of a site. The British army was there scattered "broadcast" over a country, from Monastir to Varna, a distance of twenty-seven miles; a district known to be pernicious to the health of man, and notwithstanding its exquisite beauty, a hot-bed of pestilence, disease, and death. Diarrhœa, cholera, fever, and dysentery soon verified by their ravages the name which the Turks gave to Devno, namely, "The Valley of Death."

"What at first sight seemed to be natural advantages, such as beauty of scenery, varied by hill and dale, wood, water, and vegetation, contained, at the same time, abundant evidence to the scientific that the elements of insalubrity existed also. It may not be useless now to enumerate the unfavourable elements which characterized the physical climate of the place.

"1. In the mornings and evenings the encampments were often enveloped in dense and thick mists, which appeared to come from the lakes. In the words of Russell, 'the lake and the streams may be said to have exhaled death, and at night fat unctuous vapours rose up fold after fold from the valleys; creeping up in the dark, they stole into the tent of the sleeper, and wrapped him in their deadly embrace.'

"2. Green wood and brushwood had to be cut down to make ground for the tents to be pitched upon; a proceeding of the most imprudent kind in a sanitary point of view, as is well know to the settlers in America and in India.

"3. The marshy nature of the vegetation in the meadow lands, combined with the presence of amphibious animals of the batrachian kind, such as frogs and tortoises. The presence, in numbers, of the common frog is considered an unerring indication of an unwholesome and marshy locality.

"4. The presence of innumerable flies of all shapes and colour, and clouds of locusts, followed by numerous flocks of insect-eating birds, combined to point out an unhealthy physical climate.

"5. The nature of the soil, combined with these features, would indicate to the geologist the unhealthy nature of the district.

"6. The range of temperature was equally unfavourable."‡

* See Parliamentary Papers of Sebastopol Committee.

† Sebastopol Committee, p. 74: Major-General Bentinck.

‡ Glasgow Medical Journal, April, 1857.

On the erection of hospitals in towns, it is obvious, as Miss Nightingale writes, that—

"Nearly all that has been said under the last head, *mutatis mutandis*, may be repeated here. If the recovery of sick is to be the object of hospitals, they will not be built in towns. If medical schools are the object, surely it is more instructive for students to watch the recovery from, rather than the lingering in, sickness. Twice the number of cases would be brought under their notice in a hospital in which the sick recovered in half the time necessary in another.

"According to all analogy, the duration of cases, the chances against complete recovery, the rate of mortality, must be greater in town than in country hospitals. Land in town is too expensive for hospitals to be so built as to secure the conditions for light and ventilation, and of spreading the inmates over a large surface-area, conditions now known to be essential to recovery, instead of filling them up three or four stories high, a condition now known to be opposed to recovery."

The statistics of hospitals show also* that, other things being equal, a hospital in a town yields not only a higher mortality, but fewer permanent recoveries, a longer duration of sick cases, and therefore a greater current expense to the administration, than a hospital in the country would do for the same number of cases.

We now come to consider the general plan of construction of hospitals. France and Belgium have already shown us the example, by embodying the great principle for which we here contend in the magnificent hospitals of Lariboisière and Vincennes, large plans of which were given in the pages of the 'Builder,' to which we must refer our readers, or to the 'Notes on Hospitals' by Miss Nightingale. For a plan of the attractive and beautiful Bordeaux Hospital, the reader is referred to Mr. Roberton's first paper. Besides these there is also part of the Beaujon Hospital at Paris, and the Saint John's Hospital at Brussels, all constructed upon the same principle. In this country we have as yet only one hospital constructed upon this principle. It is the new hospital at Blackburn, which (as we are assured by a reliable informant) "will soon be ready to receive patients. The new Ashton-under-Lyne Infirmary is also in course of erection. These new hospitals," he continues, "will in time put to shame the unwholesome institutions for the reception (*not the cure*) of the sick and hurt in most of our large metropolitan and provincial towns." The only objection that can be raised to the Blackburn Hospital is that from the small size of the pavilions it will be found to be somewhat expensive in administration. This general plan, which embodies the great and only sound principle of hospital construction, is that which is known as the pavilion system; that is, the separating or breaking up of the hospital into a number of distinct pavilions, containing generally not more than one hundred sick in each.

Seeing, then, that we *must* have hospitals, and that the sick must be distributed over a large area, in a number of separate buildings, rather than one large one, how are these separate buildings to be arranged? We shall first show how they are *not* to be arranged.

* See Sources for Statistics already quoted, ante.

Figures 1, 2, and 3 represent arrangement of buildings at present occupied for hospital purposes, which ought to be carefully avoided.

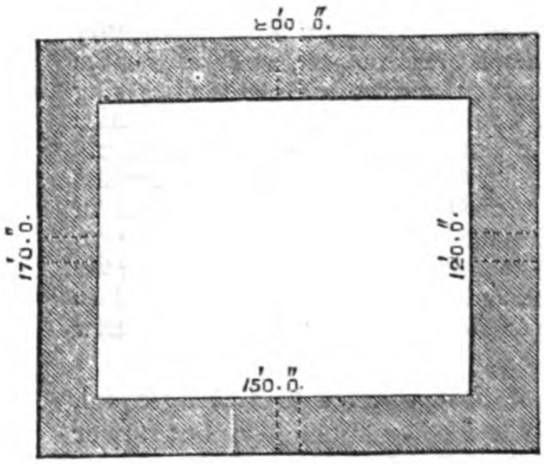


Fig. 1.

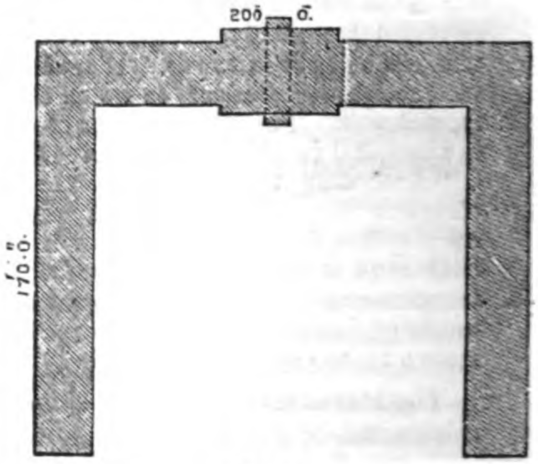


Fig. 2.

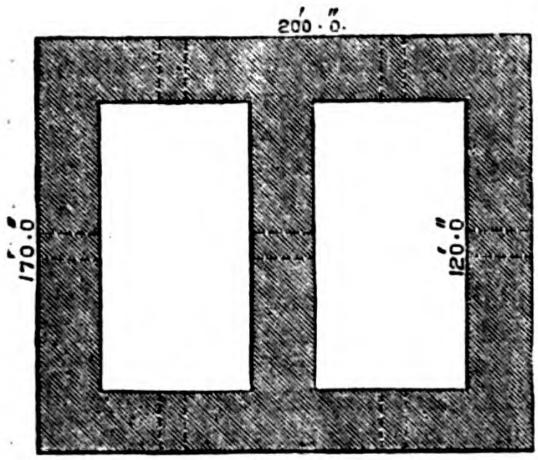


Fig. 3.

Fig. 4.

It may be considered certain that wherever such arrangements exist, injury to the sick is so constant, that were it practicable, all the angles should be opened to admit of the circulation of air. The simplest form of structure for ensuring light and ventilation is to build hospital wards in a straight line (Fig. 4), with windows on both sides—i.e., back and front—the length-way of the ward being the length-way of the building, and the administration in the centre. By such an arrangement as this, however, no more than four wards could be obtained, if the building were two stories high. For small hospitals, not exceeding 120 sick, this plan would be economical and efficient. The direction of the axis of such a building should be from north to south, a little inclining to the east, which would ensure the sun shining on both sides every day of the year, and would also protect the wards from the north-east winds. One staircase suffices for such an hospital. Carried from the bottom to the top of the building, and ventilated above the roof, the stair would cut off entirely one set of wards from the other, which is all that is necessary to prevent the possibility of any intermingling of foul air. An hospital such as this is also capable of extension, by projecting wings at each end of the building, as represented in Fig. 5, the arrangement adopted in the new infirmary at Dundee,

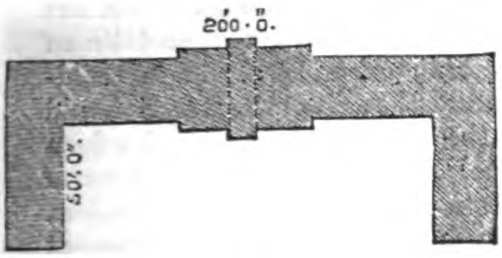


Fig. 5.

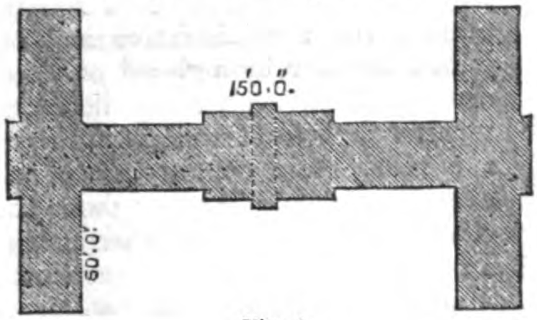


Fig. 6.

the front of which extends across an area of 350 feet, the length of the wings being 120 feet. The other form (Fig. 6) is the one carried out in St. George's Hospital, London, the Jenny Lind Hospital, Liverpool, and many other hospitals. In such cases additional staircases must be put at the ends. The disadvantages of such additions are the closed angles produced; but if the wings were very short in proportion to the length of the front, the closed angles are of less consequence. A better arrangement is that where the wings are entirely detached from the centre (Fig. 7), and where they are connected with it by a corridor on the lower floor. This is the plan adopted in the great military hospital of Vincennes.

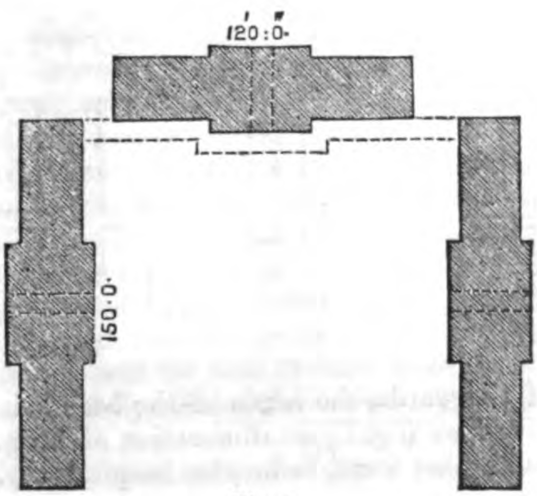


Fig. 7.

All of these plans, however, have the disadvantage of not admitting extension beyond a certain limit. The only plan which allows as much extension as can be necessary in any single hospital up to (say) 1000 sick (beyond which, hospital management becomes very difficult), is the plan adopted in the hospital at Bordeaux, or, still better, that of the Lariboisière at Paris. Fig. 8

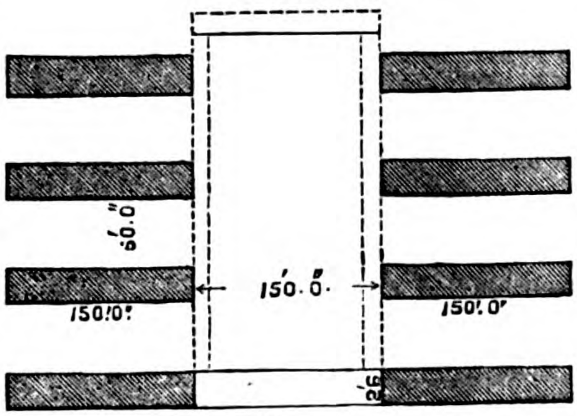


Fig. 8.

represents the arrangement of the separate blocks or pavilions. Here each block or pavilion, containing one hundred and two sick, constitutes a separate hospital. There are six of these blocks, arranged on two opposite sides of a square, and there are four blocks containing the administrative and other offices. All the pavilions are joined together by a glazed corridor along the lower flat, and by an open terrace above for convalescents taking exercise. In such buildings, for the sake of sunlight in this country, the axis of the wards should run nearly from north to south, and the distance of the blocks from each other should be about twice the height of the side walls. In warm climates, such as our Mediterranean stations, this distance may be less. In the very elegant new garrison hospital in course of erection at Malta, from designs by the Sanitary Commission on Hospitals and Barracks, the distance between the pavilions is about a third less; and the walls most exposed to the sun are double, to subdue the effect of the sun's rays in the interior of the wards, while the windows are provided with "jalousies." It is obvious that the great objects aimed at by such an arrangement are subdivision of the sick, free ventilation outside and inside the building, abundance of light, and windows on opposite sides

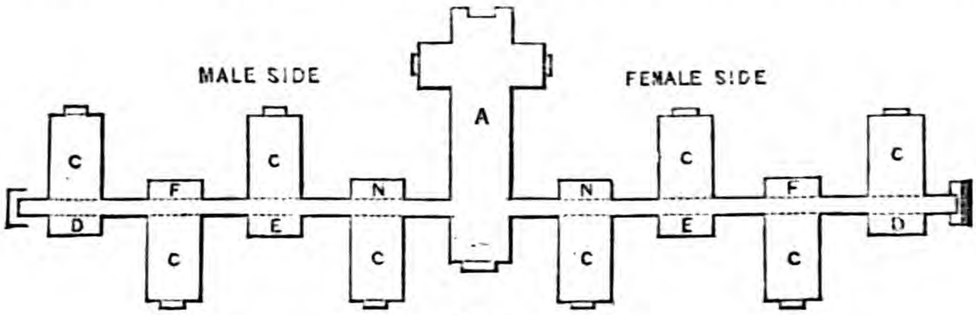


Fig. 9.

of the wards, the heads of the beds being between the windows. We give here (fig. 9) an illustration of how the pavilions may be arranged in another form, as in the hospital at Blackburn, the ground-plan of

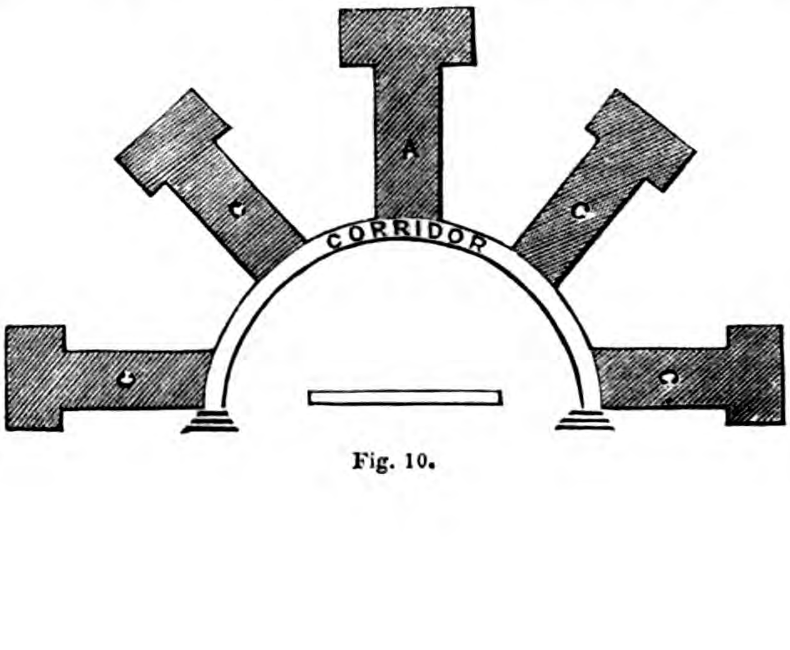


Fig. 10.

which we have here reduced from Mr. Roberton's large plan given in his second communication, and the objections to which we have already stated.

We have also seen a plan suggested by Dr. Parkes, in which the pavilions are arranged in the form of a crescent, as in Fig. 10. Such a plan as this is more adapted to warmer climates, as it is obvious that in this country the pavilion blocks could not all be arranged with their axis from north to south. In this semicircular form, the extension upon such a plan is of course limited by the area of the circle in which the pavilions are projected.

Upon the pavilion system, for which we here contend, the beautiful hospital at Renkioi was erected. It was designed by Brunel and superintended by Dr. Parkes, who writes that

> "Nothing could exceed the simplicity of the whole arrangement; it was a repetition of similar parts throughout, and experience enables me to say that nothing could be better adapted for a hospital than this system of isolated buildings, between every one of which was a large body of moving air, rendering ventilation easy and communication of disease from ward to ward impossible."

We wish we could afford space to quote the conditions given by the late Mr. Brunel in designing these buildings. We must refer to Dr. Parkes' concise and excellent 'Report on the Formation and General Management of Renkioi Hospital,' addressed to the Right Honourable Secretary of State for War, of date December 1st, 1856.

Some important details intimately related to the general plan of hospitals require to be noticed under this topic. An hospital for 1000 sick ought to have none of its offices in duplicate. It should have one kitchen, so situated that the diets can be easily carried to any ward, and in the shortest possible time to all. The kitchen should be separately ventilated, and should not be under any part of the building used for the sick. The laundry should be at a distance from the hospital, and in no way connected with it. The offices of the administration should be conveniently situated, not only for facility of access, but for efficiency of superintendence. No sewer or drain should pass under any part of the hospital where there are sick. Sewers and drains should be outside, and free even of the external walls; there should be ample means for ventilation, inspection, and flushing. The water-supply should be of the purest and softest description. It should be laid over the whole building, hot and cold. This arrangement, together with that of lifts, saves at least one attendant to every thirty sick. For water-supply there should be a water-tower and tank placed centrally, affording water under high pressure. The mains should be of cast-iron varnished, the service-pipes of wrought-iron; there should not be any cisterns, but the supply should be constant. Fire-cocks or "hydrants" should be placed at short intervals. Each supply-tap should have a waste sink underneath it, and these should be placed so as to save labour. All staircases and passages should be wide, light, and airy. The steps should consist of stone, but may be covered with wood. All hospitals should have convalescent wards supplied with dining and day-rooms. The square within the hospital and the space between the pavilions should be laid out as garden-ground, with well-drained and rolled walks, and shaded seats for convalescents.

So much for the general architectural arrangement of hospital buildings. We proceed to consider, lastly, the ward arrangement with reference to light, ventilation, and administration. In spite of many popular fallacies, we believe that every sick ward should be capable of being flooded with sunlight. Obviously, wards for the treatment of eye diseases are excepted. The windows, therefore, should bear a large proportion to the wall-space in all hospitals—not less than one to two.

Few greater mistakes can be committed in hospital construction, as far as light and ventilation are concerned, than placing the windows at *one end* of a ward, or even at both ends, with beds ranged down the sides, their heads to the dead wall. Such we have observed to be a very common arrangement in the worst German hospitals; it also is the arrangement in some of our military hospitals, and strange to say, it is the plan adopted at Netley. All plans and ward arrangements should provide for the greatest economy in administration consistent with the objects for which hospitals are, in the first instance, instituted. Facility of superintendence and nursing must exist. The wards should therefore be of such a size and so arranged that the head nurse may have all her sick under her eye at once, and the head nurse's room should have a window looking into her ward, one ward sufficiently large being allotted to each nurse.

These common errors in hospital-ward construction are shown in Figs. 11, 12, 13, and 14.

Fig. 11 represents the arrangement in King's College Hospital, and in the new wards at Guy's, London. Fig. 12 is the ward arrangement usually carried out in buildings which have been converted into civil hospitals. It was designedly adopted in the military hospital, Portsmouth, as also in many other military hospitals. Fig. 13 is the arrangement in a few ill-constructed foreign hospitals—e.g., one at Rotterdam and the new garrison hospital at Berlin. It exists also at Chatham garrison hospital, and it is to be perpetuated at the Victoria Hospital at Netley! Fig. 14 shows the kind of arrangement in the wards and corridors of the old Royal Marine Hospital at Woolwich. These diagrams do not pretend to extreme accuracy, they simply indicate arrangement; and the plans for wards which they illustrate are contrary to every sound principle of sanitary construction, and ought for the future to be avoided in hospital building.

As to the size of wards, it would appear that is the best which accommodates from twenty to thirty-two sick, with a height of from fifteen to seventeen feet. Wards of this moderate size are less subject to an hospital atmosphere than wards of double the size. The cubic space for each patient in this climate has been fixed by European sanitary science at not less than fifteen hundred feet, and a good proportion for a ward to hold twenty patients (ten on each side of the ward) would be eighty feet long, twenty-five feet wide, and sixteen feet high.

Such dimensions would give to each of the twenty beds one thousand six hundred cubic feet, and it would allow thirteen feet between foot and foot of opposite beds, with an average width of sixteen feet to each two beds.

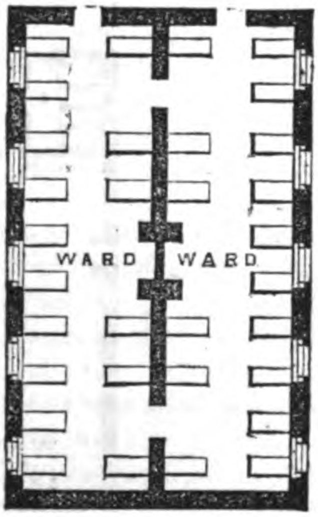


Fig. 11.

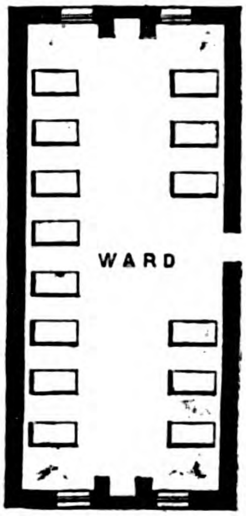


Fig. 12.

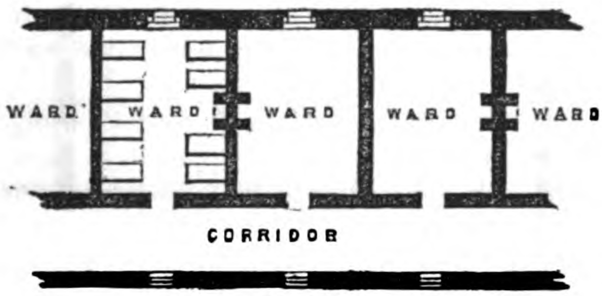


Fig. 13.

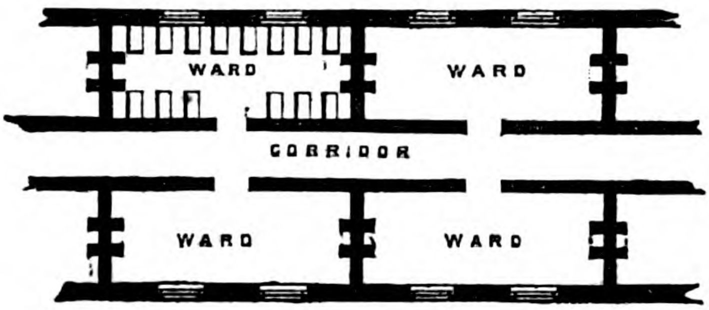


Fig. 14.

A design in detail for a pavilion hospital is given in the 'Builder,' which we here reproduce, Fig. 15; and the following short summary of the conditions of its construction are there detailed—

"The distance between the blocks should be double their height; in this country there should be two flats in a pavilion, and one ward to a flat. The

DESIGN FOR A PAVILION HOSPITAL.

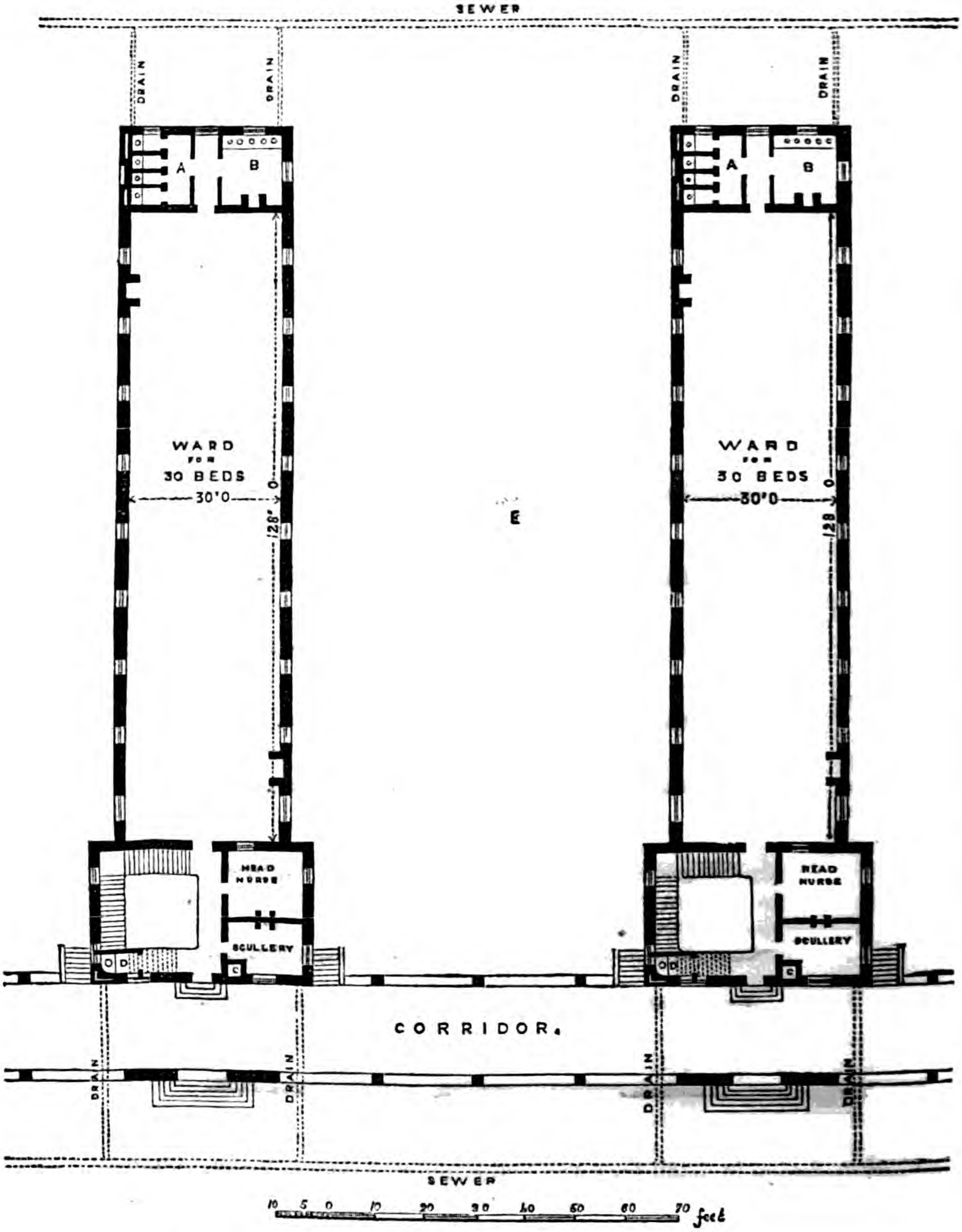


Fig. 15.

A. Ward closets.
B. Bath and lavatory.
C. Lift in scullery.

D. Private closet.
E. Ornamental ground.
Ward windows to be 4ft. 8in. in the clear.

hospital should be erected to form a square, the basement story of the pavilions being connected by a corridor, and the whole of the basements erected on arches. The wards should hold twenty to thirty sick, each bed should have from fifteen hundred to two thousand cubic feet of air space allowed to it.

"The following table exhibits the proportions of a ward for thirty-two patients. The first column gives the proportions of such a ward in the Lariboisière Hospital; the second, the proportions adapted to a larger cubical space, such as is given in our best hospitals in this country:—

	I.			II.	
	ft.	in.		ft.	in.
Length of ward	111	6	...	128	0
Breadth	30	0	...	30	0
Height	17	0	...	17	6
Wall spaces between end walls and windows	5	0	...	6	4
Breadth of windows	4	8	...	4	8
Breadth of wall space between windows . .	9	2	...	11	4
Height of windows	13	0	...	13	6
Cubic space per bed	1760	0	...	2100	0

"It is undesirable to increase the width of any ward beyond thirty feet, because the distance between the opposite windows becomes then too great for efficient ventilation.

"The proportions of a ward for twenty patients might be eighty feet long, by twenty-five feet wide, and sixteen feet high. This would give about one thousand six hundred cubic feet to each bed.

"One window at least should be allotted to every two beds. There are hospitals with a window to each bed. The windows should be double, or be glazed with plate-glass, to prevent loss of heat. Tripartite windows, like those of Middlesex Hospital, are useful for ventilation. The ward walls should consist of pure white polished Parian cement, or some other equally white non-absorbent substance. Grey-coloured cements should be avoided: they never look clean; they give the ward a sombre appearance, and they hide dirt. The best ward flooring is oak. No sawdust nor other organic matter, capable of rotting, should be placed underneath the flooring."

In conclusion, we would draw attention to the arrangements for latrines. There should be not less than one closet to each ten patients. We coincide in the opinion that these closets ought to be external, but near the wards, and under such an arrangement of plan as to prevent any possibility of atmospheric contamination. Probably with the intention of avoiding angles, and of preserving an even, unbroken surface to the pavilion walls, the doctrines of the text of the 'Builder' have not been practically followed out in the design, Fig. 15 ante. In the ventilation of the latrine, the principle appears to be somewhat departed from, which is inculcated throughout the articles in that paper—namely, by open windows on opposite sides.

The latrines, as represented in Fig. 15, and enlarged in Fig. 16, are not, we think, sufficiently external to the wards, while the position of the windows and doors connected with them is such as to drive the air of these places directly into the ward. We much prefer the plan figured by Mr. Roberton as that of the Hospital of St. John, at Brussels. It is shown in Fig. 17, and at once tells its plan. Here the latrines are external, while the arrangement of the windows is consistent with those of the rest of the building, and with that arrangement for ventilation which Miss Nightingale considers the best.

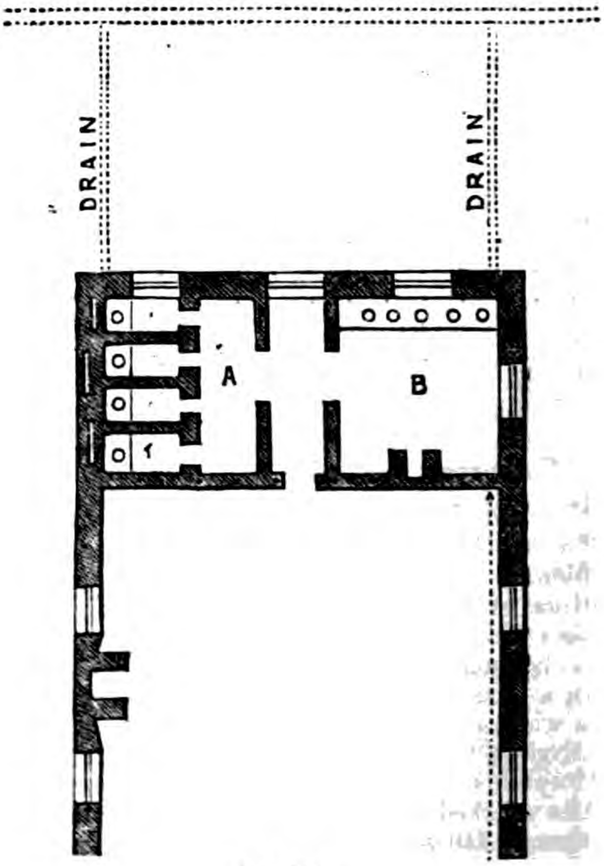


Fig. 16.

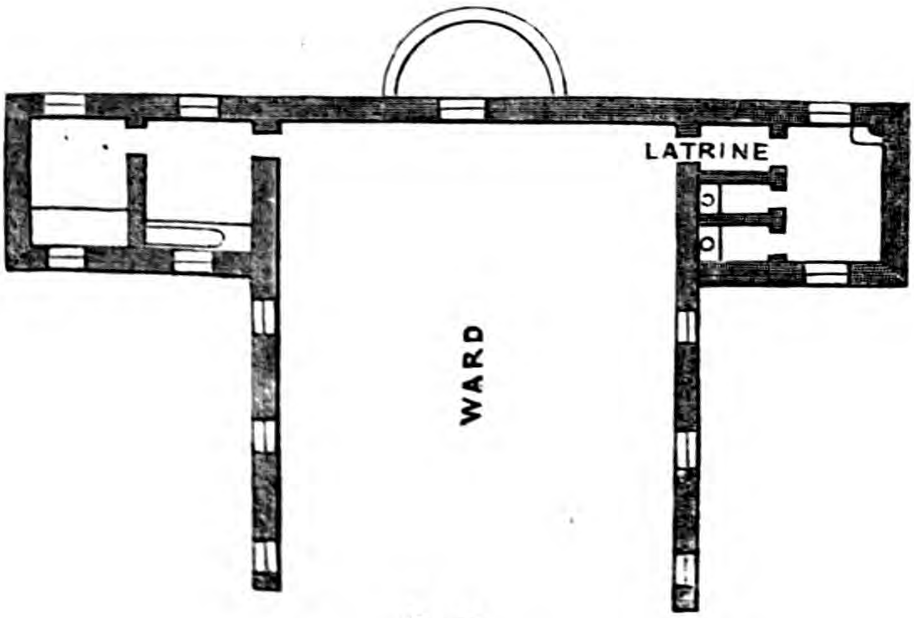


Fig. 17.

The following ward plan (Fig. 17), with the latrine arrangements in the new summer hospital at the Charité, at Berlin, we give here as an example of the worst possible construction. The general latrines are placed in the corridor, whilst smaller ones are also placed in each of the wards. The wards, corridor, and water-closets all open into each other—the best possible arrangement for preserving a uniform hospital atmosphere, and the worst arrangement for an hospital.

Our space will not permit us to discuss the vexed questions relative to ventilation. We could take up as much space with this topic as has been devoted to this article. If the subject of ventilation is a difficult one as regards buildings in general, it is greatly more so in regard to hospitals. The emanations from sick people are given off in increased abundance, and with increased rapidity, compared with those from the healthy, so that the air of a ward is vitiated with much greater rapidity when filled with sick, than if it were filled with the same number of healthy people. It is still more excessively vitiated if all the sick are fever cases—if the hospital is a fever hospital. Many gases, we know, are also diffused with great rapidity, as when liberated from decaying animal substances and fermenting organic matter, by chemical decompositions of various kinds. The diffusion of these gases takes place with a rapidity proportioned to the square roots of their densities; and no artificial cements will prevent such diffusion. Many gases also pass through fluids and so-called solids. Hydrogen gas and its compounds easily pass through the pores of stucco; so that plastered walls and ceilings are no barrier to the diffusion of cesspool emanations. Hence the intolerable and incurable nuisance of them. Latrines therefore, as generators of this gas, ought not to be placed in contact with hospital wards.*

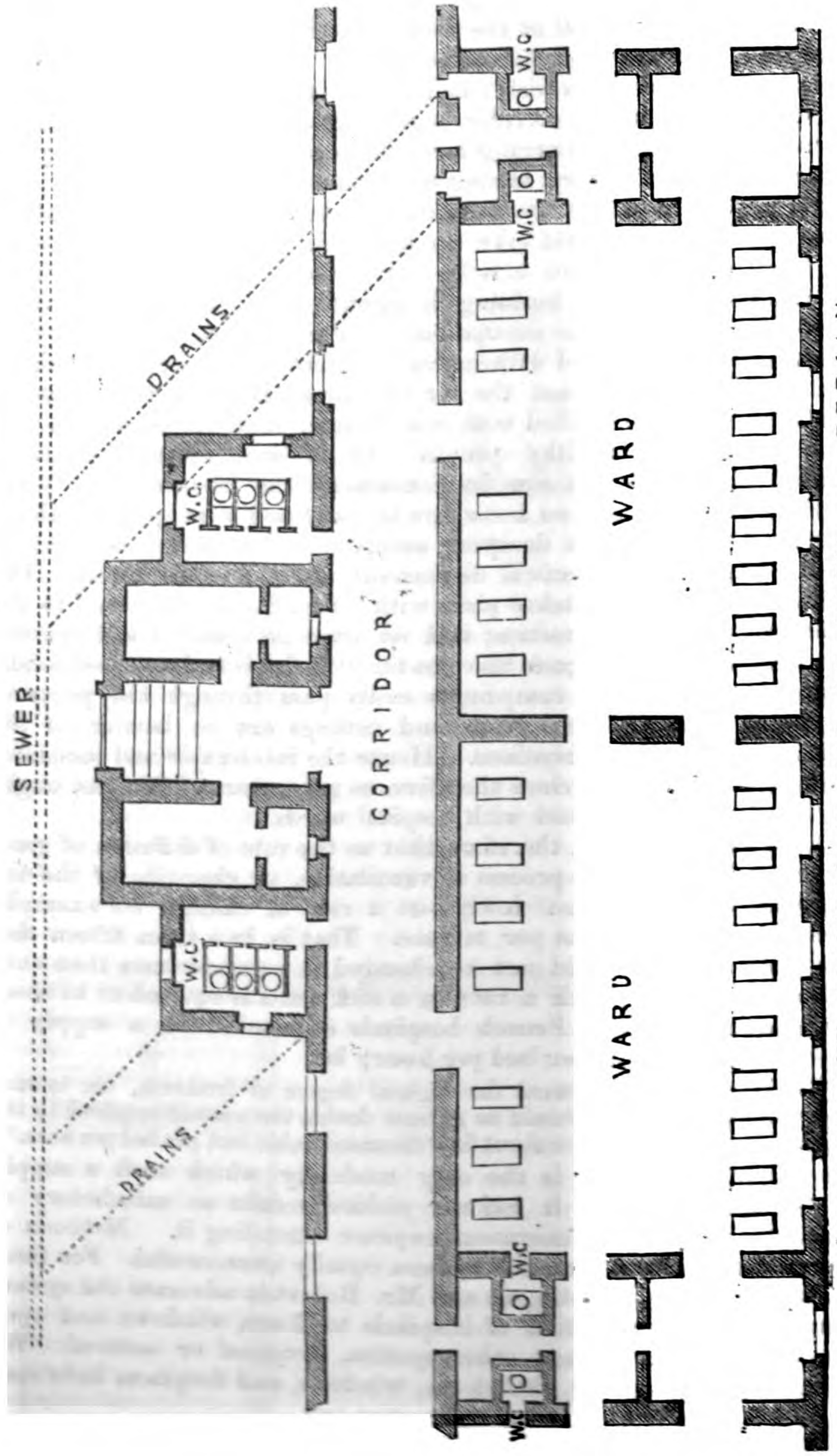
It has been supposed, therefore, that as the rate of diffusion of gases is fixed and certain, the process of ventilation, or changing of the air, of a ward ought to go on slowly—at a rate of change, for example, equal to one vertical foot per minute. That is, in a room fifteen feet high the whole air should not be changed at a greater rate than once in fifteen minutes. Such a rate in a sick ward is equivalent to absolute stagnation. The French hospitals contracted for a supply of 2220 cubic feet of air per bed per hour; but

"To give the air of a ward the highest degree of freshness, the amount of air passing through it should be at least double the amount required by the French hospital contracts, or about four thousand cubic feet per bed per hour."†

Artificial ventilation is the only mode by which such a supply can be obtained; but it has not yielded results so satisfactory as to counterbalance the enormous expense attending it. Methods of *natural* ventilation have hitherto been equally unsuccessful. For these reasons both Miss Nightingale and Mr. Roberton advocate the system of trusting the ventilation of hospitals to doors, windows, and open fires, rather than to any other system, *artificial* or *natural*. We believe, on the contrary, that doors, windows, and fireplaces have each

* The Builder, Sept. 1857.

† On Artifical Systems of Ventilation, by Dr. Sutherland, appendix to Sanitary Commission, p. 457.



NEW SUMMER HOSPITAL AT THE CHARITE——BERLIN

Fig. 17.

special functions to perform, altogether apart from their use as apertures either for the egress or ingress of air. Besides, it is undeniable that there are occasions during the day, as well as during the night, during summer, as well as during winter, when open windows and doors are unbearable. There are also places—such as staircases—referred to by Miss Nightingale (p. 96), and which, she says, are to be ventilated from the roof. Moreover, sick wards require more air than any other inhabited place, and therefore some auxiliary method or system of ventilation is absolutely required, besides doors, windows, and fireplaces.

We have every reason to believe that the benefits of ventilation may be fully secured to wards and apartments as well as to staircases, by an arrangement at once natural, simple and efficient, which does not depend on the existence of doors and windows, and with the efficient action of which the presence of a fireplace or a fire does not in the least interfere, (as we have often seen it put to the test,) but rather accelerates its action, just as the presence of human beings increases the demand for fresh air. We have neither time nor space to describe this method, which has been patented by Mr. McKinnell, of Glasgow, and already fully described in detail in the pages of several of our medical contemporary periodicals.*

We feel that the subject of hospitals has met with but imperfect justice at our hands; but if we succeed in fixing the attention of our profession on the important hygienic considerations which we have attempted to develop, we shall have gained our end.

We think the country and the profession owe much to Miss Nightingale for her earnest and determined advocacy of the principles we have here detailed. Were it not for the publication of her 'Notes on Hospitals,' the valuable material contained in the parliamentary Blue-book would have been lost to the public at large. In the regulations just issued by the War Office—the last of the works noticed at the head of this review—we think we can also trace ample evidence of her handiwork in the very clear and able rules under the head of "Hospitals" at p. 40, and the "Regulations for Nurses" at p. 122.

We must apologize for abridging the title-page of this book, for the title is really not one by which the book can be conveniently named. The title-page represents, in truth, a tabular view of the contents of the whole volume. It is one of the important results which have followed the Report of the Royal Commission already noticed; and looking to its contents, it may surely be fitly and concisely entitled, 'Regulations Affecting the Sanitary Condition of the Army.'

With reluctance we leave the subject, and appeal to those who have power and influence, to use that power and influence in the direction we have indicated, for the purpose of remedying hospital defects, and preventing their existence from the first. To architects, to hospital committees, to civil and royal engineers, to our professional brethren, and to all men of science and benevolence, of whom our country may be so justly proud—to one and all we commend the subject of Hospital Hygiene.

* Medical Times, May 1st, 1848; Sanitary Review for 1848.

Review II.

1. *Lectures on Pathological Anatomy*, delivered at Guy's Hospital during the Summer Sessions of 1857-58. By Samuel Wilks, M.D. London, Fellow of the Royal College of Physicians, Assistant Physician to Guy's Hospital, Lecturer on Pathology, and Curator of the Museum, &c. &c.—*London*, 1859. pp. 472.

2. *Descriptive Catalogue of the Preparations in the Museum of St. Thomas's Hospital.* Printed by Order of the Governors. Vols. II. and III. *Pathological Anatomy.* Completed and Edited by Sydney Jones, F.R.C.S., M.B., Curator of the Museum, &c. &c.—*London*, 1859.

We have here before us two works, the latest emanations from those immense repositories of science and experience—the hospitals of the Borough. Moreover, the works in question are constructed with special reference to the museums of Guy's and St. Thomas's Hospitals. They may, therefore, be fitly reviewed together. But as the one is a mere "Descriptive Catalogue," and makes no pretension to originality, either of design or of execution, we may dismiss it in comparatively few words. It is in every way creditable, so far as we have been able to go into the examination of it, to the industry and skill of Mr. Jones. At the same time we are bound to confess that we have seen catalogues in which it was more easy to find one's way. We would point particularly to the admirable pathological catalogue of the Royal College of Surgeons, and the very similar one of St. Bartholomew's Hospital, as presenting very great facilities to the reader in the way of synoptical arrangements in each section, and we are rather sorry that Mr. Jones has not seen his way to something of this kind.

On the whole, however, we are well satisfied that St. Thomas's Hospital has, in this work, obtained what has been long wanting—a satisfactory guide to the accumulated treasures of a museum which, if not as rich as that of Guy's, is beyond all doubt a collection of great interest, containing, as it does, not only specimens in every department, but a considerable number of memorials of the works of that great surgeon, Sir Astley Cooper. We congratulate Mr. Jones on the completion of the three years' task which devolved upon him after the retirement of Mr. South from museum work.

The magnificent museum of Guy's Hospital, which has furnished forth so large a portion of the materials for the works of Bright, Hodgkin, and others, has found a new kind of illustration in the lectures of Dr. Wilks. These, however, as being founded to a considerable extent also on original investigations, will command more of our attention at present.

It is hardly necessary to say of a book by Dr. Wilks that it is an exposition, to a considerable extent, of personal observation, and that the opinions expressed are worthy of careful consideration. The rich stores of pathological material already accumulated, or in the course of accumulation, at Guy's Hospital, have in this work found a careful and zealous recorder; and neither has the observing eye nor the constructive intellect been wanting to the task of framing a consistent whole out of these multitudinous fragments. Yet we do not deny that, good as this book is, we should like to see it even better, and we therefore make no apology for beginning with a somewhat large criticism.

The first thing that strikes the reader on turning over even a few pages of this work is that it seems to be addressed to but a narrow circle of readers. Not that this is borne on the title-page of the work, or is anywhere directly stated, but that the whole style and manner, and to no small extent the materials also, bear reference to a small audience seated within the precincts of the museum at Guy's, and handing round to one another specimens illustrative of the points insisted on by the lecturer. Here, for instance, is an entire paragraph of the work, extracted from p. 83, the usefulness of which to the general public seems rather more than doubtful:

"Melanosis of the heart is seen in this specimen (1400^{25})."

Throughout the book there are innumerable references to preparations in the hospital museum, and too often the descriptions are plainly intended to be supplemented by the specimen; nay, in various instances we have been provoked by finding questionable doctrines supported by reference to preparations of which no adequate description is given, and which our opportunities do not afford us the means of consulting in person without a considerable amount of trouble. The book, in short, is so framed, not that "he who runs may read," but that he who reads must run to Guy's Hospital if he would read to good purpose. Now, a catalogue of a museum is a very good thing, and a systematic treatise on pathological anatomy is a very good thing; but an attempt to combine the two cannot fail to lead to disappointment. An occasional reference to a particular preparation in illustration of an uncommon or doubtful fact, accompanied by a full detail of all the circumstances necessary to convince the reader of its bearing upon the subject in hand, is indeed not only unobjectionable, but highly desirable; but the perpetual iteration of references to things unseen is not only a severe trial of our patience, but in some respects of our faith also.

Dr. Wilks has been led into this peculiar fashion of writing, no doubt, by the prevalent mania for "lectures." He says, indeed, in the preface to this volume, that the original design of it was merely "a syllabus, accompanied by references to certain typical preparations in our museum," and that, by the solicitations of his students, he was induced to send the whole of his lectures exactly as delivered. Now, it was natural enough for the students to wish to see in print the very lectures by which they had been instructed; and yet it was fitting that, in addressing a larger audience, Dr. Wilks should have recast his conceptions so as to adapt them more perfectly to the end in view. The same mistake is constantly made in the matter of "Clinical lectures" by persons who forget, or who are not aware, that really good clinical lectures are almost always unsuitable for reading, while thoughts thrown together in a form fitted for reading are almost of necessity divested of the peculiarities of a clinical lecture. The same rule applies with perhaps still greater force to demonstrative lectures on preparations. The matter contained in this book deserved more attention to the form, and we trust that Dr. Wilks may soon have an opportunity, in a new edition, of making it more suitable for the general public.

Meantime, let us seek, almost at random, for a few of the more important doctrines of morbid anatomy as taught at present in Guy's Hospital.

The first part of the work is devoted to an examination of the anatomy of the different systems and organs in disease. The second part contains an account of "Associated morbid conditions and essential diseases;" in other words, of the morbid anatomy in detail, of particular forms of disease, especially in the case of constitutional affections. The latter is perhaps the most interesting and valuable part of the work, and contains a very good and extremely condensed summary of the author's personal observations on many important subjects.

Of *abscess of the brain*, it is observed that "it is Dr. Gull's belief that a primary cerebral abscess seldom or never occurs." It would have been desirable that this conclusion should have been justified by more details of evidence, as it will probably be doubted in some quarters, though quite in accordance with our own experience. We have found syphilis to be, next to tubercle, pyæmia, and external injury, the most remarkable cause of cerebral abscess.

Two cases only have occurred to Dr. Wilks during the last two years in which apoplectic symptoms were unaccompanied by morbid phenomena (simple apoplexy of older authors). He correctly directs attention to the state of the kidney, as furnishing in many cases the explanation of such symptoms. In delirium tremens, epilepsy, and chorea, there are no constant morbid appearances; it is worth while, however, to observe that Dr. Wilks has seen several fatal cases of chorea, and that in one of them there was a softening of the spinal cord. It is further very remarkable that Dr. Wilks has always found morbid changes in the heart. "In *all* the fatal cases of chorea which have come under my own notice (*query*, how many?), a small row of bead-like vegetations has existed on the mitral valve." This point is one of great interest, and deserved to have been stated more exactly and in greater detail. The account given of the morbid appearances in tetanus is very much in accordance with general experience, and is of considerable interest. There is no spasm of the heart, which is found, as in death by asphyxia, with the right side loaded; the brain and spinal cord are commonly normal; the rigor mortis is not greater than usual, though the muscles are sometimes found ruptured by the violence of the spasms. Dr. Wilks has seen only one case of hydrophobia; the appearances on dissection revealed nothing very unusual in that disease.

In the descriptions of the morbid associations of insanity and of heart disease, there is nothing particularly worthy of notice.

Dr. Wilks justly remarks that acute affections of the respiratory organs, though very common as diseases, are rarely, when uncomplicated, causes of death. Bronchitis, in particular, is commonly only fatal when an acute attack supervenes upon the chronic condition, or when this latter proves the fatal termination of another disease, such as morbus Brightii. Pneumonia, too, when fatal, is very commonly a secondary affection. This is in accordance with our own experience, and we suppose with that of most of our hospitals, though hardly with the Registrar-General's returns, in which pneumonia, and still more bronchitis, figure as contributing more largely to the mortality than most other diseases.

The lesions associated with Bright's disease are the subject of some interesting though brief remarks. Dr. Wilks distinguishes two great divisions among these diseases of the kidney, "one which is essentially chronic, and often of many years' duration, while the other has a much briefer history, and is connected with dropsy; the former may be found with various other degenerative changes in the system, while the latter constitutes the principal disease." In the dropsical form the kidney is large and pale; the immediate cause of death is usually an acute serous inflammation, or pneumonia, or bronchitis (the latter never absent); œdema of the glottis may also occur, but is seldom the sole cause of death. In the other form the kidney is granular, often contracted; death is frequently by uræmic intoxication, and in such cases there may be nothing found after death but an atrophied state of the kidneys. More commonly, however, there is associated with this a diseased state of the arteries, and hypertrophy of the left ventricle of the heart, and death may occur through various serous inflammations, pneumonia, bronchitis, softening of the brain, or sanguineous apoplexy.

In appreciating the remarks in this part of the work on the much-vexed subject of Bright's disease, it is necessary to keep in view that Dr. Wilks holds a sort of middle course between the doctrine of *one* lesion in Bright's disease, as maintained by Frerichs, and that of *many* different forms, as held by some modern observers. The views maintained by Dr. Wilks in this and other places can hardly be fairly brought under discussion at present; they are, however, deserving of all consideration, both as being tolerably simple in themselves, and as being in many points in accordance with the opinions entertained by Dr. Bright. We entertain little doubt that there is, as Dr. Wilks supposes, an atrophic form of Bright's disease in which the kidney is never enlarged, and a form of enlargement in which the organ has little tendency to become small (or, at least, is usually fatal without becoming small); and we think it is indisputable that in the latter form the essential lesion is an intra-tubular exudation which loads the organ with accumulated fibrin, and other morbid products that are with difficulty excreted. It is evident, too, that in the forms with great enlargement the disorganization is more rapid than in the others; nevertheless, we must say that, after all that has been written on the subject, we are by no means convinced of the *essential* distinctness of the atrophic and hypertrophic forms in all cases. We believe, too, that they often occur in succession in the same case—i.e., a hypertrophic process is followed by an atrophic; as also occurs in cirrhosis of the liver, to which Dr. Wilks (we think rightly) compares certain forms of Bright's disease. The subject is one of great difficulty, and we cannot say that any of the general doctrines hitherto put forward on Bright's disease has carried complete conviction to our minds. Dr. Wilks has at least the merit of clear and intelligible description, and on one point he has opposed himself to what we have long regarded as a great error—the recognition of the fatty kidney as being (*per se*) a form of Bright's disease. This error appears in Gluge's early memoirs on the subject; but it received an immense impulse from the well-known paper of Dr. G. Johnson in the 'Medico-Chirurgical Transactions,' and has never, we think, been properly dealt with by that observer. Fatty and granular deposits in the renal epithelium are, we think, an *accident* of Bright's disease; but in no case do they form the essential lesion on which the recognised symptoms of that disease depend. On the contrary, we have seen the highest degrees of "fatty kidney" without a single symptom or pathological character of Bright's disease, commonly so called. We quite agree with Dr. Wilks (and, we may add, with Frerichs) as regards some cases, at least, of Bright's disease with fatty changes in the tubules, that these are

"Altogether a secondary process, and due to a change or degeneration of the inflammatory products; and therefore the term fatty, as applied to the essential nature of the disease, must be as erroneous as styling a cancerous tumour or a phthisical lung fatty, because both contain an abundance of oil." (p. 369.)

The account of *Pyæmia*, and the cognate subject of *Puerperal Fever*, is such as to show that Dr. Wilks has given careful consideration to each subject; but here, as elsewhere, we find that the results of complicated scientific inquiries are displayed with rather too great conciseness, and controverted points are disposed of too quickly, to make the work a sufficient guide to the student of modern pathology. At the same time we are bound to confess to a general agreement with the author, and to observe that his statements of facts are for the most part unexceptionable. The same remark extends to many of the other subjects treated of in this part of the work, the most important of which, perhaps, is syphilis, on which Dr. Wilks has some personal observations worthy of consideration. But as these have been fully recorded in the 'Pathological Society's Transactions,' we shall not detain the reader with them at present.

We take up a few other points scattered through the remainder ot this book, very much in the order (or rather *no* order) in which we have noted them in the course of our somewhat desultory readings.

Pancreas.—Dr. Wilks seems to us unduly sceptical about the existence of cancer of this organ. From our own observation, we should say that an unquestionably cancerous form of disease is not unfrequent and certainly more frequent than any disease of the nature of "cirrhosis," to which the author refers as often mistaken for cancer. We observe also with some surprise in a work emanating from Guy's

Hospital, that no allusion is made to the remarkable observations of Dr. Bright tending to connect disease of the pancreas and duodenum with the peculiar symptom of fatty evacuations by stool. The fact that the connexion is not constant, nor even frequent, does not diminish its importance when it does occur.

Waxy degeneration.—The author, like all the more accurate modern observers, distinguishes the "lardaceous" or "waxy" state of the liver and other organs from the fatty, which has been confounded, and is occasionally associated, with it. We are far from considering his descriptions, however, of these lesions as up to the mark. The leading idea is that the waxy deposit is an adventitious matter more or less closely allied to infiltrated tubercle. We believe it neither to be an adventitious matter, nor to have any close alliance with tubercle, further than being frequently present in tubercular subjects. It is incorrect, also, to speak of the waxy matter as "a translucent pearly substance *occupying the tissue, and having no structure.*" (p. 322.) In the great majority of cases, and especially in the liver, to which this description more especially refers, the tissue affected with the waxy degeneration may be seen under the microscope to be actually transformed, not merely displaced by the deposit; which is thus, in the most absolute sense, a *degeneration*—an alteration, namely, of the normal tissue-elements, and especially of the albuminous tissues, by which they undergo chemical change, preserving to a considerable extent their anatomical forms. We recommend Dr. Wilks to reconsider his views on this subject. He has, however, very correctly stated the general characters of the waxy liver, and has especially alluded to the contrast between it and the fatty liver in respect to specific gravity, a point frequently omitted by authors. Like Dr. Wilks, we have found the waxy liver in some instances upwards of 1080 in density; while the fatty liver approaches the specific gravity of water, or may be even lighter. The recent observations on the connexion of the waxy and "amyloid" degenerations are hardly alluded to by Dr. Wilks.

We notice a singular error in p. 302, in which diphtheritic inflammation is apparently confounded with dothinenteritis! We trust this is only a slip of the pen; but there are other indications that Dr. Wilks has not been a particularly careful reader of French pathological anatomy, especially in p. 239, where he evidently confounds miliary tubercle with the so-called "crude tubercle" of Laennec; a totally unaccountable mistake.

Another subject very imperfectly treated is that of spurious melanosis of the lungs. The observations of Wm. Thomson, and the more recent speculations of Virchow, on this subject, have apparently not fallen within the author's reading. To the former, indeed, there is something like a reference at second hand, in connexion with a rather interesting case of a man in Guy's Hospital, who had worked "for years in the vaults of London Docks, where torches and candles are continually blazing," and who was admitted to the hospital labouring under bronchitis, with "very black" expectoration. We suspect that anything approaching to the expectoration of a genuine case of "miner's lung" is exceedingly rare in London; as, indeed, specimens of this form of disease are very rare in London museums. The occurrence of a case of this kind, therefore, in Guy's Hospital (and Dr. Wilks seems to intimate that there has been more than one in his experience) deserved, we think, a more detailed statement of the facts than we find in this place.

On the important subjects of *Pericarditis, Endocarditis, Myocarditis,* and *fibrinous obstructions of the vessels* (Embolie of Virchow), Dr. Wilks shows a laudable anxiety to state the whole of the facts, and neither to luxuriate in theory nor to abstain from just and fair inferences. This part of the work is one of the most instructive; it is characterized throughout by the careful and unostentatious use of large stores of valuable information. In regard to myocarditis, the author supposes (like Dr. Stokes) that the muscular substance is frequently diseased in cases of rheumatic pericarditis and endocarditis, and that this is the cause of those sudden deaths that occur in rheumatism, as well as of some of the enlargements of the heart which follow that disease, without the intervention of pericarditis or of valvular affections. It is rather difficult, of course, to get over the fact, that in most of the sudden deaths there seems to have been no distinctly visible morbid affection of the muscular tissue; on the other hand, it is argued with truth that in the hearts of rheumatic subjects we occasionally find chronic lesions of the muscular fibre accompanying old pericarditis and endocarditis; and further, that in recent pericarditis the neighbouring muscular tissue is frequently granular. The whole of this subject merits more attention than it has received from the majority of writers on cardiac disease.

In his descriptions of the diseases of the pulmonary tissue, we agree with the author to a considerable extent. He correctly recognises the essential identity of the condition of the lung in infants that have not breathed, or in whom the lung has expanded imperfectly (atelectasis), with that which is sometimes produced as a secondary condition (the *état fœtal* of Legendre and Bailly). But while he fully admits the fact brought to light by these eminent and well-known observers, he fails to follow it out to its legitimate conclusions in pulmonary pathology. Thus we have no hint given, under the head "Lobular Pneumonia," that the great majority of the cases so called have been cases of the *état fœtal;* all the controversies, and all the real information, which have been accumulated on this subject are passed by without a word, and Dr. Wilks absolutely treats the lobular pneumonia of children as if it was an affection of the same class and order with the metastatic abscesses of pyæmia! Similar remarks apply to vesicular and catarrhal pneumonia, to typhoid pneumonia, and to peripneumonia notha. In all these cases the author fails to apprehend an important morbid relation which recent researches have very clearly established. In fact, we have a shrewd suspicion that he has really not read with any degree of care either the memoir of Legendre and Bailly, or the admirable descriptions of these conditions of the lung in the works of West, and of Barthez and Rilliet, which are surely to be found somewhere about Guy's Hospital. It is only, at least, on the theory of great want of attention to the original sources of our information on this subject, that we can understand so great an error as the ascribing of Legendre and Bailly's discovery to Dr. Alderson (see p. 219). No one would willingly detract from the credit due to Dr. Alderson for his truly admirable memoir on the morbid appearances in hooping-cough. The accuracy of his observations in matters of detail is unquestionable, and they have been fully corroborated by Dr. Graily Hewitt, writing with all the light of modern experience. Dr. Alderson is entitled to the praise of having described accurately, although neither he, nor any one else at the time, had any conception of the full significance of his descriptions. In no sense, however, can Dr. Alderson be correctly said to have solved the riddle of the *état fœtal.* That was reserved for Legendre and Bailly; and by giving their credit over to Dr. Alderson, a wrong is done to both parties.

On the subject of *Emphysema of the Lungs*, Dr. Wilks rather avoids giving a decided opinion. He appears to be favourable to the views of Dr. Gairdner, as to the inspiratory origin of this lesion in connexion with partial collapse of the lung, in so far at least as to adapt the objections urged by Dr. Gairdner to all the older theories; but he gives almost equal ear to Dr. Jenner, who, as is well known, maintains under a new form the expiratory theory as applicable to some, at least, of the cases of this disease. The arguments upon both sides are stated carefully and impartially, though too briefly and with too little emphasis to make much impression on those who may read them here for the first time.

Such are some of the merits, and such the defects of this work, as they appear to us to demand notice. We have spoken freely of the latter, because we entertain a high opinion of the former. Dr. Wilks is eminently a student of nature, and he gives ample evidence in these pages of the earnestness and simplicity of purpose with which he has applied his mind to the immense mass of materials which his position has furnished to him. Nothing but the most sincere appreciation of the services which he has rendered, and the assurance of those he may yet render, to science, would induce us to speak of errors and omissions where there is so much to commend. Good books are not too plentiful among us, and we should do great injustice to our own sense of what is fitting were we not to say that this is emphatically a good, a useful, and, above all, a thoroughly honest book. That we should like to see it improved in form, we have already said. That we should wish to have its deficiencies corrected in detail, we need not say. That it will in any new edition require the addition of a good and complete index, is only what may be said of every book, and especially of one like this. But above all, we hope to see Dr. Wilks emancipate himself more and more from the somewhat narrow and local feeling under which he writes. Let him always have it clearly before him that he is a member of the great community of science, and not a mere "Guy's" man, living in and for the hospital and museum, and the well-known "Reports." He has accumulated a vast store of experience; to make it available to the full, he must enter into communication, more widely than hitherto, with the great minds of the present and of the past, and make it his aim not to reflect or to rule the ideas of a coterie, however distinguished, but to take the world for his audience, and to say to it what is worthy of being said to all. If he be determined, therefore, to write a students' book, let it, in the next edition, be a book for *all* students whatever. The present book goes over too much ground, and goes over it imperfectly. Still, it is, as we said before, a good book, and cannot fail to repay a careful perusal.

Review III.

1. *A Practical Treatise on the Diseases of Children.* By D. Francis Condie, M.D., Fellow of the College of Physicians, &c. Fifth Edition, Revised and Enlarged.—*Philadelphia*, 1858. pp. 762.
2. *Lectures on the Diseases of Infancy and Childhood.* By Charles West, M.D., Physician to the Hospital for Sick Children, &c. &c. Fourth Edition, Revised and Enlarged.—*London*, 1859. pp. 754.
3. *Third Report of the Clinical Hospital, Manchester: containing Results on Physical Development, Hooping-cough, and Transmitted Diseases.* By James Whitehead, M.D.—*London*, 1859. pp. 117.

Two well-known works upon pædiatrics are before us: the most reputable American and English treatises upon an important speciality—viz., Dr. Condie's 'Practical Treatise,' and Dr. West's 'Lectures upon the Diseases of Infancy and Childhood.' The former has attained to its fifth edition within fifteen years, and the latter to its fourth issue within the space of twelve. This alone is sufficient proof of the value of the labours of these two physicians, and of their appreciation by the public. The books in question differ in some points, however, very materially from each other, and each has its special claims to notice. Dr. Condie aims at an encyclopædic summary of the general pathology of, and all the "medical" diseases invading the child, together with some surgical and congenital affections and accidents occurring within a month after the birth. Dr. West confines himself chiefly to a consideration of the more important diseases of the nervous, respiratory, circulatory, and digestive systems. Dr. Condie builds up his separate articles with some matter of his own, and with much assiduously gathered from other people; Dr. West mainly relies upon the results afforded him by 900 personal observations and 180 post-mortem examinations made amongst nearly 30,000 children within the last twenty years. Dr. Condie gives us many valuable and interesting memoranda upon all sorts of the misfortunes of infancy, whilst Dr. West occasionally presents us with a lecture upon some special malady, which is fresh, lifelike, and masterly in its way. We have a legion of scraps of useful knowledge from Dr. Condie upon more topics than we can mention, though we remember that cutaneous diseases, purulent ophthalmia, hydrocele, burns and scalds, and such like items, occur amongst their number. From Dr. West we have nothing of this latter kind—he ignores it altogether, but gives us, e.g., memoirs upon paralysis, disorders of the mind, and the like, which leave Dr. Condie a hopeless competitor. The American practitioner may be thankful that he has ready at hand such an epitome of knowledge concerning the diseases of early life as is Dr. Condie's 'Treatise,' whilst the American student must be warned of certain of its doctrines, which, if put in force to their letter, will lead him astray. The English student and practitioner may rely upon what Dr. West chooses to tell them, whether as regards theory or practice, though on some points they may occasionally wish to know a little more. The chief faults of Dr. Condie are first, that he is not an impartial writer; and secondly, that the *therapeia* he recommends is by far of too violent and interfering a character. That he is but a partial witness to the truth, we have his own admission, when telling us that "he has made use of the labours and adopted the opinions of others only when he has found them to correspond with or to be confirmatory of the results of his own observations and inquiries." (p. v.) This is a one-sidedness which, however satisfactory to an author, is not exactly suited to a reader of inquiry and judgment. Of the many sides there are to a question, it is surely not expecting too much for the latter to desire some knowledge about two of them. In the next place, the general rule of treatment advised by Dr. Condie is, in our opinion, even dangerously heroic—so heroic, indeed, occasionally, that we can scarcely believe he himself is accustomed to follow it. If he really does do so, then all that we can say is, that American children must have capital constitutions. We are not afraid of leeches, calomel, antimony, &c., nor do we look upon either expectancy or brandy-and-water as the veritable and universal therapeia; but really the author's system of depletion—his leeches, his calomel, and his blisters, are of so universal application and so vigorously employed, as to lead us to caution the young practitioner not to follow him too blindly. It matters not whether it be "inflammation of the brain" or "scarlet fever," "enteritis" or "diphtheria," "pneumonia" or "coryza," loss of blood is deemed advisable by Dr. Condie. And so of calomel; croup, gangrenous angina, apoplexy, erysipelas, gastritis, indigestion, &c., require calomel. It is true that in advising blisters, Dr. Condie cautions the reader not to allow them to do more than "produce a general redness of the skin." Now, this advice in actual practice, especially amongst the poor, is rarely attentively followed by the nurse or attendants. If not followed, and a sore be produced in the young child, in nine cases out of ten amongst the children of the class alluded to, the remedy will be worse than the disease. We therefore caution the student most particularly against heedlessly applying cantharides plaster to young children. "Blisters are remedies," says Dr. Condie, from which much advantage will be derived in most of the cases of pneumonia occurring in children." (p. 310.)

"It may be well (writes Dr. West) to offer a caution with reference to the employment of blisters—a measure to which we often have recourse with advantage during the resolution of pneumonia in the adult, but which as a general rule is not advisable in young children whose lungs have been solidified by disease." (p. 334.)

From what we ourselves have seen, we know not of any recommendation we should feel less desirous of following than that contained in the following remarks:

"Among the local remedies which appear to be the most generally applicable to erysipelas occurring in children, is a blister sufficiently large to extend over the diseased surface, and for a short distance beyond it; after vesication has been produced, the serum should be evacuated, and the vesicated surface dressed with fresh lard." (Condie, p. 568.)

As to what Philadelphian infants may bear we may be said to have only a theoretic idea, but of what the London and its suburban juvenile population have a tolerance we know from a very fair practical experience. This forces us to assure Dr. Condie that the therapeutic procedures he would seem to follow with such advantage in Pennsylvania, would often destroy his patients here, were he to change his quarters and practise amongst us. But we suspect, after all, that our author is not exactly a trustworthy guide even for young and vigorous America. Our knowledge of other transatlantic writers upon the diseases of children leads us to regard Dr. Condie as exceptionally heroic and interfering in his ways, notwithstanding our American friends generally are somewhat bolder than we are in the therapeutics of children. Moreover, our attention has lately been called to an accusation of Dr. Parker, of New York, against his countryman—viz., that he has described sixty-eight diseases, and has recommended the employment of calomel in fifty-one! That Dr. Condie's 'Treatise' contains a great deal of various and valuable information we willingly allow, but that portions of it require to be studied with great caution, we must equally maintain.

The new issue of Dr. West's 'Lectures' exhibits such additional changes and clinical illustrations as must continue to keep them in advance of all information connected with the speciality of which they treat. We refer of course to works in the English language, and in so far as a scientific exposition of certain diseases of childhood, based upon personal experience, is concerned. To say—as we have heard it disparagingly observed—that Dr. West's book does not allude to surgical, cutaneous, and some other diseases, and is therefore disappointing and incomplete, is to totally misconceive the scope and intention of the 'Lectures.' Those who will bear these in mind cannot fail of being amply satisfied with the way in which they, as inquiring readers, are treated. That a large circle of such persons has been so requited, we have sufficient proof in the facts of the third edition having been for more than a year out of print, and of the fourth now being upon our table. As the work at present stands, we would direct particular attention to the lectures upon "paralysis," "night terrors, disorders of the mind, and idiocy," "diphtheria and croup secondary to measles," "diseases of the heart," "typhoid fever," &c.

Dr. Whitehead's third report contains many observations of an interesting and practical character. We may draw attention in the first place to the following remarks relative to the offspring of the more dirty of the lower orders:

"A maculated state of the skin is frequently met with in the children of this class of people, consisting of small circular spots the size of face-freckles, not, however, occupying the face and hands as freckles do, but every other part of the body except the feet. They seem to infest parts covered by the clothing, while freckles are only seen on parts exposed. They have a purple colour like the maculæ of typhus—not a pale yellow like freckles, and are unassociated with any form of disease saliently expressed, but they seem nevertheless to indicate a depraved habit of body. The skin has a sickly opaque colour, the flesh is flabby, the temper fretful, and the energy subdued. The parents appear to regard their presence as natural and unavoidable, as they are never the subject of treatment, nor is attention directed to them when the body is examined for other purposes. They seem to be peculiar to the lowest classes, and are caused by sloth and personal neglect, as they are never met with in the offspring of the cleanly and thrifty. There is no doubt that this *macula cachectica* is in reality a disease of the skin due proximately to interrupted cutaneous transpiration. . . . . In such persons the spots which appeared in infancy are often seen up to adult life." (p. 15.)

We have several tables and other details connected with the "teething process," worthy of attention. From these it may be deduced that in children born at the full time in healthy localities, of untainted parentage, and properly nourished, the teething process will generally commence between the fifth and eighth months, that at the age of fourteen months, ten or more teeth should have been cut, and that six teeth are the minimum number compatible with good development and favourable prospects at that age. In two children of bad development, dentition was not accomplished until the thirty-ninth and forty-eighth months respectively. We have at this moment a little patient who is thirty-four months old, and has cut only four small incisors. This child is rachitic. Precocious dentition, or the irruption of the first teeth about the third month, does not appear from Dr. Whitehead's statistics, to offer much promise for the favourable progress of after development. Further dentary evolution is often arrested, the child remaining backward in *general* development: on the contrary,

"The precocious irruption of *all* the twenty teeth is the constant attribute of an excellent state of development. Not a few of the children who accomplished the teething process at sixteen or eighteen months, or earlier, were able to walk freely at nine months, and were exceedingly strong in all their physical faculties." (p. 26.)

Dr. Whitehead regards the period at which the power of walking becomes fairly established as the most trustworthy index of a favourable or unfavourable developmental condition, whilst the loss of this faculty after it has once been established (especially if this be associated with impairment of the general tone, and not due to acute disease), is always one of the first palpable signs of an incipient check to the process of development. In the 'First Report of the Manchester Clinical Hospital' it was stated, that the anterior fontanelle appeared to have the largest dimensions at from five to seven months of age, and not at birth, as usually supposed. This statement is here retracted, as additional inquiry has shown that it was founded upon insufficient and mixed data. The same Report also gave thirteen months as the average age at which the fontanelle closed in children of good development, but more recent data extend the period to fourteen months and a half.

"As a rule, a child of good development, with closure of the fontanelle at fourteen months and a half, has usually at the same time (or ought to have) about fourteen teeth, and has been able to walk firmly several weeks or months; while in one having at this age the fontanelle largely open, it frequently happens that not more than two to six teeth have appeared, and he is unable to walk; and even at the age of two years, when the teething process should be completed, the fontanelle being still open, there are generally not more than eight to twelve teeth." (p. 30.)

The settlement of few questions would be more advantageous to the public than that of the performance of vaccination possibly subjecting the patient to receive, along with the vaccine lymph, some other virus afforded by a diseased child. The popular belief of all kinds of bad influences being thus transmissible from one person to another has, from the time of Jenner, stood greatly in the way of an universal adoption of the practice of vaccination. It is true that we do not expect to hear at the present day much about the "moral iniquity of infecting the human system with the blood of a brute," and of such extreme consequences as of "the face beginning to resemble that of an ox," or of "patches of cow's hair" actually springing forth on the unfortunate vaccinated. Still, such violent antagonists as Messrs. Gibbs, Johnson, Carnot, Nittinger, and others, approach very near this mark in telling us (and often with not the most polite words) that vaccination is but another term implying "the deterioration and destruction of the human race, an opposition to nature, a cruel superstition, and much besides that is bad." It is not likely, however, that the general public will universally and quietly adopt the opinion that nothing but cow-pox virus *is*, or *can be*, transferred upon the blade of the inoculating lancet, so long as medical men of good repute are upon this point of a different judgment. We do not think that there can be a doubt but that by far the *majority* of the profession believe that nothing but vaccine virus, no other potential germ than this particular lymph, is transmissible by vaccination; though it is admitted that impure lymph has injurious consequences of its own, giving rise, on the one hand, to local irritation, bad sores, wounds, &c., and their consequences—and, on the other hand, to imperfect protection against variola. But this party would, we presume, coincide with Dr. Seaton, when he maintains that these latter events

"Ought really never to occur; and that which it specially imports the public thoroughly to understand is, that they are not the results of the due performance of vaccination, but the result of its improper performance, that they are perfectly avoidable, and that they constitute therefore a reason for care in the performance of vaccination, but no objection whatever to the operation itself."

There exists a minority, however, which either doubts, or vehemently denies, all this. The extreme of this school believes—e.g., that the potencies of syphilis, of spedalskhed, of rheumatism, of the exanthemata generally, of cutaneous affections, and even of the scrofulous and tuberculous dyscrasiæ, may be, nay actually are, constantly bestowed in conjunction with the vaccine virus. Whilst some, like Dr. Tinsley, of Cuba, affirm that the vaccine virus, after passing through the system of the negro, has been thereby rendered valueless for the protection of the white race; others laugh at such an idea, and ask if the vaccinious matter is inoperative when taken from the teats of a black cow! We have thus one party affirming that all sorts of terrible constitutional vices are liable to be conveyed from child to child by vaccination, whilst the other (like M. Taupin) asserts that whatever may be the disease from which a child suffers, and who furnishes the vaccine matter, the virus of the former forms no coalition with that of the latter. In respect to such dyscrasiæ as scrofula and tuberculosis, as Mr. Simon observes, it might as well be said that vaccination communicates a Roman nose or a landed estate. MM. Guersant and Blache,* referring to M. Taupin's experience, and the more than two thousand children it included, remark:

"The lymph obtained from children affected by acute and chronic diseases, by idiopathic fever—by the exanthemata—by thoracic, cerebral, and abdominal inflammation—by neuroses, such as chorea, hysteria, epilepsy, &c., was quite as active as if it had been borrowed from children who were quite well. It was followed by *vaccinia* as abundant and regular, and as preservative against variola, as in the latter instance; and it was equally well established that the virus did not transmit any malady, whether acute, chronic, contagious, or non-contagious. A great number of children affected by itch, scarlatina, measles, chicken-pox, varioloid, and variola, furnished a vaccine lymph which never communicated any of these inoculable diseases. The same circumstance held good in cases of rachitis, scrofula, syphilis, tuberculosis, chronic eruptions of the scalp, &c. &c. Not in any case—we designedly maintain it—did the virus communicate aught but *vaccinia*." (Op. cit.)

We may add to the above remarks, that M. Taupin has several times inoculated children with vaccine matter taken from others after their death, and that such inoculation was always unsuccessful with the patients, who at an after period were re-vaccinated with perfect success.

Now let us hear Dr. Whitehead:

"The occasional presence of eruptions on the skin, and other forms of disease, as an entailment, apparent or actual, of vaccination in a family not previously subject to such affections, undoubtedly operates in the minds of many very much to the depreciation of the procedure as a preventive and healthful measure; and certainly, in not a few instances there would seem to be just and sufficient reason for such prejudice. But the cause of this is not to be found in the vaccine virus in its pure state; it is due to a morbid material superadded, in its nature peculiar and extraneous. The noxious matter commonly conveyed by vaccination is the syphilitic poison. A child of naturally vigorous constitution, whose blood is tainted with the poison of syphilis, may retain the outward appearance of health up to three, six, or twelve months,

* Dict. de Méd., tom. xxx. p. 413.

or even to two or three years, or longer, before a characteristic outbreak shows itself. The parents of such a child may also have the semblance to superficial observation of faultless health, although still possessing the seeds of this malady in a degree sufficient for its transmission to their offspring. It is from such sources that mischief is often derived, and disseminated by vaccination and other modes of implantation; and it is thus that the efficacy of this great sanitary measure has been in many instances rendered questionable. . . . . . . This child, although at the period alluded to in an improving condition, was far from being cured; and the lymph in the vesicles, notwithstanding their healthy aspect, was probably so far charged with morbid principle as to be capable of infusing into the blood of any on whom it might have been implanted for the purpose of vaccination the syphilitic poison and all its consequences." (p. 53.)

In a journey lately made by Schiött* through the "Stift Bergen" of Sweden, the traveller found in some districts of the place more than one half of the inhabitants suffering from eruptive spedalskhed. Vaccination was performed without the least heed being taken whether the lymph was borrowed from an affected or from a non-infected person. Upon his return, Schiött brought the general subject of the transmission of morbid potencies in union with vaccine matter before the Medical Society of Christiana for consideration, as the Storthing was about to issue new regulations relative to the enforcement of vaccination through the country. Steffens maintained that the syphilitic poison might be conveyed by vaccination, particularly if the lymph was taken after the seventh day; but believed it was "not probable" that the virus of spedalskhed could be thus transmitted. Faye was of the same opinion regarding spedalskhed, and his own personal experience as respected syphilis, &c., was negative as to its propagation in the way mentioned. According to Boeck, we could not at present definitely adjudicate upon the matter either way; he admitted that cases did occur which seemed to speak in favour of the belief that specific poison germs, dyscrasic potencies, could be conveyed along with vaccine matter from one child to another, but we certainly were wanting in decided proofs of it. We may here observe that Mr. Marson, of the Small-Pox Hospital, has vaccinated more than 40,000 persons; and he states, that "he has never seen other diseases communicated with the vaccine disease, nor does he believe in the popular reports that they are ever so communicated; whilst Mr. Leese, of the National Vaccine Establishment, who has vaccinated perhaps more even than Mr. Marson, "has never seen struma nor any rash of any kind produced." Dr. Seaton tells us, too ('On the Protective and Modifying Powers of Vaccination'), that Mr. Marshall, late of Knighton, Herefordshire, who, with his predecessor in practice, has kept an accurate register of all vaccinations performed by them, from the year 1805 downwards, in 5439 vaccinations never met with anything of the kind; and observes, that in a country district—such as that in which Mr. Marshall practises—it is scarcely possible that any anomaly should have occurred without his being acquainted with it. Our own direct experience would entitle us to go only to the extent of

* Journal für Kinderkrankheiten, Band xxxii. s. 137. 1859.

affirming that we have seen some half-a-dozen cases of intractable cutaneous disease having their *point de départ* in the sores of vaccination, and which have widely spread, and plagued the patient for many years. This cutaneous disease approaches to the "impetigo rodens" of Willan and Startin, and has been considered by others, we suspect, as a variety of superficial lupus. That at one period of its course it is not a mere local malady of the integuments of the arm we are certain, as we have in some of the cases alluded to, known the disease to break out in circular patches upon the face so soon as we had quieted, if not arrested, the manifestation of it upon the extremity. Beyond this we could not go, and if, *theoretically*, we felt disposed to grant it as not impossible that the transmission from child to child of other constitutional miasms than those of vaccinia might be effected in vaccination, we must practically admit it as "not proven." Even if it were proven, the *trustworthy* evidence of mischief must be considered as so small in amount as to cause the deleterious results believed occasionally to ensue, not to weigh anything against the vast advantage which the performance of universal vaccination would bring in its train.

Dr. Whitehead gives us plenty of statistics connected with *whooping cough*, and discusses this malady somewhat in detail. We shall confine ourselves to his remarks in relation to treatment. His tables certainly go to show the error of the not unprevalent opinion that this disease is almost uncontrollable by treatment, and that change of air is the only thing likely to be of much avail. Dr. Whitehead maintains that if the patient be brought early under treatment, not only may the symptoms be moderated, and other contingent diseases warded off, but that the duration of the disease may be materially shortened. Cases that came under treatment within six days of their commencement, ran a course only of thirty-seven days; whilst others, most decidedly neglected, completed a period of one hundred and eleven. The remedies employed were, in the simple cases, or when the complicated cases had been reduced by other treatment to this condition, Dover's powder alone, or combined with camphor, camphor inhalations, emetics, belladonna, and local irritants, but always with either opium, Dover's powder, or belladonna as a principal remedy. Occasionally the Dover's powder was replaced by the tincture of opium, given in some aromatic water. The general modes are thus reduced to the *opium* treatment, and to the *belladonna* treatment; by the former, fifty-eight cases were treated, and terminated, on the average, in twenty-eight days from the beginning of treatment. Belladonna was used in seventy-six cases, the average duration of the treatment was twenty-two days. It was given

"In the form of powder of the leaves, never the extract, as this is an uncertain preparation; and sometimes in the form of solution of the nitrate of atropia. When in the form of powder, half a grain, mixed with five grains of sugar, was given to a child twelve months old, twice a day; then, after two days, if well tolerated, three times, then four times a day, or oftener, and in larger doses, being gradually increased until a specific effect was produced. The solution of the nitrate of atropia was prepared so as to contain one ninety-sixth of a grain in a tea-spoonful of the liquid; this dose of the salt is equal in its therapeutic effect to about half a grain of the powdered leaf, so that a tea-spoonful may be given twice or thrice daily to a child twelve months old. . . It is highly probable that were the belladonna treatment largely adopted in each case, and associated with suitable hygienic regulations, the duration of the disease might be reduced from its average of forty-two days, to that of twenty-eight or thirty days, and both its concomitant and consecutive accompaniments be materially lessened. (p. 90.)

We think it right, in connexion with the employment of belladonna, to inform our readers that we have found the phantastic delirium so characteristic of its power produced by four doses of a quarter of a grain each of the pulv. fol. belladon., given to a little girl two years and a-half old. It had little or no effect also in allaying the spasm of the glottis, though in the case of her brother, who was two years older, and was tolerating the drug well, we believed the belladonna to have been decidedly beneficial. Upon the whole, we should advise rather smaller doses to young children, and more caution to be employed generally in respect to the use of belladonna, than Dr. Whitehead appears to think necessary.

Syphilis is another malady which Dr. Whitehead always feels interested in discussing. We have in the report before us much tabular information concerning it. The author remarks:

"To the duration of this disease there would seem to be no limit. It has no tendency to wear itself out without mischievous entailment. It degenerates by time into forms of scrofula, tuberculosis, and glandular affections; into osseous degeneration, atrophy, hepatic disease, and dropsy, but does not disappear untreated without the infliction of some such disastrous consequences." (p. 106.)

That so insidious a malady as constitutional syphilis in the child is worthy of the most prudent and watchful therapeutic measures, few of our readers can gainsay. We hold it therefore incumbent upon such practitioners as have possessed more than ordinary means for arriving at a judgment upon the surest and safest method of cure, to make it known, and support it by all legitimate measures. We are particular upon this point because we very much fear it is being attempted by some whose experience is utterly valueless, to oppose such views of treatment as we believe to be the only true ones, and of which Dr. Whitehead himself is no mean supporter.

"Regarding the treatment of syphilis, much need not be said. An extended and continued experience goes still more strongly to confirm the belief already expressed elsewhere (on hereditary taints), that no remedy with which we are at present acquainted is at all comparable with mercury in its curative efficacy." (p. 107.)

As we are unhesitatingly of this opinion, and as we have arrived at it, like Dr. Whitehead, after what we consider sufficient warranty and experience—but not more than sufficient—viz., the observation of such numbers of children as a special institution alone can afford, we regard all statements similar to those expressed in a late correspondence in the Lancet (vol. ii. 1859, pp. 274, 302, 347, &c.), as unworthy of the least attention, or as of any weight in the attempt to change the treatment of infantile syphilis from the long-tried mercurial method. From the first, we had our suspicions that the writer who there (op. cit.) offers himself as the champion of 'Young Edinburgh,' knew but little about the particular subject we are at present discussing; we would not believe that if he and others had seen and treated syphilised infants and children to that extent which alone would authorize them to be dogmatic upon the matter, they would write of mercury in the terms which they have thought fit to employ. As Dr. Whitehead justly observes:

"Primary and other existing symptoms may be made to disappear for a time by other measures, and sometimes the first-named form, as to its external characters, will vanish without any remedial interference whatever, but the mere subjugation, for a season, of cutaneous phenomena, is no adequate evidence that the blood is free from taint. The symptoms are still liable to reappear after months or years, and during this interim of quiescence and apparent immunity, the poison remains in the system." (p. 107.)

"The ill consequences alleged to have resulted from the *proper* use of mercury are, in my opinion, altogether fabulous and imaginary. The effects of the disease, neglected or illtreated, have probably been mistaken for those of the remedy. Almost any remedy whatever, or even innocuous articles of diet, if used injudiciously, may be made to produce evil consequences, and no remedy is safe in the hands of the incautious." (p. 107.)

"The cases treated by non-mercurial remedies, and those in which mercury was insufficiently employed, exhibited a constant tendency to relapse, each return being as severe in its destructive tendency as the preceding one had been, and sometimes more so, although not always assuming the same form, nor uniformly attacking the same parts of the body." (p. 108.)

As the treatment of certain of the disorders of females constitutes a necessary part of the system of the Manchester Clinical Hospital, Dr. Whitehead does not forget to allude to such matters in his report. It is worthy of note, too, that whenever practicable, in cases of syphilis of women and children, and where the father is believed to be still affected, he has been requested to present himself, and, for the sake of his wife and family, to submit to treatment. By many men the offer has been thankfully accepted. From amongst the women, the cases selected for special mention upon the present occasion are chiefly those of metrorrhagia, and for the purpose of recommending an agent which Dr. Whitehead has found "eminently serviceable" in the treatment of uterine hæmorrhage. This agent is the *achillea millefolium*, or common yarrow, an indigenous member of the natural family "compositæ," and to be met with everywhere. It is a plant highly extolled by the old herbalists as "an excellent medicine in the overflowing of the menses, bloody fluxes, and bleeding piles." It was believed to have other virtues besides this, but of these our author is ignorant. Of the anti-hæmorrhagic virtues of the "yarrow" he has a high opinion; he has used it, he informs us, rather extensively in public and private practice for three years, and the results stated in the report go entirely to confirm those of previous trials. The drug may be employed either in the form of tincture or of decoction.

Though we have arrived at the end of Dr. Whitehead's report, we must admit that the space and opportunities at our command have permitted of our glancing only at some of the more interesting and important topics to which its author alludes. The present forms the *third* official statement which has issued from the Clinical Hospital at Manchester, and which we trust will not be the last for many more years to come. Though unpretending in form, the reports are intrinsically valuable, and contain much matter in small space. In them our attention is directed rather to the results of observation than to the ingenuities of hypothesis, to points of practice rather than to the suggestions of theory, and so far their spirit is in accordance with that of our own disposition. But seeing the high value set upon, and the great use made of mere number, rates, proportions, and tables of statistical detail in these reports, we think that we shall be offering Dr. Whitehead not altogether a useless caution in recalling to his mind the well-known aphorism—"*Ars medica non est demonstrationibus ornata, hæ mathematicis propriæ sunt, nobis sufficiat ex probabili ratiocinari.*"

Review IV.

1. *Leçons sur la Physiologie et l'Anatomie comparée de l'Homme et des Animaux*, faites à la Faculté des Sciences de Paris. Par H. Milne-Edwards. Tome quatrième. Première Partie. *De la Circulation du Sang.—Paris*, 1859.

Lectures on the Physiology and Comparative Anatomy of Man and Animals, delivered at the Faculty of Sciences of Paris. By H. Milne-Edwards. Volume IV. Part I. *On the Circulation of the Blood.—Paris*, 1859.

2. *Traité Pratique d'Auscultation.* Par MM. Barth et Roger. Cinquième Édition.—*Paris*, 1860. Chap. II. Art. I. *Auscultation du Cœur.*

Practical Treatise on Auscultation. By MM. Barth and Roger. Fifth Edition.—*Paris*, 1860. Chap. II. Art. I. *Auscultation of the Heart.*

3. *Traité Experimental et Clinique d'Auscultation appliquée à l'Étude des Maladies du Poumon et du Cœur.* Par le Docteur J. H. S. Beau.—*Paris*, 1856. Section : *Auscultation du Cœur.*

Experimental and Clinical Treatise on Auscultation applied to the Study of the Diseases of the Lungs and Heart. By Dr. J. H. S. Beau. Section : *Auscultation of the Heart.*

4. *Principles of Human Physiology.* By W. B. Carpenter, M.D., F.R.S., F.G.S. Fifth Edition.—1855. Chap. VI. *Of the Circulation of the Blood.*

5. *Experiments and Observations on the Action and Sounds of the Heart.* By G. B. Halford, M.D., Lecturer on Anatomy, Grosvenor-place School of Medicine. Pamphlet, and 'Medical Times and Gazette.'—1858.

6. *On the Cause of the Rhythmic Motion of the Heart*; being the Croonian Lecture delivered at the Royal Society, May 28th, 1857. By JAMES PAGET, Esq., F.R.S., &c. ('Proceedings of the Royal Society.')

FROM the period when the illustrious Harvey promulgated his views of the circulation of the blood, the labours of physiologists and physicians have been constantly directed to the elucidation of the various phenomena presented by the circulating organs. It is impossible to contemplate the beautiful simplicity, the terseness of language, the clear and masterly manner, with which Harvey advanced his doctrines, or the wonderful acumen with which he seized upon and explained some of the most recondite problems of physiological science, without feelings of admiration for his transcendent talents, and the indomitable perseverance with which he pursued his researches. In comparison with the results which followed his investigations, the labours of those who have succeeded him in this particular branch of science have added but little, have been but, as it were, small contributions, to the great fact he established, which revolutionized the whole science of organic life, and paved the way for the foundation of the true principles of medicine. Although some of the views which Harvey advanced may not have stood the test of subsequent researches, the truth of the "circulation" remains unassailed and unassailable. It is impossible even now, after the lapse of more than two hundred years since his death, frequent as have been the eulogies pronounced in his honour, to do other than recal to mind the labours of our countryman, and the debt of gratitude we owe him, on the occasion of offering a few remarks on that subject, which he has so essentially made his own, and with which his name is so indelibly connected. Harvey lived to reap a reward which does not fall to the lot of all men to enjoy. Opposed by many—nay, by most of his contemporaries—when he first made known his views, he yet lived to see his principles triumphant, and his facts universally acknowledged and taught throughout the great schools of Europe. Honoured by his countrymen and by the scientific world in general, made physician to the Court, elected, by the unanimous voice of its members, to the office of President of the Royal College of Physicians,* fortune smiled on the great anatomist; and when, at the ripe age of eighty years, death closed the scene of his labours, it might have been expected that his ashes would be deposited in some appropriate mausoleum, and that his posterity would mark, by some monument to his memory, the estimation in which he was held by an admiring profession and a grateful public. Amongst the many achievements of science which owe their birthplace to Britain; amongst the results of labours which have immortalized our countrymen; amongst the brilliant conceptions of genius which have gradually unfolded themselves into established truths; amongst the great discoveries which have been fertile in blessings to the human race, and have marked an epoch, alike in the history of the world and

* Harvey, although elected, did not fill the office.

in the history of science,—none stand out in greater relief, than that for which we are indebted to William Harvey. In making these remarks we feel truly ashamed when we think of the neglect to which our countryman is consigned. Known and honoured wherever civilization has spread; admired and appreciated wherever medicine is practised or physiology taught; reflecting on the place of his birth the honour due to his genius,—an annual oration to his memory is all that we have bestowed on one of the greatest philosophers this or any other country has ever produced, and one of the greatest benefactors the world has ever known. Though other nations raise their memorials in honour of warriors, statesmen, and philosophers of almost every degree, England bears the reproach that she still neglects her men of science, and whilst perennial brass or imperishable marble may perpetuate the lineaments of a general, a politician, or a successful alderman, the remains of the discoverer of the circulation moulder for two centuries in their obscure resting-place, and no monument is raised to mark our admiration of his genius, or our appreciation of his labours. Our profession has lately honoured itself in honouring Hunter, and from the manner in which the call for subscriptions to his statue fund has been responded to, we may hope ere long to see, in some appropriate place, the graven features of our great physiologist and surgeon. With this result before us, shall we neglect his illustrious predecessor? Since the time when a deputation from the Royal College of Physicians paid a visit to the tomb of Harvey, and reported that it was inadvisable to remove his remains, we have been in constant anticipation that its Council would follow the example of a sister College, and inaugurate a movement, to repair in some degree the neglect for which the whole profession is responsible; and sure we are that if the authorities of the College would initiate a step for the erection of a statue in honour of its most illustrious member, they would meet on all sides with a cordial support.

Although we have placed several works and papers at the head of this article, it is not so much our intention to pass them in review, as to take them as the basis for some observations on the subject of the circulation generally, and to consider the present state of our knowledge with reference to some of the phenomena presented by the various circulating organs. The space allotted to us will not admit of a lengthened treatment of the subject, and we shall therefore be compelled to pass rapidly over some points, in order to afford an opportunity of dwelling more at length on others. The work which stands at the head of the list we have given, we consider as the best and most complete exposition of the physiology of the circulation we are possessed of. It is the work of one of world-wide reputation, who has made the subject of physiology generally the study of a long life, and it bears upon it marks of great individual investigation, and extraordinary literary research. To the physiological student its pages are invaluable, for not only does it offer an able *résumé* of the generally-received opinions of the present day, but it teems with references to the various essays, memoirs, and papers of those who have made original experiments or observations, references which are of the highest value.

In the observations we propose to make, we shall not confine ourselves to the facts detailed in the works which are before us, nor to the references they contain, but drawing our resources from various quarters, and from our own investigations, we shall endeavour to place, as succinctly as possible, before our readers, the present position of our knowledge with reference to the different subjects we shall consider; and in attempting to explain some of the phenomena which the circulating organs present, we shall call to our aid, not only the facts which their condition in health, and experimental physiology, afford, but those also which pathology offers to our notice. We believe there are no facts of greater importance, with reference to practical medicine, than those which relate to the subject we are about to examine; for when we consider the light which the various conditions of the circulating system throw on the nature of morbid affections, and the indications they afford with reference to treatment, we cannot but feel the importance of a correct knowledge of the phenomena as they exist in health, that we may the more readily appreciate their significance as we find them altered in disease.

In considering the subject we shall follow the order in which we find it dealt with in the work of Milne-Edwards, and we shall begin with

The Movements of the Heart.—The subject of the movements of the heart occupied the attention of the ancient physicians, and Harvey and Haller devoted much time to its investigation; but it has been only of late years that the importance of a correct knowledge of these movements has been fully appreciated, in consequence of its direct bearing on the diagnosis of various cardiac affections. We believe that, notwithstanding the difficulties which beset the investigation of the subject, from the complicated nature of the heart's movements and the rapidity with which they are performed, we are now in possession of the most correct knowledge with reference to those movements, and the manner in which they succeed to each other, and we cannot agree with M. Milne-Edwards when he says "there is much uncertainty on several fundamental points."

A difference of opinion has arisen as to whether there is a regular alternation, or not, in the systole of the auricles and ventricles. It has been maintained by some physiologists that there is no period of repose, that the auricular contraction is immediately followed by the ventricular, and this by the auricular, without any pause. Cruveilheir thought he recognised this in a new-born infant, in which the heart was exposed; but M. Follin has stated that, in a similar case of malformation, the ventricular systole immediately followed the auricular, and then ensued a short period of repose, and this has been confirmed by the observations of Dr. Mitchell in a case of ectopia cordis, to which we shall have again to refer. This is quite in accordance with what has been seen by others, and what we have frequently witnessed in vivisections of dogs, donkeys, &c., and in the horse, as stated by MM. Chauveau and Faivre. The auricular contraction takes place rapidly, and is immediately succeeded by the ventricular, and a sensible interval is observed before the next auricular contraction ensues. We consider these observations on the lower animals are amply sufficient to prove the fact with regard to man, but we are not without further evidence in connexion with the phenomena as they occur in him. No doubt many of our readers had the opportunity, some time ago, of witnessing a curious and very interesting case of malformation, or fissure of the sternum, in the person of a gentleman who came over to this country from Germany, M. Groux. As we shall have to recur to this case at a subsequent part of the article, we shall content ourselves now with saying, that the tumour which existed in the middle of the fissure, which was acknowledged by most of those who saw it, to be the right auricle, diminished in size—i.e., contracted, immediately before the impulse and first sound (or the ventricular contraction), and that such contraction was exceedingly rapid.

We think it right to dwell for a few moments on the nature of the movements of the heart, as they influence the different cavities during their systole and diastole, and the more so as we believe that much misapprehension exists as to the exact changes which take place. It has been pointed out by Bryan, Sibson, Halford, and others, that when the ventricles contract, the base descends; and although there is some slight difference of opinion as to whether the apex is drawn up towards the base or not, the fact is well established, that if it be so drawn up, it is only to a very small extent; of the truth of this, especially as regards dogs, we are able to bear witness, and we quite agree with the following remarks of Dr. Halford:

"During the systole, or contraction of the ventricles, the base of the heart approaches the apex; the latter at the same time is pressed downwards, backwards, and from right to left, describing a part of a circle; the ventricles assuming a contracted globular form, descend, describing also a part of a circle, but passing forwards, and from right to left. When the contraction of the ventricles is complete, the auricles, being filled with blood, occupy part of the space within the pericardium previously taken up by the ventricles, drawing down with them the great vessels, which at this time are reacting on their contents."

Careful observations, we are told, were made by sticking pins in the ventricles during their action, to ascertain which was the least moveable part of the heart, and the apex was found to be so. These remarks are entirely in accordance with those of Mr. Bryan, which were made as long ago as 1834. That gentleman introduced pieces of wire, through the intercostal spaces, into different parts of the heart of a horse, immediately after the animal had been shot, and he found from examination when the heart had ceased to beat, that the wire introduced near the base of the heart was the one which had moved through a greater space than another which had entered at the apex. His observations on the nature of the heart's movements are most important. He points out the necessity of the ventricles descending during their contraction, in order that room may be made in the upper part of the pericardium for the distending auricles; and he shows how a conical muscle, like the heart, can perform its actions without altering the shape of the pericardium, and without occupying more space at one period of its rhythm than another. His remarks appear to us to be borne out by observation of the moving heart. If we examine a heart beating in its pericardium, we do not observe any puckering of the membrane, which always seems to be fully distended, and of course is always in contact with the surface of the heart; and we notice very little alteration in the shape of the sac. We believe that the membraneous bag contains at all times very nearly the same amount of material; that as the ventricles diminish by their contraction, the auricles are becoming distended, and the great vessels are made fuller; by these means the pericardium is kept full. If the ventricles contracted from apex to base—if, as is asserted by Carpenter, the apex were drawn up towards the base, the distension of the auricles would be interfered with, there would be no space for them to dilate in; but inasmuch as the base descends, whilst the apex remains with but little or no upward movement, a space is afforded, which is occupied by the dilating auricle as the ventricle contracts. It would thus appear, that the space occupied in the chest by the pericardium and its contents is at all periods of the rhythm the same, or nearly the same. During ventricular diastole the ventricles rise so as to fill a very large portion of the entire space; during their contraction they descend and occupy only the lower part of the space, the upper part being filled as fast as the ventricles empty themselves by the dilating auricles. When we begin to reflect on the subject, we find this is the only way in which the movements of the heart could take place, without the contraction of one set of cavities interfering with the dilatation of the other.

Diastole of the Ventricles.—In speaking of this subject we commence by quoting the following passage from the work of Milne-Edwards:

"These reservoirs (the ventricles) being relaxed, nothing is opposed to the entrance of the blood into their interior, and the forces which determine the filling of the auricles would suffice alone to bring about this result; but there is another circumstance which also contributes to facilitate the entrance of the liquid into the lower floor of the heart—this is, the natural tendency of the walls of the ventricular cavities to separate from each other, after the contraction of which they had been the seat has ceased. A muscular fibre which is at rest does not remain shortened, as it is during its contraction; and when the movement of systole, which has brought the walls of the ventricles together, so as to efface more or less completely the cavities they circumscribe, has ceased, the elasticity with which their tissue is endowed leads them to separate from each other again. Thus, when the heart is in its state of repose, it does not fall on itself, as an empty sac would do, but the ventricular cavities are more or less *gaping* (*béantes*). Now, the force which produces the return of these reservoirs to their normal size ought to tend to draw into their interior the blood contained in the reservoirs with which these cavities are in communication. The attracting force (suction force) thus developed is very feeble, but it must contribute to produce what I will call the first period of the ventricular diastole."

We have given the above quotation, as we believe it expresses a view which has been supported by many physiologists, of the active power, or suction power, exercised by the ventricles during their diastole. We must not confound this theory of Milne-Edwards with the exploded one of the active dilatation of the ventricles, from contraction of a portion of their muscular fibres, a view ably combated by Haller. The condition above described is said to be due to the elasticity of the ventricular walls. Now, notwithstanding that some experiments are adduced by Milne-Edwards, and others, in proof of the above position, we think the theory of so unphilosophical a character, that it ought not to be for a moment entertained. It is asserted that the ventricles, after their contraction, tend of their own accord to separate their surfaces from each other, and to suck in the blood from the auricles; and some physiologists have spoken of the *vacuum* thus produced. We need not dwell on this question of a *vacuum*, such a condition is contrary to all physical laws, and could not possibly exist; but we must consider at greater length the other question involved. On what principle such a power as is alluded to above is attributed to a hollow muscle, a power which we are disposed to think no one would attribute to an ordinary muscle, we are quite unable to see; nor do we see the slightest necessity for any such action on the part of the ventricles, in order to render their distension complete, or as an element in carrying on the circulation. As soon as the ventricles have contracted they *relax*, and when in this relaxed state they offer no obstacle to the inflowing of the blood from the auricles. The *vis a tergo* must be quite sufficient to send the blood into a cavity, the walls of which are perfectly relaxed, and easily dilatable; and this force is alone exercised, we believe, during the auricular pause, at which time blood is pouring into the ventricles; at the termination of the auricular pause the auricular contraction takes place, and the ventricles become fully distended. We think that sufficient distinction has not been drawn between the two conditions of the ventricles during the period of so-called diastole—viz., relaxation and dilatation. The former applies to the condition of the muscular fibre, and exists throughout the whole period of diastole—it constitutes, in fact, the rest of the muscle; the latter is the distension of the cavities, at first slow, and subsequently more rapid, from the auricular contraction. If any suction power is exerted, it must be at the period of *relaxation* of the muscular fibres before the dilatation begins; and we are quite unable to see how this relaxation could of itself produce a *gaping* of the cavity. It produces a condition which renders the ventricles easily dilated. It appears to us to resemble very much the condition which may be brought about in the closed hand. Contract the muscles, moving the fingers and thumb, and the fist is firmly clenched, and you must overcome the force which has effected this before you can insert anything into the hand, but relax the muscles, and but little power is required to effect the same. With reference to the experiments which have been performed to show that after the heart is removed from the body, and placed in water, it will suck in the water after each squeezing of the ventricles, no importance can be attached to them in proof of the point we are considering. When the pressure of the hand is removed from the ventricles, the weight of the water alone is sufficient to cause a partial distension, without admitting the existence of a suction power.

The Impulse.—The nature of the impulse has been the subject of much discussion, and even at the present day some difference of opinion exists, both with reference to the period of the heart's rhythm at which it occurs, and the cause by which it is produced. Physiologists generally agree that it takes place simultaneously with the ventricular contraction, and this we consider has been absolutely proved. But M. Beau, in France, and some experimenters in this country, have maintained that it occurs during the diastole of the ventricles, and is produced by the brisk entrance of the wave of blood injected by the auricular contraction. M. Beau supports his opinion by an appeal to experiments on frogs, cocks, rabbits, and dogs; but he confesses, that those he made on the two latter kinds of animals taught him nothing, from the rapidity of the movements of the heart, and the short duration of its contractions, after the chest was opened. Now, although we are not prepared to say that there is any difference between the rhythm of the heart in the lower vertebrate animals experimented on by M. Beau (and we believe there is none), and in mammalia, we nevertheless feel that, even admitting the truth of M. Beau's observations, the results of his experiments cannot be allowed to outweigh the conclusions of those who have frequently and with great care experimented on the larger higher animals. The most powerful argument apparently used by M. Beau in support of his theory, is furnished him by the following experiment:—He has seen that if one of the auricles is opened, the direction of the movement executed by the apex of the heart is modified; the apex is no longer carried forwards, but it deviates to the side opposite to the uninjured auricle; and this M. Beau easily explains on his hypothesis, for if the ordinary movement of the apex is the result of the injection of two waves of blood simultaneously from the auricles, the movement, he thinks, ought to be altered if one wave is suppressed, and the direction would be away from the side of the acting force. But the fact which he has adduced in support of his theory admits of a much more satisfactory explanation, as has been shown by Milne-Edwards; for when one of the auricles is opened, or pressure is used, so as prevent the blood reaching the ventricle, the latter is impeded in its action, the direction of the plane occupied by the base of the heart becomes altered, and the axis deviates to the injured side.

That the impulse of the heart against the side of the chest in no way partakes of the nature of a blow, we believe to be fully established by the experiments of Hope, Bryan, the Committees of the British Association, Sibson, Halford, Chauveau, Faivre, and others, and our own observations accord with their results. We cannot believe that the heart recedes from the surface of the chest, or that the impulse is produced by the impinging of the apex against the fifth intercostal space. We believe the impulse is produced by the ventricles assuming, during their contraction, a globular form, and in this act they press against the inner surface of the chest, and push outwards the intercostal spaces. When the heart is grasped in a living animal, the impulse or bulging of the muscle is felt on all sides, and not more in one part of the organ than in another; it is felt on the under surface of the diaphragm, when the posterior wall is touched through that structure, as well as on the anterior surface, or at the apex of the ventricles.

Further, we may adduce in support of our position, that the impulse is not the result of a blow of any particular part of the heart, the fact that the phenomenon is by no means confined to one spot. It is usually in health most distinct in the fifth intercostal space, but it may be frequently felt in the fourth as well, and in cases where the heart is enlarged, the extent of surface over which the impulse becomes sensible is much greater.

In confirmation of the views we have expressed of the movements of the heart, and the nature of the impulse, we may refer to the case of ectopia cordis observed by Dr. Mitchell, and already alluded to. He watched the movements of the heart for an hour and fifty minutes.

> "The pulsations were twenty-five in a minute before the separation of the umbilical cord, after it they fell to twenty and seventeen. After the auricles were distended with blood they emptied themselves by a gentle flowing motion, and immediately after the ventricles contracted. The effect of the ventricular contraction was to shorten the heart from base to apex, and to cause a considerable bulge or projection in the centre, giving rise to an evident elevation of the fingers when laid on it. The apex of the heart was not elevated."*

We shall not dwell longer on the subject of the movements of the heart, although there are many points of interest in connexion with it, but pass on to consider briefly:—

The Action of the Valves.—The mitral and tricuspid valves are placed at the auriculo-ventricular orifices, and by their closure prevent the blood regurgitating into the auricles; and the semilunar valves perform the same function with reference to the ventricles. In the few observations we propose to make, we shall confine ourselves to the action of the auriculo-ventricular valves. We believe the closure of these valves is of a purely passive kind, and not influenced by the muscular action of the columns which are found in the interior of the heart. We have evidence in a considerable number of experiments, that when the valves are lying in a state of rest, any fluid directed forcibly against them will effect their closure. It has been the opinion of many physiologists, that the musculi papillares play an active part in the closure of these valves; but we believe we shall be able to show that the only function they perform is to support the valves against the column of blood, and prevent them being thrust into the auricles—in fact, to hold them in such a manner that they may form the resisting roof of the ventricular cavity. We believe that M. Baumgarten, in 1843, gave the first true explanation of the mode in which these valves are closed; but the view, although adopted by some physiologists in this country, has not found its way into many of the text-books perused by our students. Valentin, in his work on physiology, has pointed out that the pressure of the blood in the ventricles is sufficient to close the valves; and Dr. Halford has more

* Dublin Journal of Medical Science, 1844.

recently shown very clearly the exact process by which the closure is effected. We quote his experiment:

"A bullock's heart was obtained, and the auricles cut away nearly as low as the auriculo-ventricular openings; the cavities of the ventricles were well washed out, and the coagula carefully removed. A vulcanized indiarubber tube, of like diameter with the pulmonary artery, was then attached by one extremity to the vessel, and by the other to a common forcing-pump; water was then thrown into the pulmonary artery, and the semilunar valves tightly shut down, gentle pressure being maintained, in imitation of what takes place during life. The right ventricle, being empty, was in the same state as when the auricle is about to inject it. On pouring water into the ventricle, the flaps of the auriculo-ventricular valve rose upon the surface of the fluid, until (the ventricle becoming fully distended) the valve formed a perfect septum between it and the auricle. The left side of the heart was tested in the same manner, and with results perfectly the same, notwithstanding the greater thickness of the valve, the larger size of the musculi papillares, and the stronger chordæ tendineæ."

The explanation given by Milne-Edwards of the way in which these valves close, is very similar to that just quoted. He shows that so perfect is the closure, that when the ventricles are full, the heart may be held upside down without the loss of a single drop of fluid. We shall refer hereafter to an experiment which shows that these valves are not only water-tight, but air-tight.

From the manner in which the valves behave under the influence of fluid injected into the ventricles, in the way we have described, a very important conclusion may be drawn with respect to their action, and that of the musculi papillares. As soon as the blood enters the ventricles the valves rise on its surface; no action of the muscles is required to draw the flaps together. When the ventricles are fully distended, the valves present a septum between them and the auricles; the former react on their contents, and the blood is forcibly impelled against the under surface of the valves; the chordæ tendineæ and musculi papillares are tightened, they prevent the flaps being thrust into the auricles, and inasmuch as the base of the heart descends during the ventricular systole, the musculi papillares contract from above downwards, and thus maintain the roof of the cavities firm and resisting till the contents are fully expelled.

We shall not dwell longer on this subject than to make one remark on the following passage which we find in the work of Dr. Carpenter:

"Whilst the right ventricle is contracting upon the blood that has entered it, the carneæ columnæ, which contract simultaneously with its proper walls, put the chordæ tendineæ upon the stretch, and these draw the flaps of the tricuspid valve into the auriculo-ventricular axis. The blood then getting behind them, and being compressed by the contraction of the ventricle, forces the flaps together in such a manner as to close the orifice; but they do not fall suddenly against each other, as is the case with the semilunar valves, since they are restrained by the chordæ tendineæ, whence it is that no sound is produced by their closure."

We object to the statement that the columnæ carneæ perform the functions here assigned to them, viz., that of drawing the flaps of the valves into the auriculo-ventricular axis. We have shown that they will rise to this position by the pressure of fluid, and that muscular action is not required; and we are quite unable to see how the contraction of muscular bands, attached to the ventricular walls in the way that the musculi papillares are attached, can have any influence except that of drawing the valves down in the way we have mentioned. We cannot, therefore, agree with Dr. Carpenter in the view he has taken of their action.

Having said thus much of the movements of the heart and the closure of the valves, we naturally pass to the consideration of the sounds by which these phenomena are accompanied.

The Sounds of the Heart.—The causes of the sounds of the heart have been and still are a *questio vexata* with physiologists and physicians. That the movements of the heart were accompanied with sound, was recognised by Harvey; but he seems to have had no clear views on the subject, and it was not till the time of Laënnec that attention was strongly directed to the fact, and it became an object of special study. The researches and investigations made in this direction have been so numerous, and the opinions expressed so various, that phenomena which we believe to be of the most simple possible character, have been rendered difficult, and obscured, by the very abundance of evidence brought forward to explain them. The investigations which have been made, especially the vivisections of various physiologists, of Hope, Williams, the committees of the British Association, &c. &c., have been of the greatest possible service to practical medicine, and have settled, we consider beyond dispute, the period at which the sounds occur, and the events with which they are simultaneous. We say this notwithstanding that some physiologists dispute the conclusions generally arrived at, and amongst them is one whose name appears at the head of this article, M. Beau. We consider it a fact, which can be easily demonstrated in the larger lower mammalia, and which does not admit of the slightest doubt, that the *first sound* of the heart takes place simultaneously with the systole of the ventricles, and not as M. Beau and others formerly, Corrigan, Pigeaux, &c., have asserted, during their diastole. We make the assertion not only upon the testimony of others, but from a careful examination of a series of vivisections of the lower animals we ourselves performed, as well as from a consideration of the phenomena as they take place in man. This conclusion, established by experimental physiology, has been of the utmost importance with reference to the diagnosis of cardiac affections, and to it we may attribute the accuracy, with which the various valvular affections of the heart may at the present day be determined.

We shall not attempt to pass in review the numerous opinions which have been entertained of the sounds we are now considering, but, from careful examination of the results of experiments performed by others, as well as by ourselves, and investigation of the human heart in its healthy and abnormal condition, endeavour to separate the false theory from that which is true, and to place the question as fairly as possible before our readers; and we may remark that our observations will be chiefly confined to the first sound, about which so much difference of opinion has existed; and we shall say but little of the second, of the cause of which most physiologists are now agreed.

We must first remind our readers of the events which are taking place during the time the sounds are produced, and then endeavour to ascertain how far each of these events may assist in their production.

The events which correspond with the first sound are: First stage of dilatation of auricles; contraction of ventricles; closure of auriculo-ventricular valves; opening of ventriculo-arterial valves; propulsion of blood against the auriculo-ventricular valves, and through the orifices of the aorta and pulmonary.

The events corresponding with the second sound are: Relaxation of ventricles (first stage of dilatation); dilatation of auricles; backward flow of blood in aorta and pulmonary artery towards ventricles; closure of semilunar valves.

It may perhaps be well to say a few words at the present time of the theory advanced and maintained very recently by M. Beau, viz., that the first sound is produced by the shock of the column of blood, propelled into the ventricles by the contraction of the auricles. The experiments we have before alluded to, seem to us to have established beyond a doubt, the fact that the sound is synchronous with the ventricular systole, and our own observations are entirely in accordance with this conclusion. In experimenting on very small animals it may be difficult to decide when the sound occurs, for the rhythm of the heart occupies so short a time, that it becomes almost impossible to analyse its movements; but with larger animals it is far otherwise, and in dogs and donkeys no difficulty is experienced, in deciding as to the order in which the different movements succeed each other, and the phenomena by which they are accompanied. Further, in addition to the results alluded to, MM. Chauveau, Faivre, and Gabriac have recently experimented on horses, and confirmed the observations of previous investigators. The examination of the heart of the horse, in consequence of the contractions of the cavities of the organ succeeding each other very slowly, is admirably adapted to give a satisfactory solution of this question.

The most important theories that have been advanced of the causes of the first sound of the heart, are, that it is produced either entirely or partly by one or more of the following actions: 1. Impulse. 2. Muscular sound. *a.* Muscular contraction—*bruit musculaire.* *b.* Muscular extension (Dr. Hope). 3. The rush of blood from ventricles to arteries. 4. The opening of the semilunar valves. 5. Closure of the auriculo-ventricular valves.

In considering the question as to which of the above-mentioned phenomena, either produces or contributes to the sound, we shall examine each separately.

1st. *The Impulse.*—Does the impulse produce, or assist in the production of, the first sound? We have already, in speaking of the movements of the heart, expressed our opinion as to the nature of the impulse, which may often be seen and felt on the surface of the chest;

and if our views are correct in this respect, it will be at once admitted that the pressure exercised by the walls of the heart, as they assume a globular form, against the walls of the chest—a pressure which in no way whatever resembles a blow—possesses in itself no element from which a sound, except one of a rubbing character, is likely to arise: such rubbing sound we know does not exist. We consider it unnecessary to dwell at length on any theoretical argument, to prove that the impulse in no way whatever contributes to the first sound, because we have experimental proof that when the sounds are heard, without the intervention of the walls of the chest, they are as distinct as, or even more so than, before. This has been pointed out by Hope and numerous individual observers, as well as by the Committee of the British Association in 1840, who say:

"The doctrine that the præcordial pulsation is caused by a blow received by the ribs, in consequence of the heart's jumping or striking against them, appears to be superfluous with a view to explanation of phenomena, and to be substantially unfounded in point of fact."

We find, however, that Milne-Edwards and MM. Barth and Roger still admit the impulse, or *choc du cœur*, as one of the sources of the first sound, and the former author specially refers to an observation of Magendie, that when the heart was pushed away from the surface of the chest, by an instrument introduced between the ribs, the first sound was no longer heard, whilst it returned when the organ was allowed to come in contact with the ribs again. This made Magendie conclude that the impulse was the sole cause of the sound—a statement entirely disproved by the fact mentioned above, of the sound being heard when the heart is exposed by the removal of the anterior walls of the chest.

When we consider the influence produced, even by a slight effusion of fluid into the pericardium, on the sounds, rendering them less distinct, and when we reflect that the intervention of a piece of lung, as we find in emphysema, although it does not push the heart entirely away from the walls of the chest, materially modifies the intensity of the first sound; and when further we consider that we do not, except under extraordinary circumstances, hear the sounds when we withdraw the ear, even for a very short distance, from the chest, the experiment cannot be esteemed as affording any proof of the statement Magendie advanced; but as simply proving that under the circumstances the sounds were not *heard*—not that they were not *produced*; to render his position tenable, he should have proved; that, when the ear, or a stethoscope, is placed in contact with the heart, its rhythm is attended with silence.

We think we may fairly conclude that the impulse is in no way concerned in the production of the first sound. Could it be proved that the heart receded from the chest, and was tilted or impinged against it at each successive systole, we might expect a sound would be produced; but were such the case, the individual would feel the shock which would necessarily result from such an action.

We propose, in the next place, to consider the subject of, 2nd. *Muscular Sound.*—The theory of muscular sound, in connexion with the heart, is the oldest to account for the acoustic phenomena its action presents. It seems to have been for a long time unquestioned, and at the present day receives the sanction and support of numerous physiologists. That the muscular structure of the ventricles must necessarily produce a sound in its contraction, appears to have been generally admitted as an established truth, and no one, until somewhat recently, took the trouble to put the question to the test of actual experimental examination, in such a manner as to elicit, in our opinion, a safe and satisfactory reply. We are aware that, in the experiments performed by Hope and Williams, the question was thought to have been settled; but we think we shall be able to show that their results in this respect cannot be relied on, and that the arguments brought forward in support of the theory, on the ground that sounds of a similar character can be produced during the contraction of voluntary muscles, are inconclusive and unsound. In speaking of the so-called muscular sound, we have to consider it in a twofold aspect—first, the muscular extension sound; secondly, the muscular contraction sound. The theory of muscular extension sound, as an element in the first sound of the heart, we owe to Dr. Hope, who describes it as "a loud, smart sound, produced by the abstract act of sudden jerking extension of the braced muscular walls."

Although the explanation of the sound given by Dr. Hope may be considered somewhat obscure, we infer him to mean that a sound is produced by the tension of the muscular walls, when the ventricles, being full, are about to contract on their contents. Now, considering the condition of the ventricular walls, that they are first gradually distended by the flow of blood from the auricles, and then filled out by the auricular contraction—that when fully distended they contract on their contents, which yield to the pressure acting on them, it is difficult to understand how a state of tension which would be sonorous, can possibly be brought about; moreover, these walls are formed of a tissue which is little calculated to produce sonorous vibrations. The case is, however, far otherwise with the tendinous auriculo-ventricular valves, which have to resist and sustain the force of the ventricular contraction, and which constitute, during the time they are stretched across their openings, a portion of the walls of the ventricles. Further, we cannot conceive that there is any reason why the muscular substance should become tense—that is to say, stretched and resisting, the only condition which could give rise to sound; for when the ventricles contract, they experience resistance only in one direction—that of the auricular orifices—and there is a free outlet for their contents in the direction of the vessels. Did the ventricles meet with resistance on all sides to their contraction, we could understand that their walls would be rendered tense, although we cannot see that even then they would elicit sound, for we think the physical condition of the muscle is inadequate to its production.

We pass next to consider the muscular contraction sound—*bruit musculaire.* Are we in possession of facts sufficiently strong to prove that the contraction or shortening of the fibres of the ventricles produces, or contributes to, the first sound of the heart? Dr. C. J. B. Williams at one time believed this was the sole cause, but he subsequently abandoned that position, and admitted a share to the closure of the valves.

The following are the observations upon which Dr. Williams relies in support of his views. A donkey under the influence of woorara was the subject of experiment. The left auricle having been laid open, and the mitral valve partially destroyed, and the right auricle having been freely laid open—

"Obs. 8.—I pushed my finger through the mitral orifice into the left ventricle, and pressed on to the right, so as to prevent the influx of blood into either ventricle; the ventricles continued to contract strongly (especially when irritated by the nail of the finger in the left), and the first sound was still distinct, but not so clear as when the ventricles contracted on their blood.

"Obs. 9.—The same phenomena were observed when both arteries were severed from the heart."

Before we make any observations on these experiments of Dr. Williams, we desire to direct attention to the experiments, which we believe to be of a far more satisfactory character, performed by Dr. Halford. After stating that, in order to settle so intricate a question as that of the cause of the first sound, there should be no rude interference with the mechanism of the heart's action, and that its cavities should remain untouched, that gentleman says:

"My proceedings were as follows:—Large dogs were obtained, and as in my preceding experiments (the animals being under the influence of chloroform), the heart was exposed, and the circulation kept up by artificial respiration. A stethoscope being applied to the organ, the sounds were distinctly heard. The superior and inferior venæ cavæ were now compressed with bull-dog forceps, and the pulmonary veins by the finger and thumb; the heart continuing its action, a stethoscope was again applied, and neither first nor second sound was heard. After a short space of time, the veins were allowed to pour their contents into both sides of the heart, and both sounds were instantly reproduced. The veins being again compressed, all sound was extinguished, notwithstanding that the heart contracted vigorously. Blood was let in, and both sounds were restored. I have thus frequently interrogated the same heart for upwards of an hour, and always with the like result."

Dr. Halford's experiments have been performed in many of our metropolitan schools, and he has given a long list of those who have witnessed them and borne testimony to the truth of his statements; amongst them we find the names of the late Dr. Marshall Hall, Drs. Wilson, Fuller, Ogle, and Messrs. Lane, Tatum, and Blenkins.

These is an interesting circumstance which took place at one of Dr. Halford's experiments, which appears to us of great importance. It shows that when only a small quantity of blood finds its way into the ventricles, the first sound is still produced. The cavæ and pulmonary veins having been compressed, Mr. Lane, at whose request the experiment was performed, listened to the heart during its contraction, and said he heard the first sound indistinctly, not so clearly as before the compression. On examination, it was found that the vena azygos entered the right auricle by an independent opening, and was not secured; the vessel was compressed with the others; the heart contracted; no sound was heard.

Now, let us compare the two experiments we have detailed above, the one by Dr. Williams, the other by Dr. Halford: the one in which, according to the former gentleman, there is evidence that the ventricular contraction produced a sound, the other in which, according to the latter gentleman, whilst the ventricles are acting powerfully, no sound is elicited. Of the truth of the facts stated by Dr. Halford, we have the testimony of many on whom we can rely, and from repeated examinations we can add our own. Of the fact stated by Dr. Williams we have the concurring testimony of Dr. Hope and the Committee of the British Association, and we entertain no doubt that in the experiment a sound was produced. But can we compare the two experiments with each other? Was there no source of sound other than the contraction of the muscle present in Dr. Williams's experiment? and is there any other in that of Dr. Halford? In the former the auricles were opened, the valves rudely destroyed, the fingers thrust into the cavities, air was allowed to enter. It would indeed be strange if with all these foreign sources of sound, with this mixture of blood, air, and fingers, the ventricular contraction were silent. In order to render the experiment conclusive, it should have proved that the contractions of the heart (whilst the source of sound from the distended valves was destroyed without any rude interference with its cavities) produced a sound, but no such experiment was ever, that we can learn, performed until that of Dr. Halford, which clearly points out that the entrance of the blood into the cavities being prevented, the vigorous contraction of the empty ventricles is unattended with sound. If it be asserted that the contraction is not of a similar character to that of the distended ventricles, and that the experiment does not prove that the ordinary contraction produces no sound, we maintain that the shortening of the muscular fibres takes place, the muscle assumes its globular form, and in fact is placed in that condition in which it ought to elicit a sound, if the muscular contraction could do so. Further, the so-called muscular sound (of voluntary muscles), which is said to resemble the first sound of the heart, takes place during the contraction of the fibres, without the muscle having any substance to contract upon. If such an argument as we have just supposed were brought forward, we think it must fall of its own weight; and we are unable to see how the advocates of the muscular theory can get over the experiment we have described. We are aware that some of those who have witnessed it have expressed a doubt of the facts, and believe that they heard a sound. Now, as we believe these gentlemen only saw the experiment performed in a crowded theatre, where there must necessarily be much trouble in maintaining absolute silence, and from the hurry in examining, and the difficulty, under the circumstances, of discriminating between a sound of rubbing against the stethoscope, and one produced in the muscle, we cannot accept their statement, in contradistinction to that of so many who have borne opposite testimony, and especially as we have performed the experiment ourselves under conditions of absolute silence, in a room with few attendants, and in the quiet of the night. We feel, therefore, that we are in a position to speak confidently of our own observations.

We may further remark, that it has always appeared to us a very strong argument against the muscular theory, that the contractions of the auricles are unattended with sound—at least with any audible sound. We do not recollect to have seen any explanation of this fact, but we know that the Committee of the British Association, in their Report of 1840, state that the auricular contraction *is* attended with sound; but they say, "it is difficult of detection, even in the naked heart, owing to its being absorbed in, or masked by, the immediately succeeding and vastly louder systolic ventricular sound." As this fact has never been observed by other experimenters, with the exception of the Committee of Philadelphia, and as we never hear the sound during the healthy action of the heart, with the chest undisturbed, we cannot attach much weight to the observation, and we think we are bound to admit that the auricular contractions are silent. We cannot, however, we must confess, understand how these contractions can be silent, if it be true that the ventricular contractions are not so. If it be asserted that they are so in consequence of the small amount of muscular tissue the auricles contain, we answer that in some animals, the auricles of which are much larger and more muscular than the ventricles of others, no difference in the number or nature of the sounds exists—we have not in the horse, the ass, or the ox, as we ourselves can testify, and in larger animals, as the Committee of the British Association have shown, a sound attending the auricular contraction, another the ventricular, and a third the closure of the semilunar valves; the silence is complete between the second sound and the next first, during which time the auricles contract. We therefore cannot understand on what principle—setting aside experimental evidence—it is asserted that the contraction of a small ventricle, like that, for instance, of the rabbit, or the human fœtus, can elicit a sound, and yet a similar contraction of a much larger muscular cavity, as the auricle of the horse, ox, elephant, etc., should be perfectly independent of such sound.

We must here allude to the auricular sound which was heard in the case of M. Groux, already spoken of. Those who had the opportunity of examining and auscultating that gentleman are aware that a sound was heard over the position of the right auricle. It was assumed by some that the sound was due to the auricular contraction. A careful examination of M. Groux on two occasions, once in our own study, during nearly three quarters of an hour, when every facility was afforded us for accurate observation, convinced us that the sound was only heard under certain circumstances, which in our opinion explain its cause. It was heard when the stethoscope was applied *over* the auricle, and *slight pressure was made.* It was not heard when no pressure was made, nor yet when firm pressure was made. It differed altogether in character from *the* sounds of the heart, and resembled somewhat the rush of fluid through a constricted tube. Taking these facts into consideration, we came to the conclusion that the sound was the result of the pressure exerted, and not of the auricular contraction, that the moderate pressure altered the shape and calibre of the auricle, and thus modified the course of the current of blood, whilst the absence of sound under firm pressure may be accounted for by supposing that the walls of the cavity were brought in contact.

With regard to the so-called muscular sound produced by the contraction of the voluntary muscles, we are very much disposed to agree with Kiwisch, that it is an acoustic delusion. We do not deny that a sound is produced, but we have never been able to satisfy ourselves that the rumbling noise which is heard is the result of the muscular contraction. Kiwisch is of opinion, that the sound is produced by the vibration of the air in the meatus and in the stethoscope, and he says that if this source of error be avoided, no sound is heard, as may be proved by listening over a uterus contracting in labour. If we are to take as a type of muscular sound, the noise produced by the contraction of the masseter, as heard when the head is lying on a pillow, we confess it possesses, in our opinion, but little resemblance to the first sound of the heart. Moreover, it is easy to satisfy oneself that the noise is not confined to the contraction of the muscle, but is also heard during its period of relaxation.

The Rush of Blood from the Ventricles into the Arteries.—Milne-Edwards remarks:

"The effect produced by the circulating current on the different yielding and elastic portions of the channels which the blood traverses in the interior of the heart, may also contribute to the production of the complex sound with which the ventricular systole is accompanied."

Amongst other authors in this country who have expressed opinions that the flow, or rush of blood, assists in the production of, or wholly produces, the first sound, is the late Dr. Bellingham, of Dublin, who says:

"The first sound of the heart we know is synchronous with the ventricular systole; in this act, the blood, compressed by the contraction of the powerful muscular walls of the ventricles, is propelled with considerable force into the aorta and pulmonary artery, the sigmoid and semi-lunar valves of which are suddenly elevated. In the rapid passage of the blood from a wider to a narrower area, there must be considerable friction between this fluid and the parietes of the arterial orifices; quite sufficient, in my mind, to produce the prolonged first sound of the heart."

We may add here, that Dr. Bellingham considers that the second sound of the heart is caused by the rapid flow of blood into the ventricles during their diastole, thus attributing both sounds to a similar cause, the flow of blood. With regard to the view advocated by Dr. Bellingham, it may be remarked, that there is no abrupt contraction of the passages through which the blood flows from ventricles to vessels; the parts which lead to the aorta and pulmonary artery are by far the smoothest portions of the latter cavities. To suppose that this action would be attended with sound, is to suppose that the blood meets with an impediment to its course, which would be to imply that nature had been at fault in the construction of the apparatus. Further, as we shall presently show, the ventricles being filled with blood before they contract, there is no violent propelling of the blood, but a quiet pushing of it into the vessels, which, as they receive it, yield in proportion to the force with which they are distended: an additional reason for the non-production of sound. Were the blood propelled into cavities filled with air, or partly with air and partly with blood, we should expect a sound to be produced; but what is the case with regard to the distension of the ventricles? Their walls are in contact at the end of each systole, relaxation of their fibres takes place, and blood is immediately poured in from the auricles by the *vis-à-tergo.* This gradually and silently distends the cavities, and at length the auricular action brings them to the necessary condition to produce contraction. There is no element of sound here; there is no splashing of the fluid blood with a fluid gas; all the actions are going on in air-tight cavities, silently and harmoniously. As well might he expect to produce a sound by succussing a man with effusion in the pleura as to expect that the current of blood in the chambers of the heart, in their normal state, would give rise to a sound; but once admit the air, another element is present and sound is elicited.

We cannot help referring to an ingenious hypothesis, advanced, and very cleverly defended, by Dr. Leared, in the 'Dublin Quarterly Journal of Medical Science' for May, 1852, of the cause of the first sound. We give his own words:

"Having now shown that sounds, closely allied to the natural first sound of the heart, can be formed by the shock occurring between two portions of a liquid of a certain consistence, one of which, on being forcibly propelled by an intermittent action, is brought into contact with the other in a state of rest, or comparatively so, I will proceed to apply the fact to the conditions existing in the heart itself, taking its left side to represent both.

"Subsequent to the elastic reaction of the aortic walls, which we must suppose does not occupy the entire period of the diastole of the ventricle, the column of blood in the upper part of the aorta attains a state of momentary repose. This column in a normal state is under considerable tension, and it is perfectly isolated from the contents of the ventricle by the semi-lunar valve. When systole occurs, the valve with its superposed blood is forcibly thrown forwards by the vigorous propulsion of blood from the ventricle; concussion now ensuing between the active and passive portions of blood, a sound is produced on the same principle and from the same cause as in my experiment; and this, *cæteris paribus,* is the essential element in the normal first sound of the heart."

Now, we may remark with reference to this view—that concussion takes place between the passive and active columns of blood,—that the nature of the concussion is such, that we could scarcely expect it to produce a sound. During the time the ventricles are filling, the blood in them rises to the level of the under surface of the semi-lunar valves, and when the cavities contract the fluid pushes open the valves, and communicates its force to the column in the vessels. There is no propulsion of the blood against the passive column from a distance; there is no space between the blood which fills the ventricles and that in the vessels; one column lies above the valves, the other beneath them. We believe it is contrary to all we know of the phenomena which accompany the passage of fluids in closed tubes, that a sound should result under such circumstances.

The experiment Dr. Leared relies on, in support of his theory, seems to us rather to prove the contrary of what he asserts. By means of an india-rubber bottle surmounted by a neck, he propelled through an elastic tube connected with the neck, a stream of water, by intermittent pressure. The elastic tube was placed horizontally under water in a large vessel; the water was therefore propelled, from the open mouth of the tube, into that which was in the basin. Dr. Leared tells us, that a sound was heard at the orifice of the tube, and that it was not produced by friction, but by the fluid itself impinging upon the passive water in the basin. He also tells us that the sound diminished in intensity from the orifice of the tube towards the bottle, at the neck of which none could be detected. Dr. Leared's experiment proves that the passage of a fluid from a wide to a narrow stream, when no impediment exists, is unattended with sound, and is therefore so far an argument in our favour. That there is any parallelism, between the impinging of a fluid from the *open end* of a tube, on fluid of the same kind, in a more or less passive condition, in a vessel, and the impulse given by the column of blood below the semi-lunar valves to that above them, we cannot for a moment admit, nor can we admit that there is any proof that the sound which Dr. Leared heard was not produced by friction of the fluid against the sides of the orifice of the tube. The fact of the fluid passing from the *open extremity* of the vessel appears to us to vitiate the experiment, and render it valueless for the purpose intended.

Some physiologists have expressed an opinion, that the collision of the particles of the blood amongst each other during the ventricular systole, contributes to the first sound. We think it quite a sufficient answer to this view, that during other periods of the heart's rhythm such collision produces no sound. The blood is poured into the auricles, from auricles to ventricles, even forcibly during the contraction of the former; it is further injected against the irregular walls of the ventricles, which present a condition, from the arrangement of the columnæ carneæ, which ought, in our opinion, to elicit a sound, if such collision were capable of producing one. But, notwithstanding this, these actions are silent. Shall we, then, conclude that what produces no sound at one time produces it at another, without the existence of any essentially different condition?

Opening of the Semi-lunar Valves.—Bouillaud thought that the recession of these valves assisted in the production of the first sound, which he attributed otherwise to the tension of the auriculo-ventricular valves. Others have had the same opinion. We shall not dwell long on this supposed cause, as we consider it cannot in any way assist in the production of the first sound. When we reflect how the valves are placed at the moment of systole: that they have a column of blood above and one below them, that they are not forcibly thrown against the sides of an empty vessel, that the blood on the upper surface has to be displaced, and that this must prevent the shock of the valves against the walls of the vessel, we cannot admit that there is any element of sound in the action we are considering, and such is the view taken by most physiologists.

The Closure of the Auriculo-Ventricular Valves.—That the closure of these valves is an element in the production of the first sound is now generally admitted, and we believe we shall be able to adduce sufficient arguments to prove, that the tension they must necessarily undergo during the contraction of the ventricles is attended with vibrations of a sonorous character, and that every other source of sound being eliminated, these vibrations are capable of producing it alone.

We must be content with referring to the names of those whose observations and experiments have forced the opinion we have enunciated on the notice of physiologists, and with endeavouring to refute the chief reasons assigned for its rejection. We believe Dr. Billing, in 1832, was the first to place the theory fairly before the profession, and in the same year Rouanet advanced and supported it in a thesis. We believe these gentlemen worked independently of each other, and the view may have been original with both. It found a warm supporter in M. Bouillaud (who however, as previously mentioned, thought that the opening of the semilunar valves contributed to the sound), whose work on the heart contains an able analysis of the views current at the time it was written. It was also ably advocated by Mr. Bryan in the pages of the 'Lancet,' and although the researches of Hope, Williams, the Committees of the British Association, the Committee of Philadelphia, etc., convinced these various investigators that there were other causes to which the sound was due, the valvular element became a generally admitted fact, and with some few physiologists maintained its ground as the sole cause.

We are not aware that any important investigations on the nature of the sounds of the heart were made, after the Committee of the British Association concluded its labours in 1840, until the year 1847, when we find a very able paper on 'The mechanism by which the valves of the heart are closed, and by which the sounds of the heart are produced,' by Dr. Hamernjk, of Prague, published in the 'Prager Vierteljahrsschrift,' vol. xi., which paper contains the views also of Dr. Baumgarten on the subject.

The next contribution of importance we find in a paper by Mr. Brakyn, of Dublin, who made an ingenious experiment, the result of which was published in the 'Lancet' of 1849, to prove the adequacy of the valves in a state of tension to produce sound. Mr. Brakyn repeated his experiment in one of our metropolitan schools, and this revived the controversy which had for some years been dormant, in this country at least, and brought out fresh labourers into the field. His experiments were followed by those of Dr. Halford, which we believe to be of a nature more calculated to settle the disputed question of the causes of the sounds of the heart than any previously instituted. We are not aware that any further investigations of importance have been made since those of Dr. Halford, which were originally performed in 1851, and have since been repeated in many of our London schools and elsewhere. About this time also (1850) Dr. Kiwisch published an able paper on "The Production of the Sounds arising in the Organs of Circulation," in which he defends the valvular theory.

Dr. Billing, in an essay read to the Hunterian Society in February, 1832, says:

"The first sound is caused by the tension produced in shutting the auriculo-ventricular valves, and the second sound is caused by the tension produced in the shutting of the ventriculo-arterial valves."

Again he writes, in 1833, in the 'Lancet':

"The succession of the phenomena of the heart's action is as follows: First the auricle contracts, then the ventricle, by the action of which latter the auriculo-ventricular valves are shut by the pressure of the blood against them. Upon the relaxation of the ventricle, the semilunar valves are shut by the backward pressure of the blood in the artery. The first sound takes place exactly synchronous with the impulsion and action of the ventricle; hence it might be supposed that the action of the muscle (as averred by some) produces the first sound. But the second sound takes place when there is no action of the heart going forward, and this is peculiarly evident when there is an intermitting pulse, as there is then a marked pause after the second sound; so that, in fact, there is nothing but the semilunar valves in operation to produce sound at the instant."

Rouanet in his thesis says:

"Numerous experiments have proved to me that every membrane, on passing from the state of relaxation to that of sudden tension, yields a sound, which varies according to circumstances, and is more or less intense in proportion to the powers of tension. Its tone increases with the fineness and extensibility of the membranous tissue. The greater size, thickness, and extensibility of the membrane render the sound duller, and the substance to which it is attached modifies the quality of sound in proportion to its thickness, softness, and elasticity."

In applying these principles to his theory, M. Rouanet attributes the first sound to the sudden approximation and tension of the mitral and tricuspid valves, during the ventricular systole; and the second to the brisk tension of the sigmoid valves, owing to the counter shock of the column of blood in the aorta and pulmonary artery. He says:

"The first sound is loud, and depends to a certain extent on the energy of the ventricles, and is duller than the second. The valves which occasion it are large, and the walls which conduct it thicker. The second sound is sharper, because the valves are smaller, thinner, and attached to more sonorous walls."

Now, we believe that in these few observations of M. Rouanet, so simply and clearly expressed, the whole subject is contained, and that he has given a truthful explanation of the phenomena. It is unnecessary to quote the experiment he performed, in which he imitated the second sound of the heart by allowing a column of fluid to fall on the sigmoid valves.

Working at the same subject, and unacquainted with the labours of Dr. Billing and M. Rouanet, Mr. Bryan, in 1833, published a series of papers of great value on the movements and sounds of the heart. He advanced, and most ably defended, the valvular theory. Amongst other remarks, he has the following:

"Any flexible solid suddenly brought from a state of relaxation to a state of tension, vibrates, and the vibrations are sonorous or not, *i.e.*, audible or not, according to its physical structure.

"At the commencement of the systole of the ventricles, the auricular valves are flapped into play, and at the instant of their closure the whole substance of the ventricles and the valves are suddenly brought to a state of tension, and then consequently vibrate. I leave it to the reader to determine, according to the laws of physics, whether the vibrations of the valves, floating freely in fluid, or the muscular substance of the ventricles, trammelled by the contact of surrounding solids, would most contribute to the formation of the first sound."

The explanation given by Drs. Hamernjk and Baumgarten of the cause of the first sound, is similar to that of Mr. Bryan.

"The first sound of the heart is occasioned by the vibration of the tense auriculo-ventricular valves, acted on by the blood propelled against them during the systole of the ventricles, and the vibration of the chordæ tendineæ."

With the view of showing that the tension of the valves was capable of giving rise to a sound, Mr. Brakyn devised the following ingenious experiment. An ox's heart being procured, a bladder is connected with the left ventricle by means of a flanged tube; with the aorta, is connected another bladder, and this latter has a tube passing from it, which is connected with a third bladder attached to the left auricle. The tube between the second bladder and the third has a metallic portion with a stop-cock in it. This arrangement of bladders and tubes allows of the passage of currents of air, representing a mimic circulation through the left heart. The experiment is performed as follows: the bladders being inflated, and the stop-cock closed, pressure is made on the auricular bladder, and the air is injected into the left ventricle and ventricular bladder; the latter is compressed, and the air, forcibly impelled upwards, produces a closure of the auricular valve, and passes into the aorta and the bladder connected with it; this bladder is now suddenly compressed, the sigmoid valves close, and the air is prevented passing back into the ventricle. The phenomena attending these actions are two sounds, one when the ventricular systole is imitated, and the auricular valve closed, the other when the elastic reaction of the aorta is imitated, and the semilunar valves are closed.

In remarking on his experiment, Mr. Brakyn says:

"The sounds being produced without any muscular contraction, rush of blood, etc., must evidently be valvular . . . . the first sound is as perfect as the second. . . . . The illustration, though conducted through air, ought to be conclusive, inasmuch as a suddenly strained membrane which gives a tympanic sound in air, will do the same in water also, as I have tried, but not so loudly."

From having repeated the above experiment, we quite agree with the observations of Mr. Brakyn; we consider the sounds resemble those of the heart as closely as possible, bearing in mind the nature of the fluid in which they are produced. The first is a duller and more prolonged sound than the second, and each differs very little, slightly only in tone, from the corresponding clear sound sometimes heard in a dilated and thin heart.

Subsequent to the experiments of Mr. Brakyn, came those of Dr. Halford, which we have already detailed; which, although they do not directly prove the truth of the valvular theory, yet afford positive evidence of the silent contraction of the muscle, and in our opinion leave no other cause for the sound. Putting together the experiments of Brakyn and Halford, we consider experimental evidence of the theory we are examining, complete; the investigations of the former taken alone might be deemed inconclusive, as they afford no proof that muscular action does not assist in the sound; but on this latter point the experiments of the latter are decisive.

We esteem it a very strong argument in favour of the view we have taken, that it has been experimentally proved that the closure of the semilunar valves is the sole cause of the second sound; and those who admit this, must also admit that it is impossible for the auricular valves to close without giving rise to a sound, which must vary in its tone, duration, etc., in proportion as the valves differ in their structure, size, attachment, etc., from those of the aorta and pulmonary artery. We have never been able to satisfy ourselves as to the existence of any difference between the two sounds, except in degree, not in kind; they are as much like each other as are two notes of music produced by the vibrations of different sized chords.

It may be objected to the experiment of Dr. Halford, that the blood is absent from the cavities, and therefore no proof is afforded that its progress through the aorta and pulmonary openings does not produce the first sound. We consider we have adduced sufficient evidence to show that "the rush of the blood" cannot be looked upon as giving rise to a sound.

It has been objected to this theory, that the sound is too prolonged to be produced by the tension of the auricular valves alone, for it is said that their closure is sudden, and takes place at the commencement of the ventricular systole, whereas the sound itself is prolonged. Now we do not assert, for we do not believe, that the first sound lasts during the whole period of the systole; but that it is more prolonged and duller than the second sound is obvious to all, and we believe that the structure of the valves is sufficient to account for the fact. We shall answer this objection, however, in the words of Mr. Bryan, for we feel that nothing can be more to the point.

> "On what grounds," he says, "is it stated that the cause of the first sound is of equal duration with the sound itself? The sound of a drum is of longer duration than the blow which caused it; a harp string vibrates long after the finger has struck it; and though the shock of the sudden tension of the ventricular valves be instantaneous, yet the vibrations excited by the shock of the sudden tension may continue."

We must now say a few words with regard to the sounds that are heard in the heart under conditions of disease, and see how far they bear out the view we have taken. First, with regard to the phenomena of hypertrophy and dilatation of the heart: under the first condition, the sounds are less distinct and clear than natural; under the latter they become more so. Can we give any satisfactory explanation of these facts under any other theory than the valvular one? We believe not. Were the first sound produced even partly by muscular action, it ought to increase in loudness in proportion to the increased size of the muscle, and diminish in proportion to its diminution. On the other hand, if the sound were due to valvular tension, having to travel through a thickened wall, it would come less sharply to the ear, as in effusion into the pericardium; whereas, in thinning of the ventricles it would be more distinctly heard; and this is what we actually find. We cannot dwell longer on this part of our subject, but we consider that the sounds which result from insufficiency, or a roughened condition of the valves, allowing either of regurgitation of blood, or producing irregular vibrations, thus giving rise to various kinds of abnormal *bruits*, afford strong proof of the truth of the theory we have been considering.

We have thus endeavoured to lay before our readers, what we believe to be the most important points in connexion with the various theories of the causes of the sounds of the heart, and after a careful examination of the different opinions which have been expressed with reference to the subject; after duly considering the numerous experiments and vivisections which have been performed, and weighing the evidence the investigators have adduced in support of their views; after repeated and attentive examination of the phenomena to be witnessed, felt, and heard in the exposed mammalian heart; after a lengthened investigation of the case of fissured sternum which we have referred to; and lastly, after a full consideration of the various morbid sounds which pathological conditions of the heart afford us, we cannot come to any other conclusion, than that the two sounds heard in connexion with the movements of that organ, are the result of the tension produced in the auriculo-ventricular and ventriculo-arterial valves by the shock of the column of blood against them, during the systole of the ventricles and during the reaction of the arteries. We believe that in the healthy condition of the heart, and of the blood, there is no other source of sound, and that every pathological alteration of the sounds may be explained on this theory and on no other. We believe that the only existing physical cause by which a sound can be produced is to be found in the reaction of these valves, that neither the impulse caused by the bulging of the muscle nor the rush of blood from ventricles into vessels, or from auricles into ventricles, nor the muscular contraction or extension, assist in producing *the* sounds of the heart.

We cannot agree with the laborious authors of one of the works placed at the head of this article—MM. Barth and Roger—that this theory is less admirable because it is exclusive, and that because there are several actions going on at the time the first sound is produced, we ought to attribute to each a part in its production. It appears to us that the most philosophical method of dealing with such a question is to ascertain how far each element may in itself assist, and by a rigorous analysis arrive, *par voie d'exclusion*, at the *actual* cause. This we venture to think has been done, and we believe that the promulgation of clear and sound views on the subject amongst our profession, and especially in our various medical schools, will have the effect of removing much of the difficulty which students always experience with reference to the action of the heart and the diagnosis of its diseases. In this respect we speak from the influence this view has had on ourselves, and having devoted much time to its consideration and to experimental inquiry bearing upon it, we can safely say that it has thrown a clear light on points of cardiac pathology to us previously obscure, and materially facilitated our diagnosis of cardiac affections.

The Rhythmic Movements of the Heart.—From the consideration of the sounds of the heart we pass to that of the causes of its rhythmic motion, and here we feel that we are approaching a subject of a very debateable character, and one on which much remains open for investigation. When we find that the heart may be withdrawn from the influence of the nervous centres, that it may be deprived of its blood, that it may even be dissevered from all its connexions and removed from the body, and yet go on performing its rhythmical movements, we are at once driven to look for some cause residing in the organ itself to account for the phenomenon.

That under ordinary conditions of the circulation the heart is dependent for its action on the regular supply of blood, is undoubtedly true, and observation shows that those cavities which are first deprived of the access of blood cease the soonest to contract. It is well known that when the heart becomes enfeebled, and then arrested in its action, the movement does not cease in all parts at the same time, and that the right auricle is the last to lose its activity, because the blood arrives at the right side of the heart after it has ceased to flow into the left. That this prolonged action of the right auricle is the result of the access of blood, and not a peculiar property of the cavity itself, was proved by Haller. By means of ligatures properly placed he prevented the entrance of blood into the right side, and retained a certain quantity in the left; the order of arrest of the movements was reversed; the left ventricle continued to beat the longest.

There is another condition which exists during the circulation, which is supposed by some to influence the periodic contractions of the heart. It is not only that, during diastole of the organ, blood is brought in contact with its internal surface, but it is also freely supplied to the muscular fibres themselves. During their systole, the walls of the heart become more or less pale, the blood is more or less pressed out of their capillaries, but during their diastole it flows freely to the muscular fibres. It has been stated by some physiologists, and especially latterly by M. Brücke, that the arrangement of the semilunar valves of the aorta is such as to prevent the blood passing into the coronary arteries during the ventricular systole, and he even believes that these vessels become emptied by that action. This view has been attacked by M. Hyrtl, who sustains the opinion that the blood passes into the coronary vessels, both during the systole and after the closure of the semilunar valves, and he defends his position, first, on the ground that in injecting the aorta by the pulmonary veins, the coronary arteries always become injected; and secondly, that in an experiment made on a sheath-fish, the coronary artery being divided transversely and isolated, he saw the blood escape during ventricular systole from the superior portion, and not from the inferior. M. Endemann has also shown that in simulating the action of the ventricles, and observing the oscillations of a column of mercury in the manometer placed in one of the coronary arteries, the mercury rose at every mimic systole, proving therefore that the fluid reached the coronary vessels; further, M. Donders has shown that the pulsation of these arteries is synchronous with the systole of the ventricles. We cannot, therefore, but conclude from these results that the blood reaches the coronary arteries during the contraction of the ventricles, but we must nevertheless admit that during this period there is a partial exhaustion of blood from the muscular tissue itself; the blood pressed out of the capillaries is not, however, forced back into the arteries, as appears from the experiment quoted—it must therefore be driven on into the veins; the muscular tissue consequently gets its most abundant supply of blood during the diastole of the heart—in fact, during its period of repose, when the fibres are relaxed and the cavities dilating.

The influence of non-supply of blood to the structures of the heart, in arresting its contractions, was pointed out in 1842 by Mr. Erichsen, and has since been observed by M. Schiff. The former gentleman showed that ligature of the coronary vessels in mammals was soon followed by a cessation of the heart's action, and that the duration of the movements was shortened when the coronary veins were opened so as to facilitate the exit of blood; whilst the contrary was the case when the blood was retained in the proper vessels of the heart. M. Schiff has further succeeded in arresting the action of the right ventricle by cutting off by ligature its supply of blood, whilst the left, remaining undisturbed, continued to contract as usual.

Dr. Brown-Séquard has attempted to explain the movements we are considering, by supposing them to be due to the carbonic acid present in the venous blood of the heart. He says:—"I believe that the beatings of the heart are excited by a principle existing in the blood, and that carbonic acid is that principle." Dr. Radcliffe has also given a similar explanation of these phenomena.

The facts which Dr. Brown-Séquard adduces in support of his position do not appear to us of a conclusive character, for we have no evidence that carbonic acid, when applied directly to the muscular fibre, stimulates it to action. In fact, as remarked by Milne-Edwards:

> "We see by the experiments of M. Castell, that the heart of a frog separated from the body and plunged into carbonic acid gas, does not beat more strongly than in the air, and its movements are arrested much sooner. In the numerous experiments of this author, the pulsations have ceased at the end of eight or even six minutes in carbonic acid gas, whilst they continued during about an hour in azote or in hydrogen, and were prolonged during more than twelve hours in oxygen."

A view of the causes of these rhythmical movements, of a novel character, has been advanced by Mr. Paget, of which we think it right to say a few words. Some of the experiments to which he alludes show very clearly that the power of rhythmic motion does not reside in all parts of the heart alike—that, in fact,—

"If, for example, the cut-out heart (of any of the amphibia) be divided into two pieces, one comprising the auricles and the base of the ventricle, the other comprising the rest of the ventricle, the former will continue to act rhythmically, the latter will cease to do so, and no rhythmic action can be by any means excited in it. The piece of ventricle does not lose its power of motion, for if it be in any way stimulated, it contracts vigorously, but it never contracts without such an external stimulus, and when stimulated, it never contracts more than once for each stimulus.

"Other sections of the heart, and experiments of other kinds, would show that the cause of the rhythmic action of the ventricle, and probably also of the auricles, so long as they are associated with it and not with the venous trunks, is something in and near the boundary ring between the auricles and ventricles, for what remains connected with this ring, or even with a part of it, in a longitudinally-bisected heart, retains its rhythm, and what is disconnected from it loses its rhythm."

Heidenham has shown that the ventricles of a frog's heart separated from the auricles, and placed with some blood in it and around it, will go on contracting, and that circular portions may be cut from its upper border to the extent of about a third of its depth, without arresting the rhythmic movements, which, however, gradually diminish in frequency as the different portions are removed, and at last cease. It would appear, therefore, that there is a certain zone, in which lies that on which the rhythmic action depends. It seems difficult to account for this fact on the supposition which we shall have again to allude to, that the action is due to some power inherent in the muscle alone, but by no means so if we consider it as the result of an influence derived from certain nervous centres or nerve ganglia in the heart, discharging rhythmically nerve-force. Now, the anatomical researches of Bidder, Rosenberger, and others, with reference to the existence of these ganglia, agree very well with this experimental evidence. Injury or removal of those parts where the nervous ganglia are most abundant, interferes with the rhythm, but does not prevent the separate portions of muscle from contracting when stimulated. The contraction of isolated portions of muscle may be due to muscular irritability, but some other power would appear to be required to produce periodic motion.

Mr. Paget is of opinion that these movements are due to nerve agency, and he goes on to ask, How is it that these nervous centres are rhythmic in their action? Why is it that these nervous centres accumulate and discharge nerve-force, as it would seem, not only spontaneously, but at time-regulated intervals?

Widening the basis of his investigations, and taking a general view of the phenomena of organic life, he comes to the conclusion that all rhythmical force is due to rhythmical nutrition. He instances as the simplest example of rhythmic movements yet discovered, that detected in the volvox globatur.

"At certain periods of the development of this simplest vegetable organism, there appear in each zoospore or in the bands of protoplasm with which the zoospores are connected, vacuoles, spaces, or cavities, of about $\frac{1}{3000}$th of an inch in diameter, which contract with regular rhythm at intervals of from thirty-eight to forty-one seconds, quickly contracting, and then more slowly dilating again."

Other instances of such actions taking place in vegetable organisms are mentioned by Mr. Paget, as well as the definite, alternate, and opposite movements of cilia. All these result without the agency of muscular or nervous tissue; they depend upon some peculiar condition, and this Mr. Paget thinks is to be found in the fact that they are the seat of rhythmical nutritive processes. He says:

"But there is another thing common to all rhythmically-acting organs—they are all the seats of nutritive processes, and I believe that their movements are rhythmical, because their nutrition is so; and rhythmic nutrition is, I believe, only a peculiar instance or method of manifestation of a general law of time, as concerned in all organic processes."

Applying the argument to the subject we are considering, Mr. Paget thinks it may be fairly assumed that the rhythmical movements of the heart are due to its rhythmical nutrition.

"Now, if rhythmic nutrition be thus proved as a necessary attendant of rhythmic action, it must be regarded as the cause and not the consequence of the action, for in all cases nutrition has precedence of other actions in organized bodies, and the time regulation of nutrition is a general and principal fact, and is a cause, not a consequence, of many phenomena which we trace in other organs than the heart, and many of which are attended with time-ordered movements."

We have referred to these observations of Mr. Paget at some length, because we consider them of much importance, and that they open a wide view of the various processes of organic life. We do not see, however, that they bring us at all nearer to the proximate cause of the movements of the heart. To say that rhythmic actions are due to rhythmic nutrition, seems to us to be the same as to say that all action or function results from organization, or that organization is the cause of all action; and with reference to the heart, we are still driven to the question, How does the rhythmical nutrition act? Does it give the muscle an inherent power of contraction? or does it enable the nervous ganglia to discharge periodically nerve-force?

And here it seems to us that we arrive at a point beyond which we cannot with our present knowledge go, and on which the opinions of physiologists are divided. That the movements of the heart were due to muscular irritability was the old Hallerian doctrine, and a modified view of this has been supported by Carpenter. He considers the muscular fibres may act without being excited, that they may become so charged with motor force (motility) as to execute spontaneous contractions. He says:

"It is not very difficult to conceive that the ordinary rhythmical movements of the heart may be due to a simple excess of this motility, which is continually being supplied by the nutritive operations, and is as constantly discharging itself in contractile action."

Amongst many French physiologists, of whom we may mention Milne-Edwards, the doctrine of the muscular irritability is most in favour, and the evidence afforded in support of it by the experiments of Claude Bernard and others appears to give weight to the theory. They have shown that it is possible by the administration of certain poisons to arrest the action of the nervous system whilst the muscular irritability remains, and, on the other hand, to annihilate the muscular irritability whilst the function of the nerves is persistent. These experiments do not appear to us decisive of the question, for in those cases where the administration of a poison has resulted in suspending one or more of the properties of the nervous system, other faculties may remain, and we do not know, with reference to the heart, how far the function of its own ganglia may or may not be interfered with. For our own part, we are disposed to consider the theory of the discharge of nerve-force from the ganglia of the heart as the true one. That these ganglia are dependent on rhythmical nutrition for the power they rhythmically discharge we entertain no doubt; and this nutrition must take place during the period of repose which the structures enjoy. The nervous force thus generated is discharged at the proper time, and contraction ensues. That there is at all times a reserve of this force which will keep up rhythmical movements for a longer or shorter period when the source of nutrition is cut off, we can easily understand, and in proportion as the heart, as well as other viscera, is more or less influenced by the great nervous centres, so will there be a greater or less dependence of the organ on other sources, except those within its walls, and so shall we find that its rhythmical action will continue when it is removed from the body, for a longer or shorter time.*

Review V.

1. *On the Origin of Species by means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life.* By Charles Darwin, M.A., F.R.S., F.G.S., F.L.S., &c.—*London*, 1859. Post 8vo, pp. 502.

2. *On the Tendency of Varieties to Depart indefinitely from the Original Type.* By Alfred Russel Wallace. From 'Journal of the Proceedings of the Linnæan Society,' July 1st, 1858.

3. *Essays on the Spirit of the Inductive Philosophy, the Unity of Worlds, and the Philosophy of Creation.* By the Rev. Baden Powell, M.A., F.R.S., F.G.S., Savilian Professor of Geometry in the University of Oxford.—*London*, 1855. Post 8vo, pp. 503.

* Since this article was written, we have received an essay on "The Action and Sounds of the Heart," by Dr. Halford. In addition to the facts brought forward in the papers we have referred to, the essay contains some valuable observations on the sounds of the heart as heard in birds. We are disposed to agree with the author that the facts as detailed by him are quite in favour of the valvular theory, but for further information we must refer our readers to the essay itself, which will well repay an attentive perusal.

4. *Introductory Essay to the Flora of New Zealand.* By Joseph Dalton Hooker, M.D., F.R.S., F.L.S., F.G.S., &c., late Botanist to the Antarctic Expedition.—*London*, 1853. 4to, pp. xxxix.
5. *On the Flora of Australia, its Origin, Affinities, and Distribution; being an Introductory Essay to the Flora of Tasmania.* By Joseph Dalton Hooker, M.D., &c. &c.—*London*, 1859. 4to, pp. cxxviii.

There is no subject more fertile in suggestive questions, and more capable of exercising the highest powers of the mind in the search for answers to them, than Natural History. There was a time, it is true, when the Botanist confined himself to collecting and drying plants, and arranging them in a herbarium according to such notions of their mutual affinities as he might be able to form from their external characters; their collocation, so long as the Linnæan system was in vogue, being generally about as natural as that of the successive articles in an Encyclopædia arranged according to the alphabetical order of their subjects. And the Zoologist of that epoch was content with filling glass cases with stuffed beasts and birds, putting reptiles and fishes into jars of spirit, and fastening down shells, insects, and star-fishes on the tablets of his museum; knowing little and caring less about their internal structure, and considering every other study but that of their external characters as absolutely profitless. Although this type is now pretty nearly extinct, one meets every now and then with an antiquated specimen of it; and its peculiarities are then brought into marked relief, by the contrast they present with the modes of thought which prevail among the best Naturalists of the present epoch.

The first great step in advance was undoubtedly made by those who showed that no classification of Plants and Animals can have any real value, which is not based on a knowledge of their *internal structure:* hence arose the Natural Method of Botanical arrangement, which, originating with Linnæus (who himself looked upon his artificial system as merely provisional and temporary), has been successively elaborated by Jussieu and Decandolle, Brown and Lindley, and other eminent systematists: and hence arose the 'Règne Animal' of Cuvier, and the 'Histoire Naturelle des Animaux sans Vertèbres of Lamarck,' which have constituted the bases of all subsequent attempts at Zoological systematization down to the present time. But within the last quarter of a century a new idea has been introduced into the Sciences of Classification; that, namely, of *development.* It is no longer regarded as sufficient to ascertain all that can be made out of the organization of the perfected type; for the completest knowledge of this, it is now fully admitted, would often leave us quite in the dark as to the real affinities of the organism. It is necessary to study the progressive stages by which that type has been attained; for often it is only in the earlier of these, before the commencement of aberrations which afterwards tend to obscure and perplex those affinities, that its true relations can be unmistakeably determined.

But the aims of the Philosophic Naturalist are not by any means confined to the building up a classification of the existing forms of Animal and Vegetable life. He knows well that however complete may be his collections of the Plants and Animals now existing, they only represent but a fragment of the vast scheme of Creation, which has peopled the globe with continually varying forms of life, during that long succession of geological ages which has elapsed since the remains of organized beings were first entombed in the sediments of the ocean-waters which now constitute the oldest of the palæozoic strata. On bringing together all the fragmentary traces which he can collect of the successive Floræ and Faunæ of the great "formations" distinguished by the geologist as marking separate periods, he finds that they fit in so marvellously with the arrangement of the existing groups,—some of them dropping at once (so to speak) into vacant spaces that seem as if purposely left for them, and others being easily accommodated by a rearrangement which makes the new grouping far more symmetrical than the old,—that they all obviously constitute parts of one harmonious system. But this unfortunately never can be fully understood by Man ; because the utmost skill of the palæontologist, though it may reconstruct a vetebrate animal from a fragment of a bone, a molluska, crustacean, or an echinoderm from a portion of its testaceous covering, a palm or conifer from a fragment of its woody stem, a fern by its leaves, or a cycad by its fruit, cannot thus reproduce any of that innumerable multitude of forms of Animal and Vegetable life which have "died and left no sign" for want of tissues hard enough to resist decay, and to whose past history, therefore, we can never, in the very nature of things, obtain the slightest clue. Circumscribed as they are, however, not only by this unavoidable restriction, but also by many other limitations necessarily arising out of the conditions under which fossil remains are preserved (to some of which limitations we shall hereafter refer), the researches of Palæontologists have been so successful, as at first to have led some of the more sanguine among them to suppose themselves justified in describing the Fauna of each successive epoch as if they had it all before them, instead of possessing such a fragmentary representation of it as any one would form of the Fauna of the present epoch by bringing together the remains of animals dredged from a small area of the sea-bottom in a dozen or two of different localities. The absurdity of the latter procedure would be scarcely more palpable than is that of the Geologists who attempt to go one step further than facts warrant, and who, not content with cataloguing the species they find in any system of formations, assume that all other types of life were absent when these were in process of deposition.

A more philosophical spirit, however, is now prevalent,—thanks, in great part to the labours of Sir Charles Lyell ; and it is coming to be generally felt that the whole fabric of geological doctrine which rapidly grew up during the first third of the present century, needs to be greatly modified to bring it into accordance with the results of those more extended and careful researches in which the second third has been so fruitful. And we do not think that we can better introduce the subject of the very important inquiry opened out to us by the remarkable treatise at the head of our list, than by noticing some of the considerations most directly bearing upon it, which arise out of the existing aspect of the inquiry into the past history of the earth, and the successive steps by which it came to present not merely its present physical features, but the very peculiar distribution of animal and vegetable forms which people its surface. The problem of the geographical distribution of living beings is in fact the one which just now possesses the very highest interest alike to the Naturalist and the Geologist; for it involves the whole question not only of *what is*, but of *how it came to be so;* bearing, in fact, just the same relation to Botany and Zoology *per se*, that Physical Geology does to Geography. And we consider the opening-up of the new ideas and new objects of inquiry in this direction, which we owe especially to the genius of the late Professor Edward Forbes, as the most important advance which has been made in the philosophy of Natural History previously to the publication of Mr. Darwin's treatise on the Origin of Species.

The unequivocal tendency of this inquiry, so far as it has been yet prosecuted, is to make evident the intimacy of the relation between the present order of things and that which preceded it, and the gradational nature of the changes by which the latter has given place to the former. It may be stated with the highest probability, from the evidence of fossil remains, that a very considerable proportion of those classes of animals now living, whose bones or shells afford means of comparison, *are* the direct descendants of animals that existed before the occurrence of those last great changes which gave to a large part of the surface of the globe its present physical features. All save a few palæontologists are now agreed that even in the earliest of the formations which succeeded the Chalk, a considerable number of shells belonging to existing species present themselves; and that the proportion goes on progressively increasing to the present time. In the case of the Mediterranean Fauna, the very curious result appears deducible from a careful comparison of the present with the former distribution of its mollusks, that all the existing species proper to it have come down from that very remote period when it was a great inland lake, these being found fossil in the successive tertiary deposits of its shores; whilst those whose descent cannot thus be traced are immigrants from the Atlantic, as is indicated not merely by their identity with species characteristic of the Boreal, Celtic, Lusitanian, and West African provinces respectively, but also by the progressive diminution in their proportional abundance as we trace them from the Straits of Gibraltar towards the Levant. The changes in climate which have favoured this intermixture (the extension of a glacial temperature to the south of Europe, for example, having at one period brought the Boreal fauna down to the entrance of the Mediterranean) have caused the extinction of many of the earlier tertiary species of the Mediterranean Province; and thus its present Fauna has come to differ widely from that of the early tertiary period, without affording any evidence of a "new creation" of species.* Now this modification has obviously been the result of geological changes of the most gradual nature, which have been in continuous operation through the whole of the tertiary period, and which have left their traces (as Professor E. Forbes long since showed) upon the vegetation of Europe, as well as upon the distribution of its Marine Animals.

In like manner it has been recently shown to be an almost inevitable deduction from the present distribution of land animals in the Malay Archipelago, that most important and extensive geological changes have taken place since the islands at present forming that Archipelago were peopled with their existing inhabitants. The two western and eastern halves of that Archipelago, the former containing Sumatra, Java, and Borneo, the latter including Celebes and New Guinea, are separated at their nearest approximation by the Straits of Lombok, which are no more than fifteen miles wide; the fauna of the former is essentially Asiatic, that of the latter essentially Australian; and there is no other intermixture between them than such as a very limited migration across this narrow channel will readily account for. Now the various portions of the Indian province are still connected by a vast submarine plain, which extends over the whole of the Java Sea, the Straits of Malacca, the Gulf of Siam, and the southern part of the China Sea, at a depth of not more than 300 feet, abruptly terminating at its limits in an unfathomable ocean. An elevation of the sea-bottom to this amount, therefore, would nearly double the extent of tropical Asia; and there is every probability that the continent *was* thus extended before that last great elevation of the volcanic range of Java and Sumatra took place, which (according to the general fact first brought into notice by Mr. Darwin, of an alternation of bands of elevation and depression) was coincident with the subsidence that separated those islands from Borneo on the one side, and from the continent of Asia on the other. On the other hand, the great Pacific Continent, of which New Guinea and Australia are doubtless fragments, and which (as Dr. Hooker has rendered probable by botanical considerations) once connected Australia and New Zealand with South America, seems to have extended itself as far westward as the Moluccas; and *its* submergence, producing the limitation and separation of the great islands of the South sea, seems to have taken place before the rise of the tropical Asiatic continent.† There are even indications that the tropical Indian continent extended so near to what is now the coast of Africa, that Bourbon and the Mauritius, perhaps even Madagascar, were outlying portions of it; and if the submergence which formed the bed of the present Indian Ocean should have taken place subsequently to the time when these countries became inhabited by Man, we have a rational explanation of the fact which has perplexed all ethnologists, and which the hypo-

* See the recently published "Natural History of the European Seas," by the late Professor Edward Forbes and Robert Godwin-Austen.

† See Wallace "On the Zoological Geography of the Malay Archipelago," in Proceedings of the Linnæan Society for Nov. 3rd, 1859.

thesis of migration can scarcely be stretched far enough to account for,—that the languages of Madagascar are not African but Malayo-Polynesian in their fundamental affinities.

Now, there is no reason whatever for the belief that what is true of the later, is otherwise than true of the earlier periods of Geological history. The more we know of the nature of that history, the more obvious does it become that it is one of continuous sequence, not of fits of alternating activity and repose. This was well expressed thirteen years ago by the then President of the Geological Society, Mr. Leonard Horner:

> "By whatever names we designate geological periods, there appear to exist no clearly-defined boundaries between them in reference to the whole earth. Such a marked line may be seen in particular localities; but every year's experience, and our more intimate acquaintance with the phenomena exhibited in different countries, and with the distribution, structure, and habits of animals and vegetables, teach us that there is a blending, a gradual and insensible passage from the lowest to the highest sedimentary strata, particularly in respect of fossil remains. The terms we employ to designate formations can only be considered as expressing the general predominance of certain characters to be used provisionally, as a convenient mode of classifying the facts we collect."

And what was thus foreseen by a sagacious reasoner upon the facts then known, has received the fullest confirmation from the results of subsequent researches; which have uniformly tended to show that the supposed boundaries are local, not universal, and that even the widest chasms close together if we trace them far enough.

Again, it may be considered as a legitimate deduction from recent Palæontological inquiry, that it is altogether unphilosophical to attempt to fix the epoch when any particular type of animal or vegetable life first appeared upon the earth. Not one such determination has been found to stand the test of more extended research. It was at one time the orthodox creed that no Mammal was created before the commencement of the Tertiary period, and no Reptile before the middle of the Secondary; simply because no remains of such had been found in the few and limited explorations then made. But we have now abundance of remains of Mammals in the Secondary period, some of them dating back to its commencement; whilst of the existence of Reptiles there is evidence very far back in the Palæozoic. Who now will be bold enough to say that there were no Mammals earlier than the New Red Sandstone, or Reptiles earlier than the Old Red?

Again, the prevalent notion that particular species are to be held as characteristic of particular strata, has been shown to require great modification by the discovery that many are really common to a long series of stratified deposits, not even being limited to the great "formations;" and that the time and order of their appearance are by no means the same in different parts of the globe. Thus, among the Palæozoic species common to Europe and to America, some are found to make their appearance first in Europe, others first in America; so that the order of their succession is reversed in these two regions.

Still more remarkable is the recent discovery of M. Barraude, that species hitherto considered as peculiar to and characteristic of the newer palæozoic, present themselves in "colonies" (as he not very appropriately terms them) in the midst of those of the older, which afterwards replace them. It is obvious that in all palæontological reasoning, large allowance has to be made for change of geographical distribution. A species or group of species may entirely disappear from one province, in consequence of climatic or other change, and yet may have its existence continued in some other region to which it has retreated. Thus there are many species of shells found fossil in Europe, representing its fauna during the Glacial period, which are now met with alive only in the Arctic seas. On the other hand, of the large Foraminifera which built up the Nummulitic limestone of the Paris basin and of Southern Europe in the early Tertiary period, when the climate seems to have been much warmer than at present, though the greater part may have become extinct, yet some species still exist in the Pacific ocean, and are now building up reefs and islands there, which a geologist of some future epoch, relying too much on the identity of specific forms, might regard as contemporaneous with the great nummulitic formation of Europe. Thus the prevalent idea that there was a new and special creation of species with every one of those marked changes in the physical surface of the globe which has given rise to a distinct "formation," proves to be inconsistent with truth; the difference of Fauna between one formation and another being often, in great part at least, the result of migrations occasioned by alterations in climate or in those other conditions which affect the existence of animals.

Another general fact of great importance in this inquiry, is the constant correspondence which presents itself, alike as to similarity and to difference, between the physical conditions under which consecutive strata were deposited, and the collective aspect of the organic life which is made known to us by the remains they entomb. Every one who possesses but a smattering of geological knowledge well knows that each great "formation," such as the Silurian among the palæozoic, or the Cretaceous among the mesozoic, is really made up of a long series of stratified deposits, which are often very different in mineral characters, but which are for the most part conformable to one another stratigraphically, and of which the fossil Fauna and Flora present the same general features. Yet with this general conformity we encounter a marked change in detail, in passing from the beginning to the end of the series; this sometimes appears abrupt enough in this country to constitute a decided break, such as that which has been supposed to intervene between the Upper and Lower Silurian or the Upper and Lower Chalk; but in other instances a very close conformity is maintained throughout, even where identity ceases, by the successive appearance of what have been termed "representative species." From what has previously been stated, it is obvious that one essential difference between the Upper and Lower Silurian can no longer be maintained; and a careful examination of the Cretaceous series in localities where it is more complete than in our own country, shows that it may be divided into eight stages, each having a fossil Fauna of its own, which, though peculiar as to its species, yet bears so extremely close a resemblance to that which preceded it, as strongly to suggest, even to such an orthodox believer in the immutability of species as Prof. Pictet, the notion of its derivation from it by direct descent.

Where, on the other hand, there is a marked change in the type of Life between successive deposits,—such, for instance, as that which distinguishes the Devonian formation from the Silurian that preceded it and from the Carboniferous that followed, or the Oolitic from the Triassic and the Cretaceous,—there is always ample evidence of vast intervening changes in the physical conditions under which those deposits were formed. And this evidence seems the most complete (in those areas at least which have been hitherto most carefully examined) in regard to those two great interruptions to the general continuity of the series, which are considered by Geologists to divide the Palæozoic from the Secondary, and the Secondary from the Tertiary. But we feel sure that we speak the conviction of all such Geologists as are not so far wedded to their earlier notions as to be unable fairly to estimate the merits of more recent views, when we say that they look with confidence to future discoveries as likely to bridge over both these chasms; important advances having been made, indeed, within the last few years. Thus the current doctrine has been, that the true palæozoic forms all became extinct with the completion of the Permian formation; and that the formation of the Triassic or New Red Sandstone commenced the Secondary period with a great scantiness of animal and vegetable life, which gradually gave place to the abundance of new forms characterizing the Middle and later Secondary period. But it now appears from the careful study of the remarkable beds belonging to the *Upper* Trias at St. Cassian in the Austrian Alps, that the fossil fauna of that period is really extremely rich; its supposed scantiness being simply due to the fact that in England, France, and Upper Germany the Upper Trias is chiefly represented by beds of fresh or brackish water origin. Now the St. Cassian beds, which are marine, contain a large number both of those Palæozoic forms which had been supposed to have died out long before, and of those Secondary forms which had been regarded as of much later introduction; thus showing the really gradational nature of the transition from one fauna to the other. And, as Sir C. Lyell justly remarks, "we can now no longer doubt that, should we hereafter have an opportunity of studying an equally rich marine fauna of the age of the *Lower* Trias, the great discordance between Palæozoic and Neozoic forms would almost disappear, and the distance in time between the Permian and Triassic eras would be very much lessened in the estimate of every Geologist." So the transition from the Secondary to the Tertiary series appears likely to be made by the great Nummulitic formation of Southern Europe and its associated beds, when these shall have been thoroughly worked out.

We cannot better sum up the results of the inquiries to which we have alluded, than in the words of Prof. Powell :—

"In all those geological periods during which we *can* trace a continuous and gradual succession of formations without marked or violent interruptions, there we invariably find a like slow and gradual change of animated life, proceeding by small modifications of *species*, until at length, comparing the extremes of the series, whole genera may be changed. If, then, in certain other cases, we find apparent *interruptions in the order of species*, apparent breaks in this orderly succession, or between such deposits of so different a character, periods intervening, during which we see that great changes or disturbances were in progress, as we must infer that those changes went on by the regular operation of physical laws, exactly as in the cases in which we *have* uninterrupted evidence,—so, by parity of reason, we must infer that the like gradual and regular changes of species went on during those periods, though all its intermediate links and steps are lost to us, and only the extreme terms are preserved. . . . . *A wide organic difference between two contiguous beds would only mark the longer interval of time between their deposition.*" (pp. 346–349.)

How entirely destitute we are of any title to draw inferences as to what forms of Plants or Animals *did not exist* at any particular epoch, from the apparent absence of their fossil remains, is every now and then made obvious by some unexpected discovery which throws an entirely new light on the history of the period. The researches of thirty-six years—from 1818, when first a lower jaw from the Stonesfield Oolite was pronounced by Cuvier to be Mammalian, to 1845, when the Spalacotherium of Purbeck was described by Owen,—had only disclosed the existence of six species of Mammalia in the whole world from rocks older than the Tertiary. Yet in 1856 and 1857 the careful examination of the thin seam of the Purbeck strata in which the remains of the Spalacotherium were found, brought to light an accumulation of bones of small Mammals, chiefly Marsupial, some insectivorous or predaceous, one purely herbivorous, and others of doubtful affinities, such as clearly shows that there must have been a great abundance and variety of Mammalian life at the period when this bed was deposited. And what makes the lesson the more instructive is the fact, that the Purbeck strata had been previously supposed to have been thoroughly studied by such excellent geologists as Prof. E. Forbes (who worked at them for months consecutively) and by other skilful collectors; that their fossil remains had been separately examined and catalogued by the officers of the Government Survey; and that from the circumstance of their being nearly all of fresh-water origin, yielding insects and fruits, with the stems and roots of trees, it had been anticipated that they would be likely to furnish remains of terrestrial quadrupeds, if any such had existed in that region when these beds were deposited. And yet, though thus interrogated by skilful inquirers, the rocks were silent; until one thin layer of a few inches in thickness—like a single page in a pile of volumes heaped to the height of a mountain,—revealed the memorials of fossil mammalia so numerous and diversified as not merely to surpass those found in all the other secondary rocks put together, but to outnumber those at present known from many a subdivision of the tertiary series.

So, again, the received canon as to the non-existence of Man upon the globe until the completion of the last great changes which gave to its surface its present aspect, has lately been overthrown by the discovery of unquestionable specimens of his handiwork under circumstances which necessitate our carrying back his origin to a period anterior to that at which extensive and important changes of level took place, forming a series of heights through which new river-beds have since had to cut themselves,—a process which, according to all rational probability, must have occupied almost as many thousands of years as Man is commonly supposed to have lived centuries. And though the inference can not yet be regarded as certain, there is a strong probability that the men who shaped the flint implements to which we refer, were contemporaneous with the Mammoth, the Ticborhine Rhinoceros, and other extinct Mammals whose bones have been found associated with these implements in the same gravel deposits or in the contents of the same caves.

Now when due weight is given to these and other considerations of the like tendency, it obviously becomes very difficult to form any rational conception as to the introduction of new types of organic life in any other mode than by *descent with modification* from those previously existing. We know that physical changes of the same order with those which formerly modified the condition of the earth's surface, are still in progress; if new *creations* of species have taken place from time to time even subsequently to the introduction of man, why should they not occur now? Yet would any one be bold enough to affirm that such new creations occur in our own day? It is true that if a collector meets with a form not previously described, he entitles it a "new species;" but by that title he means only a species new to science; and he would not on any account be thought to imply that it has not existed from the beginning of the present order of things.

As Dr. Hooker has well remarked :—

"The boldest speculator cannot realize the idea of a highly organized plant or animal starting into life within an area that has been the field of his own exact observation and research; whilst the more cautious advocate hesitates about admitting the origin of the simplest organism under such circumstances, because it compels his subscribing to the doctrine of the 'spontaneous generation' of living beings of every degree of complexity of structure and refinement of organization."

And he adds in a note:

"It is a curious fact (illustrative of a well-known tendency of the mind) that the few writers who have in imagination endeavoured to push the doctrine of special creations to a logical issue, either place the scene of the creative effort in some unknown, distant, or isolated corner of the globe, removed far beyond the ken of scientific observation, or suppose it to have been enacted at a period when the physical conditions of the globe differed both in degree and kind from what now obtains; thus in both cases arguing *ad ignotum ab ignoto*."*

The *extinction* of species is now universally admitted to be a gradual

* Flora of Australia, p. xxvi.

process, depending upon a variety of agencies, of which sometimes one, sometimes another plays the principal part. Many examples of it have occurred during the short period which has elapsed since the interposition of Man has disturbed the previous equilibrium. And no one would now dream of calling in the aid of general destructive catastrophes to account for these successive disappearances, which have been coincident with the successive appearances of new forms at past epochs. On that old doctrine of a succession of convulsions, each of which swept the globe of its living inhabitants, and left it ready to be re-peopled afresh, there was no more difficulty in imagining a general renewal of the creative *nisus*, than in conceiving of that by which the first-created forms were introduced. But the hypothesis of occasional and general cataclysms having now given place to an induction based on a far surer foundation of evidence—that, namely, of a continuity of change, more rapid in some regions, less sensible in others, but not less certainly in progress at the present epoch than in times past,—it seems almost necessarily to follow (as has been extremely well urged by Professor Baden Powell) that the succession of forms of Organic Life has been alike gradational, both as to the extinction of the old and the production of the new, and has been determined by causes still operative. If any one should be bold enough to maintain that a production of animals or plants *de novo* does every now and then occur within human experience, he would be fully justified in attributing the introduction of new forms at any antecedent period to a like agency. But if the notion of such new developments in our own period be scouted as unscientific, presumptuous, atheistical, and the like, it is for the advocates of successive creations in past times to show that they deserve any other character. If, on the other hand, it can be shown that the existing forms of Plants and Animals have undergone such modifications within the limits of human experience, as to justify the idea that in a longer succession of ages and under a greater diversity of conditions, those modifications might have been carried to the extent of producing differences such as those by which species and genera are ordinarily distinguished, it is obvious that a legitimate basis is afforded for the inquiry whether this has not to be accepted as a *vera causa* adequate to account for the phenomena of palæontological succession, and whether the hypothesis of successive creations of living beings has really any better foundation than that of a succession of destructive convulsions.

We believe that the time is now fully come for such an inquiry to be taken up and prosecuted to its utmost limits. The subject, it is true, is by no means a new one, and is popularly believed to have been disposed of by the refutation of the fallacies of those who in times past have advocated the doctrine of the transmutation of species. With this doctrine the name of Lamarck is commonly and not unjustly associated; not because it originated with him, but because he first gave it a scientific aspect, and advocated it on the basis of an extensive and profound acquaintance with the Natural History both of Plants and Animals. But to this association no small amount of ridicule and of misrepresentation is attached, of which we feel called upon to take this opportunity of examining the grounds. In enumerating the causes which tend to produce modifications of animal form and structure, Lamarck unfortunately laid great stress upon the efforts which the being would itself make to execute some new action, as causing the development of an appropriate organ,—the attempt to fly, for example, bringing wings into existence, the swimming of fishes causing the production of fins, and the continual stretching of the giraffe's neck in reaching the food it most liked being the cause of its elongation. Now this absurdity is often quoted as the essential part of Lamarck's theory, whereas it is only an accessory part of it, applicable to the animal kingdom alone, and especially to its higher types. All that Lamarck says of the tendency to vary, which shows itself in Plants and Animals generally, and which is the real basis of his doctrine of transmutation, might be urged by the most philosophic botanist or zoologist of the present time.

Again, the doctrine of transmutation is commonly regarded as atheistical; and Lamarck has been branded as an atheist for upholding it. Yet nothing can be more unfair, as is obvious from his own very explicit statement on the subject:

"Doubtless," he says, "nothing exists but by the will of the sublime Author of all things. But can we assign to him rules in the execution of his will, and fix the method which he has followed? Has not his infinite power been able to create *an order of things* which should successively give existence to all that we behold, as to all that which exists but of which we have no cognizance? Assuredly, whatever may have been His will, the immensity of His power is always the same; and whatever be the manner in which that Supreme Will has been exercised, nothing can detract from its greatness. Reverencing, therefore, the decrees of that Infinite Wisdom, I limit myself within the boundaries of a simple observer of nature. Hence, if I should succeed in clearing up any part of the course which it [Nature] has followed in effecting its operations, I shall say, without fear of deceiving myself, that it has pleased its Author that it should have this faculty or that power."

What can be more truly philosophical, or more truly religious? We cannot suppose that the virulent detractors from the merits of this great man,—who was not merely a Botanist of vast acquirements and did much for the establishment of the Natural System, but who possessed a knowledge of the Invertebrate Animals far surpassing that of Cuvier,—can have read more than those sections of his work which fairly lie open to adverse criticism; the larger part of them, we feel pretty sure, know it only at second hand; and we believe that the day is not far distant when it will be admitted that his great misfortune was in living in advance of his time. Nothing can be more clear and precise than his advocacy of that doctrine of local and gradational change (in opposition to the then current notion of general catastrophes), the establishment of which will carry the name of Sir Charles Lyell down to posterity as that of the great reformer of Geological science. The intimate connexion on which we have been insisting, between the successional modifications of the physical conditions of the

* Philosophie Zoologique, tom. i. p. 56.

globe, and the changes which its living inhabitants have undergone,—a connexion whose intimacy speaks strongly of a causative relation between the two orders of facts,—was discerned by the sagacity of Lamarck, though he had not a tithe of the present evidence on which to rest it. In short, there is scarcely a consideration suggested by the recent progress of Geological inquiry which his far-sightedness had not glimpsed; and the Philosophic Naturalist may still have recourse to his much-abused work for suggestions of the highest value in the prosecution of the inquiries to which his attention must now perforce be given.

It has been unfortunate for the doctrine of "transmutation," that its most prominent advocate in our own day should be an author much more distinguished for the ingenuity of his reasonings and the cleverness of his style of exposition, than for the accuracy of his knowledge of facts. We have ourselves felt called upon to criticise with some severity the shallow assumptions and specious arguments of that brilliant but unsound book 'Vestiges of the Natural History of Creation;' but our criticism was less directed to the fundamental doctrine, than to the grounds on which it was advocated; and we have uniformly done our best to resist the clamour raised by theological prejudice against the book and its author, on the asserted ground of their irreligious character. To us it has always appeared that the question ought to be discussed upon its scientific merits alone, and that the evidence of Creative Design is just as great upon one hypothesis as upon the other. Nobody would think of advancing it as an objection to modern Embryology, that it teaches that the human infant, instead of first coming into existence as a fully-formed though minute *homunculus*, begins life in the condition of the simplest protozoon, and successively acquires those peculiarities of organization which end in constituting him a Man. And we do not suppose that the naturalist who first found out that butterflies and beetles were caterpillars in the earlier stage of their existence, instead of coming out from the egg in the full possession of their insect attributes, was considered on that account less religious than his neighbours. Why, then, should it be regarded as impious to maintain that an analogous development went on during what may be called the life of the world; and that the existing forms of Plants and Animals have originated by genetic descent with modification from those which preceded them, even as the latter did from yet older forms, and so on, back to the beginning of Life on our planet? To deny that such *might* have been the Will of the Creator, is virtually either to deny that His power is constantly exerted in maintaining that regular succession of similar forms, on which the notion of the "permanence of species" is based, or to set limits to the exercise of that power, by asserting that it could not have been exerted in any other mode than that which Man chooses to prescribe.

We could cite passages from the recent writings of many men of high scientific reputation,* which would show that they regard the question of the immutability of species as by no means settled in the

* See, for example, Sir H. Holland's Medical Notes and Reflections, p. 22.

affirmative. And we know, from the many expressions of concurrence in the fundamental principles now advocated by Mr. Darwin, which the publication of his views has called forth, that there was a more general preparedness for their reception than had been supposed; the minds of thoughtful men being open to any suggestion which should furnish a clue that might help us to trace the connexion between existing and antecedent races, and might bring into reconcilement past modification and present fixity.

It is not a little singular that the same solution of this problem should have independently occurred to two Naturalists, Mr. Darwin and Mr. Wallace; each having been apparently led to it by the study of the phenomena of the geographical distribution of animals. In the philosophic mind of Mr. Darwin (there are few men of science in our own country at the present time, who have so justly earned a title to the honourable designation of philosopher), the idea was at first cautiously entertained; it was gradually developed into a systematic form, and subjected to a great variety of tests; and when its author had satisfied himself of its soundness, he applied himself for several years, during such time at least as his feeble health permitted him to labour, to the preparation of a work which should contain not only an exposition of his views, but a full statement of the evidence on which they are based. In the mean time, however, Mr. Wallace transmitted from the East Indian Archipelago, the scene of his zoological labours, a memoir containing a concise exposition of the very same doctrine; and the publication of this memoir in the 'Proceedings of the Linnæan Society' was accompanied by extracts from Mr. Darwin's work, which had been written several years previously. The importance of at once giving to the world a fuller statement of his views than those extracts afforded, was urged upon Mr. Darwin by his friends; and it is to these circumstances that we owe the earlier appearance of a more compendious treatise on the Origin of Species than that which Mr. Darwin had originally planned, and which he still hopes to produce so soon as his health and strength allow him to complete it.

The fundamental positions taken up by Mr. Darwin may be concisely stated as follows:

1. Although it is the general fact that the characters of each type of Plant or Animal are transmitted unchanged from parent to offspring, yet trivial departures from that type are continually presenting themselves, and more important variations every now and then occur.

2. So long as a free intermixture of individuals is kept up, and external circumstances remain unchanged, the larger as well as the smaller variations are usually merged (so to speak) in the general average, and the specific type remains unaltered.

3. If, however, the individuals which are distinguished by any peculiarities of conformation be kept separate from the rest, and be caused to breed together, those peculiarities will be established and perpetuated as the characters of a new race, which will remain distinct from that of the parent stock so long as it is not allowed to breed with

it. It is by such *artificial selection* that all our breeds of domesticated animals have been engendered; the breeder taking advantage of any new peculiarity which he thinks he can turn to useful account, and keeping the animals which present it apart from all others, unless he aims, by "crossing" his breed with some other, to get rid of some undesirable feature, or to introduce some desirable attribute.

4. Although the condition of feral or wild races is so entirely different from that of the races under the influence of Man, as at first sight to exclude the notion that the perpetuation of varieties can be effected by any such selective agency, yet that very difference of conditions brings them under a new set of influences, which will tend to produce an analogous result. The life of all wild animals is a *struggle for existence;* and their relative abundance and power of maintaining their ground is determined far less by their relative fertility, than it is by their power of resisting the agencies constantly at work for their destruction. Those, therefore, which possess the organization that confers the largest amount of such resisting power, will be those that will be likely to survive the longest and to propagate their kind. The more weakly individuals, or those which have some peculiarity of organization which (under the circumstances) places them at a disadvantage, will be earliest removed, their places being filled up by the offspring of the more vigorous.

5. Thus so long as the external conditions which affect the existence of any race remain unaltered, the characters of that race will not tend to change, from the time when they have once been brought by this process of *natural selection* into the fullest harmony with those conditions; and thus a species may remain permanent for any number of years or ages, simply because no new form has arisen that could surpass the old in the perfection of its adaptation to surrounding conditions.

6. But if those conditions should undergo a change, the harmony previously existing between the constitution of the race and the circumstances under which it exists is disturbed; and that disturbance may be such as to occasion the extinction of the race, unless it shall possess within itself some power of accommodating itself to the change. That accommodation may take place in two ways; either by the direct influence of external conditions in modifying the constitution of the race (as where it is subjected to a change of temperature or of atmospheric pressure); or by the process of *natural selection*, which will no longer tend to maintain the original type, but on the contrary to bring about and establish a modification of it. For if among the aberrant forms that present themselves from time to time, any should occur which are more in harmony with the new conditions of the species than is that of the original type, then the individuals possessing that conformation will have the advantage in the struggle for existence, and will consequently maintain their ground, whilst their less pliable relatives are (as brother Jonathan would phrase it) "improved off the face of the earth." Thus a new race will come to take the place of the old, just as a new breed of domestic animals having superior qualities super-

sedes that from which it was derived; the only important difference between the two cases lying in this, that the *artificial selection* practised by Man has for its purpose to perpetuate only those qualities which he regards as likely to be useful to himself, and which are for the most part such as would be rather disadvantageous than otherwise, if the race had to maintain its existence by its own unaided resources; whilst the process of *natural selection* operates for the good of the race *per se*, and tends to bring it into its highest state of perfection as a self-sustaining and independent aggregate.

7. Thus, then, a species which has presented the aspect of permanence for any length of time, may be caused to undergo a change at any period, and may continue to present that varied form for a long succession of ages, undergoing a further departure from the original type whenever a fresh change in the conditions of its existence shall occur.

8. The question of degree of modification thus comes, in Mr. Darwin's view, to be only one of *time;* and he holds that any amount of change of type is conceivable on the principle of natural selection, if an unlimited lapse of ages be allowed for its operation. On this principle he would trace back all the species of one genus as derived by direct descent with modification from a single prototype; all the genera of one family, in like manner, he regards as having had a common ancestor still more remote; and by parity of reasoning he would derive all the orders of one class, and even all the classes of one sub-kingdom, from the same stock; thus reducing the probable number of primordial forms of animals to some four or five, or even, carrying the same analogy still further, to a single one.

The facts and arguments by which these positions are supported, are set forth in the first five chapters of Mr. Darwin's treatise; wherein are considered (1) the Variation which occurs under Domestication, (2) the Variation which occurs under Natural Conditions, (3) the Struggle for Existence which all living beings, Plants as well as Animals, have to maintain, (4) the operation of Natural Selection, and (5) the Laws of Variation. We shall not offer our readers a detailed analysis of these admirable chapters, because we think it much more fitting that such as desire to make themselves thoroughly acquainted with Mr. Darwin's views should have recourse to his own very lucid and readily accessible exposition of them. And we shall only say that we should strongly suspect either the intellectual capacity or the candour of any man, who should attentively peruse them without being strongly impressed by the cogency of the considerations adduced by Mr. Darwin in support of his fundamental principle of Natural Selection. For ourselves we do not hesitate to say that they appear to us of irresistible force; but the acceptance of the principle by no means involves the acceptance of the conclusions which Mr. Darwin deduces from it; and, as we shall hereafter endeavour to show, the question of the community of origin of the higher groups, such as orders, classes, and sub-kingdoms, is one of a very different nature from that of species, genera, and even families.

To those who have been accustomed to look upon *species* as natural types of form definitely marked out by fixed characters which are transmitted without modification or variation from parent to offspring, it is necessary first to prove that a tendency to variation exists, as well in a state of nature, as under the artificial circumstances of cultivation and domestication. Having ourselves made this inquiry a special object of pursuit from the time that our attention was directed to it by Dr. Prichard five-and-twenty years ago, we feel ourselves in a position to affirm without hesitation, that those Naturalists who possess the most comprehensive acquaintance with any department of Zoology or Botany are those who are most disposed to admit the existence of wide variation; the multiplication of species distinguished by minute shades of difference having been the work of that class of men, who think that Natural History essentially consists in collecting and cataloguing.

It is very fortunate for our present purpose that the publication of Dr. Joseph D. Hooker's Introductory Essay on the 'Flora of Australia,' which has been prepared subsequently to the first enunciation of Mr. Darwin's views, enables us to place before our readers the testimony of the man who has probably the highest claim of any living botanist to speak with authority upon this question, so far as the Vegetable Kingdom is concerned. Having received a thorough scientific training from his distinguished father, the former Professor of Botany at Glasgow and present Director of Kew Gardens, Dr. J. D. Hooker has largely profited by the opportunities he has enjoyed of visiting many different countries and of studying the vegetation of various regions; and having been led by his own taste to make the geographical range of species an object of special study, he has had the advantage not only of being able to collate his results with those furnished by the largest and best-named botanical collections in the world, but also of receiving a larger amount of assistance from his fellow-naturalists than falls to the share of most.

The total number of species of Flowering Plants known to exist upon the surface of the globe has been estimated by different botanists, upon data pretty much the same, at 150,000, at 100,000, and at 80,000. In Dr. Hooker's opinion, 50,000 would be much nearer the mark. Among the examples which he has given (in his Introduction to the 'New Zealand Flora') of the fallacious methods on which the higher estimates have been constructed, we find that no fewer than nineteen species have been made of the common *Potato*, and many more of *Solanum nigrum* alone; that the *Pteris aquilina* (the common brake fern) has a different botanical name in almost every country in the world; that the *Vernonia cinerea* of India has given rise to at least fifteen book-species; and that many of the commonest European plants have several names in Europe, others in India, and still others in America,—to say nothing of the host of garden names for themselves, their hybrids and varieties, all of which are catalogued as species in the ordinary works of reference whence such estimates are compiled. The case of *Oxalis corniculata* is peculiarly instructive: of this, which is one of the most widely diffused and variable plants in the world, no fewer than eight species are made by Cunningham and Richard in their 'New Zealand Flora,' whilst they actually exclude from it the plant whose varieties have given rise to all these.

It is obvious to the intelligent looker-on, that the multiplication of botanical species is the work chiefly of persons who have confined their attention to some local Flora, and who have very little acquaintance with anything beyond; whilst the reduction of the species thus created is subsequently accomplished by the careful comparison of similar plants brought from remote localities, and especially by the comparison of what are obviously varieties in one country with the reputed species of another. Thus in apologizing to the local botanists of New Zealand for the abolition of their eight species of Oxalis, which they affirm to be distinguished by the constancy with which they retain their states under varied conditions, Dr. Hooker says:

"I value such facts very highly, and attach great weight to them, and did these varieties occur only in New Zealand, I should perhaps have withheld so strong an opinion on the subject; but such is not the case, for *O. corniculata* varies as much in numerous other parts of the world; and admitting, as every one must, that varieties are known to retain their characters with more or less of constancy for certain periods, some other evidence is necessary to shake the opinion of the botanist who grounds his views on an examination of the plant from all quarters of the globe."

The following is another example of the same kind, the special value of which for our present purpose consists in the general remarks which are drawn from Dr. Hooker by his reference to it:

"I have been led to dwell upon this point, because I feel sure the New Zealand student will at first find it difficult to agree with me in many cases, as for instance in so protean a Fern as *Lomaria procera*, whose varieties (to an inexperienced eye) are more dissimilar than are other species of the same genus. In this (and in similar cases) he must bear in mind that I have examined many hundred specimens of the plant, gathered in all parts of the south temperate hemisphere; and have found, after a most laborious comparison, that I could not define its characters with sufficient comprehensiveness from a study of its New Zealand phases alone, nor understand the latter without examining those of Australia, South Africa, and South America. The resident may find two varieties of this and of many other plants, retaining their distinctive characters within his own range of observation (for that varieties often do so, and for a very uncertain period, both when wild, and also in gardens, is notorious), and he may perhaps have to travel far beyond his own island to find the link I have found, in the chain of forms that unites the most dissimilar states of *Lomaria procera;* but he can no more argue thence for the specific difference of these, than he can for a specific difference between the aboriginal of New Zealand and himself, because he may not find intermediate forms of his race on the spot. We do not know why varieties should in many cases thus retain their individuality over great areas, and lose them in others; but the fact that they do so proves that no deductions drawn from local observations on widely distributed plants can be considered conclusive. To the amateur these questions are perhaps of very trifling importance, but they are of great moment to the naturalist who regards accurately-defined floras as the means for investigating the great phenomena of vegetation; he has to seek truth amid errors of observation and judgment, and the resulting chaos of synonymy which has been accumulated by thoughtless aspirants to the questionable honour of being the first to name a species."*

We would fain hope that Dr. Hooker is correct in saying that "the time is happily past when it was considered an honour to be the namer of a plant;" but we fear that there are still too many who are scarcely yet able to appreciate the truth of an excellent remark, which he has unfortunately consigned to a note instead of giving to it the prominence which it deserves, that

"The botanist who has the true interest of science at heart, not only feels that the thrusting of an uncalled-for synonym into the nomenclature of science is an exposure of his own ignorance and deserves censure, but that a wider range of knowledge and a greater depth of study are required, to prove those dissimilar forms to be identical, which any superficial observer can separate by words and a name."

The error of the ordinary species-maker consists in basing his idea of a plant upon the form and aspect which it presents in a small number of specimens collected within a limited area; he makes no allowance for the effects of local peculiarities in temperature, humidity, soil, or exposure, unless he can absolutely trace the cause to the effect; and hence he attaches great importance to habit, stature, colour, hairiness, outline of leaves, period of flowering, &c., all of which characters are recognised by the more experienced botanist as pre-eminently liable to be affected by external conditions. A truly philosophical systematist like Dr. Hooker, on the other hand, bases his conclusions on the most extensive comparison he can make, not only of dried specimens in herbaria, but of living plants in all latitudes; and thus he comes to acquire a knowledge of the influence of external agencies, not only upon the general phenomena of vegetation, but also upon individual forms. It has been after this fashion that Mr. Bentham has studied the British Flora; with the result of annihilating about a fourth of its reputed species. And the more thoroughly and extensively this method is carried out, the more, it is now obvious, will it tend to simplify botanical science, by reducing the number of really distinct specific types, and clearing out from our systematic treatises the vast mass of rubbish with which they have been crowded by the unscrupulous creativeness of species-mongers.

Dr. J. D. Hooker's investigations early led him to oppose the common practice of fixing upon some one particular form, out of many varieties, as the original type of a species (according to the ordinary hypothesis of "distinct creations"), and considering the rest as derivations from it. Too frequently the term is used merely to characterize that individual of a species which was first cultivated, described, figured, or collected, or that form which happens to be most abundant in the neighbourhood of the writer; and it may of course happen that all the individuals thus referred to may present anomalous or exceptional states of the true type. The only clue we possess to the detection of this, is that which we can derive, in the case of any species of world-wide or "mundane" distribution, from a careful comparison of all its

* New Zealand Flora, pp. xiii. xiv.

variations and from a contrast between these and those of its allies; a sort of medium may thus be eliminated, which may be assumed as an approximation to the original type; but with how little certainty this can be inferred any one may judge for himself, if he endeavours on the same principle to determine what was the original type of the Human species,—whether Adam and Eve were Arabs or Hindoos, Negroes or Mongols, Caucasians or Hottentots, Red Indians or Malays. In regard to plants of limited distribution, especially those restricted to particular islands, the case is different. These are regarded by Dr. Hooker as the remnants of a much larger and more widely diffused Flora, of which a considerable part has been extinguished by subsequent geological changes; and it is of course not only possible but probable that those changes so far modified the condition of the survivors, that their present forms differ considerably from those under which they originally existed. "Practically, then," he says, "the type is a phantom;" that is, in assuming any existing form of a species as the representative of the original, we go far beyond what the facts of the case justify; and our "idea" of a species must necessarily be incomplete, until we have before us *all* the varieties it has presented not only in space but in time.

The still more extended experience acquired by Dr. Hooker since the publication of his 'New Zealand Flora' has only served to confirm and extend the views which he there announced in regard to the variability of the forms assumed by Botanists as specifically distinct.

"The limits of the majority of species," he says ('Flora of Australia,' p. iii.), "are so undefinable that few naturalists are agreed upon them; to a great extent they are matters of opinion, even amongst those persons who believe that species are original and immutable creations; and as our knowledge of the forms and allies of each increases, so do these differences of opinion; the progress of systematic science being, in short, obviously unfavourable to the view that most species are limitable by descriptions or characters, unless large allowances are made for variation."

In the course of a twenty years' study of plants, Dr. Hooker has been engaged in classifying many Floras,—large and small,—insular and continental,—arctic, temperate, and tropical; embracing areas so extensive and varied as fully to justify the assumption that the results derived from these are applicable to the whole vegetable kingdom. In every Flora he finds that whilst some species appear so distinct from one another that most botanists agree as to their limits, their peculiarities being transmitted with little or no change from parent to offspring, and no graduated transition being effected by intermediate forms, there are others which so run into one another as to leave the most practised botanist in a state of perplexity as to their limits, and to prevent him from referring the offspring with any certainty to their parents; so that the entire of such a group has to be regarded as a continuous series of varieties, between the terms of which no hiatus exists suggesting the intercalation of any intermediate variety. The genera *Rubus, Rosa, Salix,* and *Saxifraga* afford conspicuous examples of these unstable species; whilst *Veronica, Campanula,* and *Lobelia* are chiefly composed of comparatively stable forms.

Of these natural groups of varying and unvarying species, some are large and some small; they are also very variously distributed through the classes, orders, and genera of the Vegetable Kingdom; but, as a general rule, the varying species are relatively most numerous in those classes, orders, and genera which are the simplest in structure; increase in complexity of structure being, in Dr. Hooker's opinion, generally accompanied with an increased tendency to permanence in form. Of this principle we could draw some remarkable illustrations from the class of Fungi. Dr. Hooker now agrees with Mr. Darwin (though long disposed to doubt his statement) that the species of large genera are relatively more variable than those of small ones; as if the multiplication of species in the former had been the result of progressive divergence from one primitive type, which possessed an inherent capacity for variation not shared by the latter. And, on the whole, he thinks that herbs are more variable than shrubby plants, and annuals than perennials. The prominent fact, however, is "that this element of mutability pervades the whole Vegetable Kingdom; no class, nor order, nor genus of more than a few species claims absolute exemption; whilst the grand total of unstable forms generally assumed to be species probably exceeds that of the stable.'

The same general doctrines are found by Dr. Hooker to be applicable to all the higher divisions of plants. Some genera and orders are as natural and as limitable by characters, as are some species; others, again, although they contain many very well marked subordinate plans of construction, yet are so connected by intermediate forms with other genera or orders, that it is impossible to limit them naturally. Of the former set, *Orchideæ* and *Gramineæ* are characteristic examples; all the plants belonging to either of these orders being readily referred to each respectively by any competent botanist, notwithstanding that they differ greatly among themselves, not only in habit but in organs of vegetation and reproduction. Of the latter we have examples in *Melanthaceæ* and *Scrophularineæ;* for, although their genera and to a great extent their species also are well-marked and limitable, yet they both contain many groups which are constructed upon very different plans, and are connected with other orders by links of affinity so gradational as to prevent their distinct limitation.

The very important proposition was enunciated by Dr. Hooker in his Introduction to the 'Flora of New Zealand,' that *we are indebted for our means of resolving plants into limitable genera and orders, to the extinction of the forms by which they were originally connected.* This view, which he believes to be now generally accepted even by those who still regard species as the immutable units of the Vegetable Creation, appears to have been suggested to him by the consideration of the effects that must have been produced upon the Flora of the Southern Hemisphere by the submergence of a large proportion of that great Antarctic Continent which seems to have formerly connected New Zealand, South America, Australia, and Van Diemen's Land, as evidenced by a comparison of their respective Floras, and (as Mr.

Wallace has lately shown) by the study of the distribution of their animal inhabitants also.

"No botanist," remarks Dr. Hooker, "can reflect upon the destruction of peculiar species on small islands (such is now going on in St. Helena amongst others), without feeling that, as each disappears, a gap remains which may never be botanically refilled; that not only are those links breaking by which he connects the present flora with the past, but also those by which he binds the different members of the vegetable kingdom one to another."

The zoologist who thoughtfully compares the existing with the extinct forms, in any division of the animal kingdom in which the fossil remains of the latter can be considered as pretty fairly representing the principal types of those which have successively existed in geological time, must see many striking illustrations of this doctrine. Thus if we turn to the existing herbivorous quadrupeds, we find one large series constituting the very natural group of Ruminantia, which seems not only to be complete in itself, but to be very distinctly circumscribed, showing little disposition to pass by gradational links into other groups; whilst in another series the non-ruminant quadrupeds are brought together under the designation of Pachydermata, not so much on account of their agreement in any common characters, as because there seems no other way of disposing of them. The families of which the Elephant, the Tapir, the Rhinoceros, the Hippopotamus, the Pig, the Horse, and the Dugong are respectively the types, differ almost as widely from each other, as regards both their dentition and their extremities, as they do from Ruminants. And yet, when the extinct forms are properly intercalated, these are found to constitute gradational links of affinity of the most remarkable kind, not only among the several families of Pachyderms, but between them and the Ruminants; so that, as Prof. Owen has shown, the whole constitute a series as natural and continuous as that of the Ruminants seem to be now. It is obvious, therefore, that the limitation of the existing Orders of Pachyderms and Ruminants, and the mutual isolation of the families of the former, is due to the extinction of a large proportion of the members of which the entire group was originally constituted.

The inquiry naturally arises, then, whether the *limitation of species*, where it really exists, is not due to a process of a like kind,—namely, *the extinction of intermediate varieties;* and having shown that such a view is sanctioned by a philosophical study of the facts presented to the view of the systematist, Dr. Hooker proceeds to inquire how far it is consistent with the results of physiological research. Our information on this subject is chiefly derived from the behaviour of plants under cultivation; which process affects them, either by suddenly subjecting them to changes in their external conditions which might otherwise have occurred naturally, or by placing them in conditions to which they would never have been exposed in the ordinary course of nature. In the former case, such variations of habit and of conformation are likely to be rapidly induced, as seem to have more gradually developed themselves among wild species growing under varied circumstances; but in the latter the results are widely different, for the plant is eventually either killed, or it undergoes changes in its character that might otherwise have never taken place,—those strongly-marked varieties, without intervening gradations, being produced, which are known to the gardener as "sports." Now the prominent phenomena presented by species under cultivation are analogous in kind and extent to those which have been observed in a state of nature;—a large number apparently remaining permanent and unalterable, while a large number vary indefinitely. With regard to those which are apparently permanent, it is curious to observe that they are not always those which are permanent in a state of nature; and further, that we have no right to conclude that, because they preserve their characters unchanged for a lengthened period, they are necessarily immutable. For it is a remarkable fact that species which have remained apparently unaffected by cultivation for many generations, frequently at length begin to vary; and that, when they have once begun, they are peculiarly prone to vary further.

Now this fact appears to us to have a very marked significance. It can scarcely be questioned that the agencies which at last produce the change in these cases, have really been operative through the whole antecedent period, although their influence has not been ostensibly manifested. And it would seem as if there were some power inherent in the "constitution" of such plants, which causes them to resist such modifying influences and to continue to repeat their ordinary type, until it is borne down by their cumulative action, after which it succumbs altogether. Every medical practitioner must have noticed analogous differences among the subjects of his daily observation. There are some individuals who are unpleasantly affected by every change of temperature or of humidity, by the least indiscretion in diet, by the slightest over-fatigue of body or mind, and yet who are seldom the victims of any serious malady: whilst there are others who seem callous to the most bitter north-easters, and rather enjoy the hot close atmosphere of our most oppressive summer-days, who appear to eat and drink with impunity what would derange any ordinary stomach for a month, and who scarcely know what fatigue means, and who yet, after the resistance of half a life to morbific agencies which seem to roll off them like shot from a bomb-proof, suddenly succumb at last to some fearful attack of disease, from which they never entirely rally, their "constitution" being said to be "broken." This hidden accumulation of the agencies continually operating on organized beings, for a long succession of years in any one individual, or for a long succession of generations in any one species, and their then sudden manifestation in some very marked alteration in the condition of the organism, is a fact about which we think there can be no reasonable doubt. Further, all physiological probability tends to indicate, that the more prolonged the influence, the more decided will be the change in the constitution of the race; just as we see in pathology that any chronic disease is more obstinate and difficult to deal with, in proportion to the length of time during which the individual has been subjected to the causes which have induced it; the most obstinate of all cases being those in which the tendency to the disease is hereditary, that is, in which the causes have had a persistent operation in a previous generation or succession of generations.

The tendency of all variation of plants under cultivation, as well as of those growing in their natural habitats, is to produce progressively-increasing departures from the original type; "the best marked varieties of a wild species," Dr. Hooker remarks, "occurring on the confines of the area the species inhabits, and the best marked varieties of the cultivated species being those last produced by the gardener." He is not disposed to concur in the common statement "that there is a strong tendency in cultivated, and indeed in all varieties, to revert to the type from which they departed." On the contrary, he says—

"The majority of cultivated vegetables and cereals, such as the cabbage and its numerous progeny, and the varieties of wall-fruit, show when neglected no disposition to assume the characters of the wild states of these plants; they certainly degenerate, and even die if Nature does not supply the conditions which man (by anticipation of her operations, or otherwise) has provided; they become stunted, hard, and woody, and resemble their wild progenitors in so far as all stunted plants resemble wild plants of similar habit; but this is not a reversion to the original type, for most of these cultivated races are not *merely* luxuriant forms of the wild parent. In neglected fields and gardens we see plants of Scotch Kale, Brussels Sprouts, or Kohl-rabë, to be all as unlike their common parent the wild *Brassica oleracea*, as they are unlike one another; so, too, most of our finer kinds of apples, if grown from seed, degenerate and become Crabs, but in so doing they become Crab states of the varieties to which they belong, and do not revert to the original wild Crab-apple. And the same is true to a great extent of cultivated Roses, of many varieties of trees, of the Raspberry, Strawberry, and indeed of most garden plants. It has been held, that by imitating the conditions under which the wild state of a cultivated variety grows, we may induce that variety to revert to its original state; but, except in the false sense of reversion above explained, I doubt if this is supported by evidence. Cabbages grown by the sea-side are not more like wild Cabbages than those grown elsewhere, and if cultivated states disseminate themselves along the coast, they there retain their cultivated form."*

The general fact, then, with regard to the varieties induced by cultivation, is that although, when allowed to run wild, they may so far revert to the type of the original that peculiarities of the latter which had been seemingly obliterated make their appearance again, yet that in doing so they so far retain the special characters they had acquired, as not to lose their claim to be considered as varieties. Hence it is that Botanists are still in doubt as to the parent species of many of our cultivated fruits and cereals; which would not be so if they showed a continued disposition to revert to the wild form. Thus the argument for the permanence of species, that is based upon the asserted tendency of cultivated forms to revert when neglected to the wild type, falls to the ground. As Dr. Hooker justly remarks, what the cultivator does is not necessarily (as some have maintained) to induce a diseased or unnatural condition of constitution, but simply to place the plant under conditions which Nature does not provide *at the same particular place and time.*

* Australian Flora, p. ix.

"That Nature might supply the conditions at other places and times, may be inferred from the fact that the plant is found to be provided with the means of availing itself of them when provided, while at the same time it retains all its functions, not only unimpaired, but in many cases in a more highly developed state."

It is no answer to such a statement to point to the admitted fact that our cultivated plants are, for the most part, incapable of self-perpetuation; for the tendency of cultivation, in many instances, is to promote the nutrition of the individual at the expense of its reproduction by seed, a state which is so far from being abnormal, that it is the ordinary condition of many wild plants, which scarcely ever multiply in any other way; whilst in other instances in which there is no deficiency in the production of seeds, the obstruction to their fertility results, not from anything unsuitable in the condition of the plant, but from the interference of agencies external to it, such as climate, the voracity of birds, &c., against which the plant cannot maintain itself unless it ripens more seeds than those agencies destroy. Cultivated wheat, for example, will grow and ripen its seed in almost all soils and climates; and as its seeds are produced in great abundance, and can be preserved alive in any quantity, in the same climate, and for many years, it follows that the extinction it would speedily undergo, if not protected by human agency, is due not to the artificial or peculiar condition of the plant itself, which is as sound and unbroken in health and vigour during its life as any wild variety is, but to the number of the enemies that attack its offspring.

For reasons which are fully stated in Mr. Darwin's chapter on Hybridism, there is ground to believe that the mixture of varieties by cross-impregnation tends rather to maintain the continuity of the specific type, than to induce departure from it; its tendency being to contract rather than to enlarge the limits of variation. That some supposed species may have had their origin in hybridization cannot be denied; but the number of these is probably small, the general fact being, that parents differing from each other in characters of sufficient importance to be accounted truly distinct species, do not produce a progeny that is capable *per se* of continuing its race.

Dr. Hooker having thus been led by his extended study of the Vegetable Kingdom to the independent conviction that every individual possesses within itself a greater or less capacity for variation, its power to change ceasing only with its life, is brought face to face with the grand difficulty of this doctrine; which lies in the admitted fact that there are limits to these mutations both as to degree and kind, species being neither visionary nor even arbitrary creations of the naturalist, but being realities, whether only temporarily so or not. Of this difficulty he unreservedly accepts the doctrine of the delimitation of species by *natural selection* (advanced by Mr. Darwin and Mr. Wallace) as a probable solution; and he does not refuse to go along with it even to the full extent of Mr. Darwin's ultimate speculation. After considering the general phenomena of the geographical and palæontological distribution of Plants, he considers himself justified by ascertained facts in the following assumptions:—

"That the principal recognised families of plants which inhabited the globe at and since the Palæozoic period still exist, and therefore have, as families, survived all intervening geological changes. That of these types some have been transferred, or have migrated, from one hemisphere to another. That it is not unreasonable to suppose that further evidence may be forthcoming, which will show that all existing species may have descended genealogically from fewer pre-existing ones; that we owe their different forms to the variation of individuals, and the power of limiting them into genera and species to the destruction of some of these varieties, &c., and the increase of individuals in others. Lastly, that the fact of species being with so much uniformity the ultimate and most definable group (the leaves as it were of the family tree), may possibly be owing to the tendency to vary being checked, partly by the ample opportunities each brood of a variety possess of being fertilized by the pollen of its nearest counterpart, partly by the temporary stability of its surrounding physical conditions, and partly by the superabundance of seeds shed by each individual, those only vegetating which are well suited to existing conditions; an appearance of stability is also, in the case of many perennials, due to the fact that the individuals normally attain a great age, and thus survive many generations of other species, of which generations some present characters foreign to their parents."*

In estimating the relative amount and rate at which different plants vary, Dr. Hooker points out that much error is often committed. Thus it is assumed that annuals are more variable than perennials; but this is chiefly because a brief personal experience enables several generations of annuals to be studied under many varied combinations of physical conditions, whereas the same experience embraces for the most part but a fractional period of the existence of a perennial. It has also been well shown by Mr. Bentham that an appearance of stability is given to many varieties of perennials, through their habitual increase by buds, offsets, &c., which propagate the individual, not the race; of this we have examples in the case of *Rubi* (brambles), which comparatively seldom propagate by seed. A large tract of ground may be peopled by parts of a single individual, whose original peculiarities may thus be widely diffused and firmly established in a particular locality; just as the vast masses of the now-well-known *Anacharis alsinastrum*, which inconveniently obstruct our rivers, canals, and water-courses, and have all been produced by the extension of a single plant imported some years since from Canada, bid fair to perpetuate in this country any departure from the ordinary specific type which the imported plant might chance to present. In like manner it is obvious that among the almost infinitely varied forms of *Diatomaceæ*, which are now such favourite objects of study among British microscopists, and of which some are disposed to make almost as great a multitude of distinct species, many of the differences that present themselves among the individuals collected from separate lakes, pools, or ditches, are due to the fact that any variety which may happen to arise among the offspring of a particular parentage, will tend to be multiplied and perpetuated indefinitely by that duplicative subdivision which among these single-celled organisms represents the budding of higher plants; instead of merging again into the average type, which

* Australian Flora, pp. xxii. xxiii.

it would probably soon do if it were to unite itself with other individuals in generation, instead of keeping up its isolation by self-division. Hence it has come to be admitted among those who have most carefully and extensively studied this group, that neither size, outline, nor distance of striæ affords a sufficient basis for the distinction of species, until it has been ascertained by an extensive comparison of forms brought from different localities in the widest area over which the species can be traced, what are the average characters of the type, and what is their range of variation.

The most important fact in favour of the permanence of species—the only one, in fact, which affords a tangible argument—is that of genetic resemblance. To the tyro in Natural History all similar plants or animals may have had one parent; but all dissimilar plants or animals must have had dissimilar parents. Daily experience demonstrates the first assumption, and seems to justify the second; but it requires years of careful observation to prove that the second is not always true. And, as Dr. Hooker has well pointed out, it is only by such Naturalists as specially devote themselves to this inquiry, that the truth is likely to be eliminated: for the chief aim of the ordinary systematist is either to arrive at an accurate knowledge of the relations of genera and orders, in which case he takes the species (as it were) for granted; or to acquire an intimate knowledge of the particular groups of species presented by local Floras, in studying which he is liable to be misled by the hereditary transmission of the minutest differences within limited areas, and by his want of acquaintance with the intermediate varieties that may present themselves elsewhere. It is because Dr. Hooker has so carefully kept in view the fundamental question of species, whilst engaged in his higher studies, and has shown such a comprehensive mastery of the general phenomena of vegetation when dealing with local Floras, that we attach so high an importance to his *pronunciamento* on this question. His concluding remarks are so excellent that we cannot forbear quoting them in full:

"It has been urged against the theory that existing species have arisen through the variation of pre-existing ones and the destruction of intermediate varieties, that it is a hasty inference from a few facts in the life of a few variable plants, and is therefore unworthy of confidence, if not consideration; but it appears to me that the opposite theory, which demands an independent creative act for each species, is an equally hasty inference from a few negative facts in the life of certain species, of which some generations have proved invariable within our extremely limited experience. These theories, however, must not be judged of solely by the force of the very few absolute facts on which they are based; there are other considerations to be taken into account, and especially the conclusions to which they lead, and their bearing upon collateral biological phenomena, under which points of view the theory of independent creations appears to me to be greatly at a disadvantage; for according to it every fact and every phenomenon regarding the origin and continuance of species, but that of their occasional variation and their extinction by natural causes, and regarding the *rationale* of classification, is swallowed up in the gigantic conceptions of a power intermittently exercised in the development, out of inorganic elements, of organisms the most bulky and complex as well as the most minute and simple; and the consanguinity of each new being to its pre-existent nearest ally, is a barren fact, of no scientific significance or further importance to the naturalist than that it enables him to classify. The realization of this conception is of course impossible; the boldest speculator cannot realize the idea of a highly organised plant or animal starting into life within an area that has been the field of his own exact observation and research; whilst the more cautious advocate hesitates about admitting the origin of the simplest organism under such circumstances, because it compels his subscribing to the doctrine of the 'spontaneous generation' of living beings of every degree of complexity in structure and refinement of organisation.

"On the other hand, the advocate of creation by variation may have to stretch his imagination to account for such gaps in a homogeneous system as will resolve its members into genera, classes, and orders; but in doing so he is only expounding the principle which both theorists allow to have operated in the resolution of some groups of individuals into varieties: and if, as I have endeavoured to show, all those attributes of organic life which are involved in the study of classification, representation, and distribution, and which are barren facts under the theory of special creations, may receive a rational explanation under another theory, it is to this latter that the naturalist should look for the means of penetrating the mystery which envelopes the history of species, holding himself ready to lay it down when it shall prove as useless for the further advance of science as the long serviceable theory of special creations, founded on genetic resemblance now appears to be."*

Such testimony as this to the value of Mr. Darwin's theory ought to procure for it a fair hearing from the zoologist; and there are not wanting indications that the several divisions of the Animal Kingdom, when studied with the like care, will furnish results of the like character. The question of *what constitutes a species* becomes in fact more and more difficult, in proportion as it is seen that no general rule can be laid down, which shall be alike applicable in all groups. If we adopt the principle formularized by Decandolle,—probably as philosophical a statement of the case as has yet been put forward,—that "we unite under the designation of a species all those individuals which mutually bear to one another so close a resemblance as to allow of our supposing that they *may have* proceeded originally from a single being or a single pair," it is obvious that it opens a further question which nothing but experience can decide, what is the degree of difference which may present itself among the descendants of a common ancestry, or, in other words, what is the *range of variation* of which each specific type is susceptible. It would appear in some cases as if the resemblance of the offspring to each other and to their common parent is so close and uniform, that a difference of a most trivial kind, such as a stripe of colour on the hide of a mammal, or the shape or hue of a tuft of feathers in a bird, or a certain spot on the wing of a butterfly, or a peculiar sculpture of the surface of a shell, is held to justify the assumption of a distinct original parentage for the group of individuals which presents it; notwithstanding that, in many other cases, such slight and superficial differences are justly regarded as of no account whatever, seeing that they occur among individuals which are known to have had a common descent. It is not unfrequently found, indeed, that differences of sufficient constancy and importance to serve

* Australian Flora, pp. xxv. xxvi.

for the separation not merely of species, but even of genera, in one group, are so inconstant and gradational in another, that they cannot be admitted to any other rank than that of individual diversities. Thus the systematist is constantly finding himself at a loss to determine what are the characters of sufficient constancy to serve for the definition of his species, and what extent of range he must assign in each case to individual variation; and this difficulty is generally found to increase, rather than to diminish, with the extent of his researches. For it is generally true that in Zoology, as in Botany, the multiplication of reputed species has been the work of men of very imperfect information, who (to use the appropriate expression of the late Prince of Canino) have described *specimens* rather than *species;* and that a large number of these can be reduced to the rank of varieties merely by a sufficiently extended comparison with each other. And this holds good no less in regard to fossil types than with respect to recent; the unequivocal tendency of recent Palæontological inquiry having been to show that there is a much greater identity in type among the species which occur through a succession of stratified deposits, than those have been willing to admit who have based their conclusions on the trivial differences presented by isolated specimens; and also to indicate that a general modification of any specific type has been consequent on a modification of the physical conditions under which it has existed.

To the Physiologist and Pathologist, the question of hereditary transmission is one of which it is almost impossible to exaggerate the importance; for, as Sir H. Holland has well remarked in one of his suggestive essays, "in considering the hereditary tendency to disease, whether arising from structural or less obvious causes, it is needful to regard it in connexion with, or even as part and effect of, that great general principle, through which varieties of species have been spread over the globe, with obvious marks of wise and beneficent design." The inquiry is one far too wide to be even entered upon in the limited space that now remains to us; but we shall take an early opportunity of bringing it under the attention of our readers, confining ourselves for the present to a notice of some of the chief points of interest discussed by Mr. Darwin in the chapters of his book to which we have not yet alluded.

In his Sixth Chapter he grapples with some of the difficulties which will naturally present themselves to the minds of those who shall attempt to follow him in the application of the doctrine of Natural Selection to anything like the extent to which he is himself disposed to carry it. Thus it may reasonably be inquired why, if species have descended from other species by insensibly fine gradations, do we not see everywhere innumerable transitional forms? Why is not all nature in confusion, instead of presenting, as we see, an assemblage of species for the most part well defined? Of his answers to these questions, marked by his usual penetration and logical consistency, we are enabled to present a summary in his own words:

"I believe that species come to be tolerably well-defined objects, and do not at any one period present an inextricable chaos of varying and intermediate links:—

"Firstly, because new varieties are very slowly formed, for variation is a very slow process, and natural selection can do nothing until favourable variations chance to occur, and until a place in the natural polity of the country can be better filled by some modification of some one or more of its inhabitants; and such new places will depend on slow changes of climate, or on the occasional immigration of new inhabitants, and probably in a still more important degree on some of the old inhabitants becoming slowly modified with the new forms thus produced, and the old ones acting and reacting on each other. So that, in any one region and at any one time, we ought only to see a few species presenting slight modifications of structure in some degree permanent; and this assuredly we do see.

"Secondly, areas now continuous must often have existed within the recent period in isolated portions, in which many forms, more especially amongst the classes which unite for each birth and wander much, may have separately been rendered sufficiently distinct to rank as representative species. In this case, intermediate varieties between the several representative species and their common parent, must formerly have existed in each broken portion of the land; but these links will have been supplanted and exterminated during the process of natural selection, so that they will no longer exist in a living state.

"Thirdly, when two or more varieties have been formed in different portions of a strictly continuous area, intermediate varieties will, it is probable, at first have been formed in the intermediate zones, but they will generally have had a short duration. For these intermediate varieties will, from reasons already assigned (namely, from what we know of the actual distribution of closely allied or representative species, and likewise of acknowledged varieties), exist in the intermediate zones in lesser numbers than the varieties which they tend to connect. From this cause alone the intermediate varieties will be liable to accidental extermination; and during the process of further modification through natural selection, they will almost certainly be beaten and supplanted by the forms which they connect; for these, from existing in greater numbers, will, in the aggregate, present more variation, and thus be further improved through natural selection, and gain further advantages.

"Lastly, looking not to one time, but to all time, if my theory be true, numberless intermediate varieties, linking most closely all the species of the same group together, must assuredly have existed; but the very process of natural selection constantly tends, as has been so often remarked, to exterminate the parent forms and the intermediate links. Consequently, evidence of their former existence could be found only amongst fossil remains, which are preserved, as we shall in a future chapter attempt to show, in an extremely imperfect and intermittent record." (pp. 177–179.)

We are somewhat surprised that Mr. Darwin has not had recourse for illustration to the phenomena presented by the Races of Man, which, until fused by intermixture, present such remarkable constancy and speciality of characters over their respective areas, as to have led many Ethnologists to regard them as distinct species, yet which those who have studied them most candidly, intelligently, and profoundly, are now disposed to refer to a common origin. The doctrine of Natural Selection could scarcely, we think, be more satisfactorily tested, than by applying it to the process by which the Earth has become peopled with races so diversified, yet each presenting features of such marked adaptation to the peculiar conditions of its existence, as to necessitate, in the opinion of some, the idea of its special creation with reference to these.

Another objection which naturally occurs to the inquirer is that arising out of the peculiar modifications of structure presented by some animals (as, for example, the bat or the whale), and the wonderful elaboration of particular organs, such as the eye, of which we hardly as yet fully understand the inimitable perfection. Mr. Darwin freely admits the cogency of this objection; but he urges not unfairly that if large bodies of facts, otherwise inexplicable, can be explained on the theory of descent, we ought not to hesitate in going further, and to admit—what cannot be affirmed to be logically impossible—that any conceivable degree of perfection may be acquired through Natural Selection. "Reason," he says, "ought to conquer Imagination;" and the Astronomer who affirms that his reason can gauge such profundity of space, and the Geologist who trusts in its ability to penetrate such unfathomable depths of time, as defy the power of the Imagination, ought to be the last to say that because they cannot conceive how an eye could thus be brought into existence, therefore Mr. Darwin's theory is false.

Some, again, will find in the special Instincts of animals,—such as those which lead the bee to construct cells which have practically anticipated the discoveries of profound mathematicians,—a grave objection to the doctrine of Natural Selection. This subject is specially discussed by Mr. Darwin in his Seventh Chapter, which contains some very curious and novel information on the subject of the architecture of bees, together with an examination of the difficulties presented on his theory by the existence of neuter insects, the parasitic instincts of birds and insects, the slave-making instincts of ants, &c. His mode of disposing of these difficulties is very ingenious; but to us it appears unnecessarily elaborate. For what are called instincts are simply in our apprehension the expressions of the habitual modes of operation of the particular organization; and any modification or further development of the organization will necessarily involve a corresponding modification of its habitual mode of action. We see this unmistakeably in Plants, the direction of whose tendrils or rootlets is often so marvellously adapted to the purposes which these have respectively to serve, as to have forced on the minds of many observers the notion that it must be guided by some degree of consciousness. We may see it clearly, too, in watching the development of Animals and even of Man; the special instincts of whose different periods of life are obviously related to the functional activity of particular organs, the alimentative dominating for the most part in the earlier stages, and the sexual when the generative system has come with adult age into energetic operation. Hence, if the principle of Natural Selection can account for the production of the organized structure, we see no difficulty in extending it to any of the actions to which it is subservient; all these being but the expressions (so to speak) of the capacity of that structure.

It will be more pertinently asked, however, in what manner, on Mr. Darwin's theory of the Origin of Species, we can account for *species*, when crossed, being sterile and producing sterile offspring, whereas, when *varieties* are crossed, their fertility is unimpaired? This subject is elaborately treated in Mr. Darwin's Eighth Chapter, which is obviously the result of a very careful and conscientious examination of the facts in regard to hybridism, that have been accumulated by the labours of those two admirable observers Kolreuter and Gärtner, as well as of many others collected by himself; his general conclusion being that no such precise law can be admitted, as that according to which it has been proposed to distinguish species from varieties on the ground of their sterility or fertility when brought into sexual connexion. For although it is doubtless a general rule that the hybrid offspring of parents whose specific diversity (according to the received views) is unquestionable, are *per se* incapable of engendering a mixed race, yet their sterility is by no means uniformly absolute, being in fact a question of degree. So, again, although it is doubtless a general fact that the crossing of forms only slightly different tends to increase the vigour and fertility of the offspring, yet it also appears that wider differences, though still within the admitted range of variation, tend to diminish and even to check fertility. It is well known that in grafting trees, the capacity of one species or variety to unite with another bears a general relation to their mutual conformity in constitution, though sometimes modified by unknown differences in their vegetative systems; and looking to the fact that the reproductive system is more readily affected than any other by causes which influence the vital conditions of any race, it does not seem strange that the greater or less readiness of one species to breed with another should be rather dependent on unknown differences in their reproductive systems, than on any special limitation of their capacity with a view to prevent their crossing and blending in nature. The sterility of hybrids, which have their reproductive systems imperfect, and which have had this system and their whole organization disturbed by being compounded of two distinct species, seems closely allied to that sterility which so frequently affects pure species when their natural conditions of life have been disturbed. There has doubtless been much vicious reasoning in regard to this matter; plants and animals which breed freely together having been assumed on that account to be identical in species; and those which are not thus fertile being assumed to be diverse. And it is a somewhat significant fact, that the two very careful experimentalists already alluded to have arrived in several instances at diametrically opposite conclusions, when they have come to apply this test in practice. We are strongly disposed, therefore, to agree with Mr. Darwin, that the phenomena of Hybridism, when carefully examined, do not support the received view that there is a fundamental distinction between species and varieties; but are at least as well explained upon the idea that sterility is a question of degree,—that degree depending upon the amount of divergence which separates the constitutional conditions of the two forms brought together.

It may be urged, however, as an objection to Mr. Darwin's views, that varieties produced by artificial selection, though differing so strongly from each other that they would be unhesitatingly ranked by any one ignorant of their origin as belonging to dissimilar species or even (as in the case of Pigeons) to dissimilar genera, are capable of breeding freely with each other; no instance being known of varieties thus engendered ever coming to be kept from mixture by reason of their sterility when crossed. The explanation of this fact, however, does not appear to us at all difficult on Mr. Darwin's view. Artificial selection is generally practised rather with a view of intensifying and perpetuating some particular feature, which may have but very little relation to the general constitutional character. Moreover, in no instance in which any great diversity of conformation has been engendered, has this been maintained (so far as we know) for any long succession of generations; consequently it has not yet been fixed (so to speak) as the attribute of the race, which commonly shows a continual tendency to reversion to the ordinary type, and is only kept up in its speciality by elimination of all the individuals that do not fully come up to the desired type. The ordinary influence of external causes, on the other hand, tends sooner (as we see in medical practice) to modify the constitutional state, than to affect the bodily conformation; and it is easy to understand from the evidence we daily have of their operation, that their prolonged agency might very considerably affect the capacity of one race to propagate with another, even though that action had not manifested itself in any very striking diversity of external characters.

In his Ninth and Tenth Chapters, Mr. Darwin applies himself to the consideration of the objections which may be raised against his views, on the ground of the entire absence of Palæontological evidence as to the past existence of any such finely graduated organic chain,—consisting of a series of intermediate links between the first progenitors of each principal type, and the diversified forms under which it has subsequently presented itself,—as his theory requires. The force of this, which he admits to be the most obvious and the gravest objection which can be urged against it, he thinks is entirely removed by a fair appreciation of *the extreme imperfection of the geological record.* To all such as have been prepared for the reception of his arguments by the mastery of Sir Charles Lyell's admirable reasonings upon the subject, in that grand work on the 'Principles of Geology,' which we fully agree with Mr. Darwin in believing that "the future historian will recognise as having produced a revolution in natural science," we feel sure that Mr. Darwin's deductions must approve themselves as necessary corollaries from Sir C. Lyell's demonstrations. To those, on the other hand, who have been accustomed to look at the revelations of Geology as giving us a succession of tolerably complete pictures of the former condition of the Earth, instead of an assemblage of isolated sketches or studies which show us rather how little than how much we know, or are ever likely to know, about its past history and its living inhabitants, Mr. Darwin's protest against the drawing of any inferences unfavourable to his theory from the paucity of the remains of the almost infinite number of generations which must have succeeded one another in the long roll of years, will seem more like the special pleading of an advocate than (what we are firmly convinced that it is) the candid representation of profound scientific truth. We are fully satisfied that he does not in the least exaggerate the imperfection of the geological record; and think it the less necessary to add anything in confirmation of his views, because we have already attempted to show, in our introductory remarks, how many indications are afforded by modern geological inquiry that the production of new species has been effected by constant and progressive rather than by intermittent action. Many of the great leading facts in Palæontology certainly harmonize most remarkably with the doctrine of continuous descent with modification through natural selection.

"We can thus understand how it is that new species come in slowly and successively; how species of different classes do not necessarily change together, or at the same rate, or in the same degree; yet in the long run that all undergo modification to some extent. The extinction of old forms is the almost invariable consequence of the production of new forms. We can understand why when a species has once disappeared it never reappears. Groups of species increase in numbers slowly, and endure for unequal periods of time; for the process of modification is necessarily slow, and depends upon many complex contingencies. The dominant species of the larger dominant groups tend to leave many modified descendants, and thus new sub-groups and groups are formed. As these are formed, the species of the less vigorous groups, from their inferiority inherited from a common progenitor, tend to become extinct together, and to leave no modified offspring on the face of the Earth. But the utter extinction of the whole group of species may often be a very slow process, from the survival of a few descendants, lingering in protected and isolated situations. When a group has once wholly disappeared, it does not reappear; for the link of generations has been broken.

"We can understand how the spreading of the dominant forms of life, which are those that oftenest vary, will in the long run tend to people the world with allied, but modified, descendants; and these will generally succeed in taking the places of those groups of species which are their inferiors in the struggle for existence. Hence, after long intervals of time, the productions of the world will appear to have changed simultaneously.

"We can understand how it is that all the forms of life, ancient and recent, make together one grand system; for all are connected by generation. We can understand from the continued tendency to divergence of character, why the more ancient a form is, the more it generally differs from those now living. Why ancient and extinct forms often tend to fill up gaps between existing forms, sometimes blending two groups previously classed as distinct into one; but more commonly only bringing them a little closer together. The more ancient a form is, the more often, apparently, it displays characters in some degree intermediate between groups now distinct; for the more ancient a form is, the more nearly it will be related to, and consequently resemble, the common progenitor of groups since become widely divergent. Extinct forms are seldom directly intermediate between existing forms; but are intermediate only by a long and circuitous course through many extinct and very different forms. We can clearly see why the organic remains of closely consecutive formations are more closely allied to each other, than are those of remote formations; for the forms are more closely linked together by generation: we can clearly see why the remains of an intermediate formation are intermediate in character." (pp. 343-5.)

The Tenth and Eleventh Chapters are devoted to the subject of Geographical Distribution, which, as we have already endeavoured to show, has lately come to present an aspect altogether new. These seem to us to be among the most interesting and satisfactory in the whole work; the clue which Mr. Darwin's theory affords, taken in connexion with the strong probability that considerable changes of level have occurred since the Earth has been peopled by its existing inhabitants, leading us towards a much better rationale of the remarkable phenomena presented by the present Geographical distribution of Plants and Animals, than any which has been previously offered. The great leading facts seem, as Mr. Darwin points out, to be explicable on the theory of migration (generally of the more dominant forms of life) together with subsequent modification and the multiplication of new forms. We can thus comprehend the high importance of barriers, whether of land or water, which separate our several zoological and botanical provinces. We can thus understand the localization of sub-genera, genera, and families; and how it is that under different latitudes, for instance in South America, the inhabitants of the plains and mountains, of the forests, marshes, and deserts, are in so mysterious a manner linked together by affinity, and are likewise linked to the extinct beings which formerly inhabited the same continent. So again it can be explained why oceanic islands should have few inhabitants, but of these a large proportion should be endemic or peculiar; and why whole groups of organisms, as batrachians and terrestrial mammals, should be absent from such, whilst the most isolated islands possess their own peculiar species of aërial mammals or bats. As the late Professor Edward Forbes used to insist, there is a striking parallelism in the laws of life throughout time and over space; the laws governing the succession of forms in past times being nearly the same with those governing at the present time the differences in different areas. On Mr. Darwin's theory these relations are intelligible; for whether we look to the forms of life which have changed during successive ages within the same quarter of the world, or to those which have changed after having migrated into distant quarters, in both cases the forms within each class have been connected by the same bond of ordinary generation; and the more nearly any two forms are united in blood, the nearer will they generally stand to each other in time and in space; in both cases the laws of variation have been the same, and modifications have been accumulated by the same power of natural selection.

The argument is brought to a close in the Thirteenth Chapter, which treats of the evidence afforded in favour of the author's views by the Mutual Affinities admitted by all Naturalists to exist among Organized beings, by Morphology, Embryology, and the existence of Rudimentary organs. Mr. Darwin urges with great force that the nature of the relationship by which all living and extinct beings are united by complex, radiating, and circuitous lines of affinities into one grand system, —the rules followed and the difficulties encountered by naturalists in their classifications,—the value set on characters, if constant and prevalent, whether of high functional importance, or of the most trifling importance, or (as in the case of rudimentary organs) of no importance,

—the wide opposition in value between analogical or adaptive characters, and characters of true affinity,—and other such rules,—all naturally follow on the view of the common parentage of those forms which are considered by naturalists as allied, together with their modification through natural selection, with its contingencies of extinction and divergence of character. Thus, in his view, all true Classification is genealogical; and community of descent is the hidden bond which naturalists have been unconsciously seeking, rather than some unknown plan of creation, or the enunciation of general propositions, and the mere collocation and separation of objects more or less alike.

On this same view, it is urged by Mr. Darwin, all the great facts in Morphology become intelligible; that resemblance in general plan of organization which is expressed by the term "unity of type," acquiring a meaning and value of which it was before utterly destitute. Nothing, as Prof. Owen has fully admitted, can be more hopeless than to attempt to explain this fundamental similarity of pattern in members of the same class, by utility, or the doctrine of final causes. On the ordinary view of the independent creation of each animal and plant, we can only say that *so it is*, or that it has so pleased the Creator to construct it. A like explanation, he argues, may be given to the great facts of Embryology; such as the very general, but not universal, difference in structure between the embryo and the adult; the early likeness of parts in the same individual embryo, which afterwards become very unlike and serve for diverse purposes; the general, but not universal, resemblance of embryos of different species belonging to the same class; the absence of any close relation in the structure of the embryo to the conditions of its existence, except when it becomes active and has to provide for itself instead of being nurtured by its parent; and the apparent retrogradation which sometimes takes place from the embryonic type as the animal approaches maturity. It was long ago shown by Von Baer that all development proceeds from the general to the special; and, as we have already pointed out, this principle has come to exert a most important influence in classification, the real place of a group being often determined rather by reference to its embryonic than to its adult type. Now, the causes of modification which originate varieties of structure will for the most part act through the constitution of the parents; and the analogy of hereditary diseases appearing late in life would seem to indicate that such causes may not manifest their effect upon the offspring, until it has attained an age approaching that at which they acted on the parent. Hence it can be understood how it is that the differences in the races of domestic animals should be much less marked when they have just come into the world, than they subsequently become. Mr. Darwin found, for example, that the newly-hatched young of different breeds of pigeons, differing so much in their adult state that they would have been ranked among different genera had they been natural productions, so imperfectly presented their peculiar characters as not to be always distinguishable. Cases occur every now and then, however, in which the dèparture from the ordinary type has shown itself at a much earlier period in the life of the offspring; and if this be perpetuated by breeding (as in the case of the "ancon" conformation of the legs, or the silky wool of the Mauchamp breed of sheep), it shows itself at a correspondingly early period in the subsequent descendants. If, then, the embryo be looked upon as a picture, more or less obscured, of the common parent form of each great class of animals, the successive modifications which it undergoes in its further development repeat in their essential features the modifications which the type has undergone in antecedent ages under the operation of natural selection.

On the same view the presence of Rudimentary Organs comes to acquire a meaning of which it is otherwise destitute; and this, to our minds, is one of the strongest arguments in favour of Mr. Darwin's theory. The same reasoning power (as he truly says) which tells us plainly that most parts and organs are exquisitely adapted for certain purposes, tells us with equal plainness that these rudimentary or atrophied organs are imperfect and useless. Such organs are generally said to have been created "for the sake of symmetry," or in order "to complete the scheme of nature;" and Mr. Paget has thrown out the idea that they serve to withdraw matter from the blood, which could not be retained in it without injury. Now, on the principle just now stated, at whatever period of life an organ thrown out of use by changed habits consequently becomes atrophied, and this condition is perpetuated by Natural Selection, the principle of inheritance at corresponding ages will tend to reduce the organ at the like period in the life of the offspring, and consequently will seldom affect or reduce it in the embryo; and hence will arise the greater relative size of rudimentary organs in the embryo, and the subsequent proportional reduction by arrest of their development. It is remarkable how much value systematists have been led to attach to the presence of rudimentary organs as indications of natural affinity; and this becomes readily intelligible on the genealogical view of classification, just as, to use Mr. Darwin's apposite simile, letters which are retained in the spelling of a word, though they have become useless in classification, often afford a clue in the search for its derivation. "Nature may be said to have taken pains to reveal, by rudimentary organs and by homologous structures, her scheme of modification, which it seems that we wilfully will not understand."

Of the Recapitulation and Conclusion contained in the closing Chapter, we need say no more than that it is a masterly summing-up of the author's argument, a worthy finale to what we do not hesitate to designate a wonderful book. From the remarks we have made as we have proceeded in our exposition of Mr. Darwin's views, our readers will perceive that we are strongly convinced of their fundamental truthfulness; and we cannot see any legitimate escape from the logical conclusion, that the process of Natural Selection has had a similar agency in multiplying the number of divergencies of type amongst the races of plants and animals living in a state of Nature, that the process of Artificial Selection has exerted on those subject to the influence of Man. But when we once go beyond the limits of our actual experience, the question as to the extent of this change is one as to which we have no data whatever for any positive conclusion, and are left altogether to the guidance of probabilities. Supposing, for the sake of argument, that we concede to Mr. Darwin that all Birds have descended from one common stock,—and we cannot see that there is any essential improbability in such an idea, so small are the divergencies from a common type presented by any members of that group, —yet it by no means thence follows that Birds and Reptiles, or Birds and Mammals, should have had a common ancestry. The very imperfection of the Geological record, on which he so pointedly dwells, takes away all power of denial that Birds may have been placed on the Earth as early as any form of organic life whatever. And to us it seems far more likely that this has been the case with regard to each of the great types marked out by decided structural and physiological peculiarities, than that these have been derived from any still more remote ancestor by the process of Natural Selection. So, too, there seems to us so much in the psychical capacity of Man, however degraded, to separate him from the nearest of the Mammalian class, that we can far more easily believe him to have originated by a distinct creation, than suppose him to have had a common ancestry with the Chimpanzee, and to have been separated from it by a series of progressive modifications.

We think it very important to the fair reception of Mr. Darwin's primary views, that they should be considered quite apart from the ultimate conclusions to which they tend in his own mind, but which others may see adequate reason for doubting or rejecting. It is among their highest merits that the mere provisional reception of them, as a step (it may be) to something still better, will give a new interest to the philosophical pursuit of Natural History, and will bring into mutual reconcilement the two great ideas of opposing schools,—the morphological notion of Unity of Type,—and the teleological notion of Conditions of Existence.

Review VI.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XLII. *London*, 1859. 8vo. pp. 461.

The present volume of the 'Medico-Chirurgical Transactions' is inferior to none of its predecessors in interest or intrinsic value. It contains twenty-five original communications, about one-half of which are medical and the other half surgical. These communications are illustrated by nine lithographic plates, five of which are coloured, by fourteen diagrams, and three woodcuts. We proceed, as usual, to lay before our readers a brief abstract of the contents of the volume:

I. *A Case of Excision of the Head of the Humerus, with its Results.* By John Birkett, F.R.C.S.—This case is mainly recorded with the object of showing the results of the operation at a distant period of time afterwards. The patient was a man aged fifty-seven, and the disease of the articular cartilage of the head of the humerus had existed for two and a half years, and had apparently originated from a severe injury of the shoulder-joint. The operation was performed on July 24th, 1855, the head of the bone being removed by a fine "bow-saw." Twenty-five days after the operation, the patient was discharged from the hospital. A year afterwards, pus still continued to flow from three sinuses, but no exposed bone could be detected, and the man could use his forearm and hand to a limited extent, while his general health was good. From the end of 1856 down to September, 1858, when the patient was last seen, the man had used his arm freely in the varied occupations of a farm labourer; he was entirely free from pain, and enjoyed perfectly good health. The upper end of the humerus could be felt beneath the integuments near to the coracoid process of the scapula.

Notes are given of a second case, in which a similar operation was performed by the late Mr. Aston Key, on January 30th, 1849, upon a man aged thirty-three. This man made a good recovery, and is now in perfect health; while, for the last six years, he has been actively engaged as a working engineer.

II. *On Disarticulation of the Scapula from the Shoulder-joint, with Removal of the Acromial end of the Clavicle.* By GEORGE MATTHEW JONES, M.R.C.S., Jersey.—This operation appears to have been only twice previously performed, first by Mr. Syme, in 1856 (an account of which operation was published in the 'Medico-Chirurgical Transactions'*), and subsequently by a practitioner in Ayrshire. In both these cases, however, the patients died within three months of the operation, so that no opportunity was afforded of ascertaining how far the motions of the arm could be restored. Not so with Mr. Jones's case. His patient was a girl, aged fourteen and a half, upon whom the operation was performed on the 10th of May, 1858, on account of long-standing necrosis of the left scapula, with numerous fistulous openings through the soft parts. At the end of twenty-four days the patient was able to leave her bed and walk round the garden, and after another week she could sew without pain or difficulty. The discharge continued for about six weeks, but rapidly diminished. Her treatment was essentially stimulant, eight glasses of wine, and sometimes more, being allowed every twenty-four hours, in addition to three pints of porter. In November, 1858, she was able to abduct her arm twelve inches from her side, and could raise the hand to the opposite shoulder or to the mouth with ease. She was able to lift considerable weights, and could scrub the floor and make her bed. Not the slightest pain was experienced upon any motion. There was a decided falling of the shoulder, but this was scarcely perceptible when the patient was dressed. The deltoid was of full size. The author agrees with Mr. Syme in

* Vol. xl. p. 107; British and Foreign Medico-Chirurgical Review, July, 1858, p. 108.

the conclusion that the danger from hæmorrhage is much less in removing the whole of the scapula, than, as in the case recorded by Mr. Liston, in the removal of only a portion.

III. *Case of Encephaloid Cancer affecting a Testicle which had been retained within the cavity of the Abdomen.* By GEORGE JOHNSON, M.D., London.—This case appears to be unique in its locality, while it was also remarkable for the large size attained by the tumour, for the rapidity of its growth, the great pain which it occasioned, and also from the circumstance that the nature of the tumour appears to have been diagnosed during life. Many cases are on record in which a testicle retained in the groin, or in the inguinal canal, has become the seat of cancerous disease, but apparently none in which the organ had not commenced its descent. After the evacuation of about four pints of dirty grumous fluid from numerous cysts contained in the tumour, this still weighed sixteen pounds. The disease appears to have commenced exactly fifteen months previous to death with severe pain in the right lumbar region; but three weeks before death, when the patient was seen in consultation with Dr. Bright, the tumour was scarcely prominent. Another remarkable feature of the case was the extreme pertinacity with which the patient concealed the knowledge of the non-descent of the testicle from most of the medical gentlemen whom he consulted.

IV. *Cases of Re-fracture of Bone, with Observations.* By F. C. SKEY, F.R.S., President of the Society.—The author advocates the re-fracture of bones which have been imperfectly united, or in which there is overlapping to any moderate extent, as an operative proceeding which is practicable, safe, and indispensable to the perfect utility of the limb involved. Various instructions are given for our guidance in the performance of the operation, and seven cases are appended to illustrate its good effects. In these cases the intervals between the original injury and the re-fracture varied up to thirteen months. Six of the seven cases are stated to have been perfectly successful; but in one it was quite impossible to make any impression on the fractured bone, although the full amount of warrantable pressure in every direction was resorted to.

V. *Cases of Dislocation of the Os Calcis and Scaphoid from the Astragalus, with Remarks on the importance of dividing the Gastrocnemius and other Tendons to facilitate reduction in various Dislocations of the latter bone.* By GEORGE POLLOCK, F.R.C.S.—Our space will not allow us to enter into the details of this paper; but it is one which well deserves the attention of the practical surgeon.

VI. *A Description of the Organs of Generation of a Hermaphrodite Sheep.* By W. S. SAVORY, F.R.S.—This case was remarkable for the conjunction of male and female organs of great perfection. The external orifice led into a vagina, beyond which was the uterus, with its two horns as in the female; but in place of the ovaries were two testes, each surmounted in its whole length by an epididymis, and by the termination of the corresponding uterine horn. In shape and aspect these testes were precisely similar to those of an ordinary male, but were very much smaller, and contained no traces of spermatozoa. The animal was, of course, essentially a male, but during life had been regarded as an ewe. Figures are given illustrative of the appearances presented by the different organs.

VII. *On the Causes of Death after Amputation.* By THOMAS BRYANT, F.R.C.S.—Mr. Bryant commences by advocating the utility of statistics in medical science, and points out that they who object to them still trust to what they call their *experience.* He justly observes, however, that experience, to be of any value, must be something definite, arrived at through practical knowledge, and capable of expression, so that, in a measure, it becomes statistical. With a view of gaining some distinct idea of the causes of death after amputation, Mr. Bryant has collected from Guy's Hospital 300 cases of amputation, and has divided these into four classes,—primary, secondary, pathological, and amputations of expediency, the last class being distinguished from the circumstance that the amputations have been performed from expediency more than from necessity, as, for example, in cases of talipes and elephantiasis. The paper contains several statistical tables, with copious analyses. The following are some of the more important general conclusions arrived at as to the causes of death after amputations:

"1. That 25 per cent. of all cases are fatal; 30 per cent. of the lower extremity, 10 per cent. of the upper.

"2. That *pyæmia* is the cause of death in 42 per cent. of the fatal cases, and in 10 per cent. of the whole number amputated.

"3. That exhaustion is the cause of death in 33 per cent. of the fatal cases, and in 8 per cent. of the whole number amputated.

"4. That the following causes of death are fatal in the annexed proportions:

	Of fatal cases.		Of whole number.	
"Secondary hæmorrhage .	7·	per cent.,	or 1·66	per cent.
Thoracic complications .	5·6	,,	,, 1·33	,,
Cerebral ,, . .	3·	,,	,, ·66	,,
Abdominal ,, . .	1·4	,,	,, ·33	,,
Renal ,, . .	3·	,,	,, ·66	,,
Hectic ,, . .	3·	,,	,, ·66	,,
Traumatic ,, . .	7·	,,	,, 1·66	,,

Of the total amputations of the thigh, 27 per cent. were fatal; of pathological amputations, 18 per cent.; amputations of expediency, 37 per cent.; primary amputations, 60 per cent.; secondary amputations, 75 per cent. The comparative frequency of the different modes of fatal termination in each of the four classes of amputation, not only in reference to the thigh, but also in reference to the leg, arm, and forearm, has been ascertained with much labour. Pyæmia was the cause of death in 42 per cent. of all fatal cases of amputation, and in

10 per cent. of all amputations. As regards the different forms of amputation, pyæmia was the cause of death in the following order: in 70 per cent. of all fatal amputations of expediency; in 43 per cent. of all fatal primary amputations; in 43 per cent. of all fatal pathological preparations; and in 25 per cent. of all fatal secondary amputations. Of primary amputations of the thigh, 35 per cent. sank from exhaustion; 15 per cent. died from pyæmia; 5 per cent. from secondary hæmorrhage; and 5 per cent. from traumatic complications. On the other hand, of primary amputations of the leg, one-half of the fatal cases died from pyæmia, and only one-eighth of exhaustion; of amputations of the upper extremity one-third of the fatal cases died from pyæmia. It would be interesting to contrast the results of Guy's with those of other metropolitan and provincial hospitals.

VIII. *On some of the Cyclical Changes in the Human System connected with Season.* By EDWARD SMITH, M.D.—The readers of the 'British and Foreign Medico-Chirurgical Review* cannot but be aware of the laborious researches by which Dr. Edward Smith has endeavoured to show the hourly changes in the rate of the pulse and in respiration in health and in phthisis. He has now extended the same inquiry to the changes of the system in health from day to day, and month to month, through the cycle of the seasons; and he has ascertained that, as the season advances from spring to summer and autumn, all the respiratory phenomena become lessened. The highest state of the functions was in spring; the period of decline was the beginning of summer; the lowest state was towards the end of summer and in the beginning of autumn; and the period of increase was the beginning of winter. In winter and in summer the respiratory phenomena may be said to be comparatively uniform for a considerable period, whereas spring and autumn are truly seasons of change. The following appear to be the variations in the system in the summer:

1. Lessened respiratory and other chemical changes.
2. Increased rate of pulsation, with increase of blood at superficies, and diminution at centres.
3. Increase in the function of the skin, increased transpiration, and increased sensibility to cold.
4. Lessened difficulty in maintaining a sufficient degree of animal heat, but greater difficulty in preventing excess.
5. Lessened appetite, and lessened quantity of food.
6. Unascertained changes in the state of the blood, in reference to the degree of oxidation, the quantity of fibrin, and the degree of alkalinity.
7. A probable increased action of the liver.

The author draws from his investigations the following deductions in reference to disease:

1. That the circumstance of the type of the same disease differing in different years may be explained by the fact, that different years

* April, 1856, p. 475; and April, 1857, pp. 335, 338.

vary in the character of their seasons. A similar explanation may apply to the varying type of the same disease in different seasons of the same year. Dr. Smith thinks it might be affirmed that what are called seasonal diseases depend, more or less directly, upon the degree of vital action existing at that period.

2. The type and complications of a seasonal disease will vary according to the direction of the advancing season.

3. The discovery of the varying states of the human system in cycles helps us to a rational explanation of the cessation of seasonal diseases; for if an epidemic arises from, or only in, a certain state of system, it must increase as that condition of system increases, and must decline when that condition of system is passing into its opposite. Several instances are given, which are thought to illustrate this opinion. We are unable here to discuss this important matter in detail, but it appears to us that the author ascribes too much influence to the condition of the system in the production of certain diseases—such as scarlatina and cholera. The condition of the system is no doubt a most powerful *predisposing*, but cannot be regarded as an *exciting*, cause of such diseases as those just mentioned.

4. The cyclical rotation in the variation of the vital powers may be one of the explanations of the "vis medicatrix naturæ."

IX. *A Contribution to the Statistics of Cancer, collected from the Cancer Records of the Middlesex Hospital.* By SEPTIMUS W. SIBLEY.—This must hereafter be regarded as one of the most important contributions to our knowledge of cancer. In the Middlesex Hospital there are certain wards which are devoted to the exclusive treatment of cancer, and hence the records of this institution bearing upon the disease in question are of peculiar value. It is with great satisfaction that we observe the increasing tendency to the production of such essays as those of Mr. Bryant and Mr. Sibley; reducing, as they do, to a tangible and practical form the immense mass of crude materials stored up in the records of our metropolitan hospitals, and bearing upon every possible question in medicine and surgery.

Mr. Sibley's statistics are founded upon notes of 520 cases, with records of 173 post-mortem examinations. Out of these, 250 cases and 120 post-mortem examinations passed under his own observation. We can do little more than allude to the varied topics upon which Mr. Sibley's statistics have thrown much new light. His observations are classified under the following heads:—Seat of the Cancer; Age; Influence of Marriage, Pregnancy, the Catamenia, &c.; Comparative Duration of Life in the different varieties of Cancer; the Effect of Operation on the Duration of Life; the Hereditary Nature of Cancer; Occurrence of Phthisis in the Families of Cancer Patients; Analysis of the Post-mortem Examinations of Cancer Patients.

Some of Mr. Sibley's conclusions are of great importance, and very different from the ordinarily received opinions upon the subject. Thus, child-bearing would appear to be one of the predisposing causes to the formation of cancer, and sterility to be, to a certain extent, a protection from it. Of the female cancer patients, 55 out of 315 were single; and among the uterine patients taken separately, there were 12 single women out of 135. Of the married women, 86 per cent. among the uterine patients, and 71 per cent. of those suffering from cancer in other organs, had borne children. Again, phthisis was traced in 37 per cent. of the families of the cancer patients; and tubercle was found in 15 out of 172 examinations. In the cases of cancer of the breast, those who had been operated upon lived fifty-three months; whilst those in whom the disease was allowed to take its natural course lived only thirty-two months.

As regards the hereditary nature of the affection, it was found that cancer was traced in the families of $8\frac{3}{4}$ per cent. of the cases. There were five instances in which the patients had two cancerous relations; and in one very remarkable case, five relatives were affected with cancer. In reference to secondary cancer it was found, that the disease was either local, or did not extend beyond the lymphatic glands in about half the cases. There were secondary tumours in 79 per cent. of the breast cancers, in 23 per cent. of the uterine cases, and in 54 per cent. of the instances of true cancer in other organs.

X. *On the Forms and Stages of Bright's Disease of the Kidneys, with especial reference to Diagnosis and Prognosis.* By GEORGE JOHNSON, M.D.—The opinion is gradually gaining ground that several very different morbid conditions of the kidney are included under the general term, "Bright's Disease." The present communication will probably tend to promote this opinion more than anything already published by the same author.

The remarks in the paper before us are confined to the *chronic* forms of Bright's kidney, which for convenience are arranged in two main divisions:—1st, the large white kidney, whether lardaceous or fatty; and, 2ndly, the small, contracted, granular kidney. It has been found:

1. That the urine secreted by the large kidney is less abundant, of higher specific gravity, more constantly and copiously albuminous; and that it usually contains clear, fibrinous, wax-like casts, either with or without oils, but none of the granular casts, which are thrown off from the tubes of the contracting kidney.

2. That the minute anatomy of the two kidneys is very different.

3. That patients who die with enlarged kidneys have almost invariably suffered from dropsy at some period of their history; whereas, the majority of those who die with a contracted kidney have never had dropsy at any time.

Dr. Johnson allows that both the lardaceous and the fatty kidney may, after being enlarged, subsequently become contracted. He maintains, however, that this is of very rare occurrence (although it would seem that there were evidences of incipient contraction in 11 per cent. of his 26 fatal cases of enlarged Bright's kidney); that a fatty kidney is almost invariably found enlarged, even in cases where it is known that the disease has existed for years; that it is easy to distinguish the physical characters of a fatty or lardaceous kidney which has become partially contracted from one which has been contracting from the first. He even believes that, by careful and repeated examinations of the urine, it is by no means difficult to diagnose, during life, the commencement and progress of atrophic change in a fatty kidney. The paper is accompanied by some excellent chromo-lithographs, representing the characteristic appearances of three different forms of contracted Bright's kidney.

XI. *An Account of Three Cases of Aneurism of, or within, the Orbit, treated by Ligature of the Common Carotid Artery, with Observations; to which is added a Report of a Fourth Case, treated since the Paper was read.* By THOMAS NUNNELEY, F.R.C.S.E., Leeds.—Three of these cases proved successful; the fourth patient sank from hæmorrhage on the sixteenth day after the operation. These cases are of considerable interest, as it would appear from the author's researches that but few similar instances have been recorded.

XII. *Case of Aneurism of the Thoracic Aorta, which opened into the Trachea and Left Bronchus, and in which Hæmoptysis occurred four years and eight months before death; with Remarks on the Circumstances attending the Rupture of Aneurisms, especially on Mucous Surfaces.* By W. T. GAIRDNER, M.D.—This case seems to be a unique one as regards the length of time which intervened between the first expectoration of blood (many ounces) and the patient's death. It is very rare for this interval to be so great as it even was in the case of the celebrated Mr. Liston, who died in December, 1847, five months after the first gush of arterial blood from the trachea. Dr. Gairdner makes the following statement as the result of his experience, which, if confirmed by other observers, cannot fail to have a most important bearing on the diagnosis of obscure cases of aneurism, where the physical signs of a tumour do not exist:

"If there be laryngeal dyspnœa and stridulous respiration (which are seldom present to any marked extent in mere laryngeal phthisis); if the epiglottis be not thickened; if the mucous membrane of the larynx, in so far as it is within reach of the finger, be sound; and if, with these signs, positive and negative, there be a persistent tendency to even the slightest amount of blood in the sputum, while auscultation and percussion give negative results, both as regards the lungs and heart, I believe that aneurism may be predicated with as near an approach to certainty as is possible without the physical signs of tumour; and further, the aneurism will be small; it will arise from the back part of the arch, or from the commencement of the innominate artery; and it will be so placed as to entangle either the left or the right recurrent nerve. These considerations have more than once led me to the diagnosis of aneurism under circumstances where, without them, it would have been impossible to give a decided opinion; and hitherto they have not led me wrong."

XIII. *A Case of Popliteal Aneurism successfully treated by Flexion of the Knee-joint.* By ERNEST HART.—The patient was a male, aged forty-one; and there can be no doubt that, under the treatment employed, his aneurism was cured. It may be a matter of question, however, how far the cure was attributable to the compression of the vessel, and how far to the simple rest in bed. We remember to have seen a precisely similar case of popliteal aneurism which had existed for nine months, and in which a cure was effected by confinement to the recumbent posture. The patient had come a long distance to put himself under the care of a celebrated surgeon, whose intention it was to ligature the femoral. As the patient's admission into hospital took place during the vacation, the operation was reserved until the commencement of the session—an interval of three or four weeks; but before this the pulsation had quite ceased, and the aneurismal tumour had well-nigh disappeared. It is to be observed, that Mr. Hart's patient had been taking active exercise previous to his coming under treatment.

XIV. *A Case of Popliteal Aneurism successfully treated by continued Flexion of the Knee-joint.* By ALEXANDER SHAW, Treasurer of the Society.—The treatment adopted in this instance was suggested by the success which attended Mr. Ernest Hart's case. The patient was a male, aged thirty; the aneurism had apparently commenced only a week before the patient's admission into the Middlesex Hospital, and the treatment occupied fifty-six days.

XV. *On some of the Effects of Primary Cancerous Tumours within the Chest.* By GEORGE BUDD, M.D., F.R.S.—The remarks in this paper are founded upon three cases which came under Dr. Budd's observation in King's College Hospital. In all of these cases there was a primary cancerous tumour in the chest, involving the root of the right lung, and accompanied by remarkable inflammatory changes. These changes consisted of firm adhesion of the lung to the walls of the chest; inflammatory condensation of the lung, in places where it was not invaded by the cancer, proceeding in three of the cases to more or less disorganization of the pulmonary tissue and the formation of pockets of pus; and, in one of the cases, where the tumour spread furthest towards the left side, adhesion of the pericardium, with abundant effusion of lymph on its outer surface. The changes in the lungs were confined to the side encroached upon by the cancer, and were hence obviously the result of the morbid growth. Cancer, however, is known to have no direct tendency to cause inflammation of the surrounding parts; indeed, secondary cancerous tumours scattered through the lung are generally surrounded by perfectly healthy pulmonary tissue. Dr. Budd attributes the inflammatory changes in these cases to the tumour involving the root of the lung, so as to implicate or destroy all or a great part of the nerves which supply that organ, and which are necessary for its healthy nutrition. The inflammatory destruction of the eyeball after division of the fifth nerve within the skull, or after destruction of the nerve from disease, and the suppurative inflammation of the lining membrane of the gall-bladder arising from the presence of a cancerous tumour in the portal notch, are quoted as instances of a similar nature.

XVI. *Two Cases of Empyema illustrating the advantage of making two openings, and adopting the plan of "Drainage," in the operation of Paracentesis in that disease.* By S. J. Goodfellow, M.D. *Followed by some Remarks on the Operation and on the plan of Treatment by Drainage generally.* By Campbell de Morgan, F.R.C.S.—Every physician must be aware of the fact that in many cases of empyema it is impossible, after repeated operations, by one opening, to prevent the accumulation of matter in the pleural cavity; and that even in cases where two openings have occurred spontaneously, there has been great difficulty in securing a free exit for the fluid. The retained secretion soon undergoes decomposition, and induces irritative fever, exhausting sweatings, and occasional attacks of diarrhœa; while the fetor of the discharge becomes insupportable. The plan of drainage here recommended with the object of obviating these evils, is the adaptation of a mode of procedure first suggested and practised by Chassaignac for the treatment of sinuses. The operation is a very simple one. The chest is punctured at the usual place between the fifth and sixth ribs, or in any convenient situation. A firm, long, somewhat bent iron probe is then passed through the opening, and directed towards the lower and back part of the cavity, where its extremity is made to press against one of the intercostal spaces, so as to be felt from the outside. An incision is then made over the end of the probe, which is brought through the opening thus made, after which a strong piece of silk thread is passed into the eye of the probe and drawn through the two openings, and the drainage-tube (which is an india-rubber tube having a diameter of about one-sixth of an inch, and perforated at frequent intervals by notching with scissors) being firmly tied to one end of the silk, is by means of this pulled through both openings. The ends of the tube are tied together, and the operation is completed. Two cases are recorded in which this plan of treatment was attended with complete success, and there can be little doubt that its efficacy will soon be tested by other observers. There is every reason to believe that the same treatment will be found to be applicable, not only for the purpose of draining the pleural sac in cases of empyema, but for the radical cure by obliteration, under conditions otherwise favourable, of ovarian sacs, hepatic abscesses, &c.

XVII. *An Inquiry into the Nature of those cases of Strangulated Oblique Inguinal Hernia termed "Réduction en Bloc ou en Masse," with special relation to the actual lesion and practical deductions derived from an examination of the cases.* By John Birkett, F.R.C.S. —"Réduction en bloc" consists in the reduction of a hernia by the forcible efforts of the patient or of the surgeon, the hernia being returned into the abdominal cavity, but external to the peritoneum, together with its investing sac, and being still constricted or strangulated at the neck of the sac. Mr. Birkett's essay is the result of a most elaborate investigation into all that has been recorded concerning these obscure cases, and concludes with some very important practical deductions as regards diagnosis and treatment, which may be summed up as follows:

1. Such cases may occur at any period of life after puberty, but are most frequent between puberty and forty years of age.

2. All the observations on record have been instances of oblique inguinal herniæ, a majority of them being scrotal, and on the right side.

3. A large proportion of the cases have been of that class in which the bowel descends into the vaginal process of the peritoneum, and hence the fact of the lesions having appeared suddenly may aid our diagnosis.

4. In these cases the testis is frequently congenitally absent from the scrotum, and forms a painful swelling in the neighbourhood of the abdominal rings. This is a circumstance which must be borne in mind in diagnosis.

5. In most cases the hernia has consisted of reducible intestine only, and therefore when this was pushed outside the sac and behind the peritoneum, nothing but the sac itself remained as an additional element to the tissues of the scrotum. But in the cases of entero-epiplocele in which the omentum is adherent to the hernial sac, it remains irreducible after the reduction of the bowel, and then more or less swelling is perceptible in the inguinal region.

6. The hernial sac may be ruptured in the application of the taxis, and its contents escape through the laceration, in herniæ of quite recent formation, as well as in those of long-standing.

7. In the majority of cases this injury must be the result of the application of more or less violence, and the surgeon may be led astray in diagnosis if he places too much reliance upon the statements of the employment by the operator of "moderate pressure," "gentle taxis," "no force," &c.

8. In the majority of the recorded cases, the lesion has been characterized by the following local indications:

a. Repeated descents of the hernia after its reduction is supposed to have been effected.

b. Swelling in the iliac fossa.

c. A hard tumour *to be felt* in the inguinal region and canal.

d. Slight protrusion at the external ring after making efforts to produce the rupture.

e. Pain on pressure, and fulness in the iliac fossa.

f. A tumour to be indistinctly felt at the internal abdominal ring.

9. In all the cases the constitutional indications of strangulated intestine have been persistent.

10. As to treatment, it is evident that if the strangulated bowel is not relieved from its constriction, and the impediment to its reduction into the peritoneal cavity removed, the death of the sufferer is certain. To avoid the risk of the occurrence in question, great care in the taxis is recommended, particularly when the patient is under the influence of chloroform, and when the hernia is of that variety which descends into the vaginal process of the peritoneum. When there are even the

slightest indications of the complication, the inguinal region must be explored, for delay is inadmissible. The hernial sac must be sought for, and opened. The finger is then to be passed through the laceration, and the portion of intestine drawn down into the inguinal canal. The finger can then be passed up along the anterior surface of the mesentery, and the constriction at the neck of the hernial sac may be divided. After this, in reducing the hernia, care must be taken to prevent it gliding through the laceration.

The paper well deserves the attentive perusal of the practical surgeon.

XVIII. *On the Administration of Belladonna, and on certain causes which modify its action.* By Henry William Fuller, M.D.—Dr. Fuller has arrived at some very extraordinary results as regards the tolerance of belladonna exhibited by children. Twelve cases are recorded of chorea in female children whose ages varied from eight to nineteen, and to whom extract of belladonna was administered in such doses as would hitherto have been regarded poisonous, yet with comparatively slight effects. In one case, a girl, aged eleven, the quantity of the extract prescribed amounted to sixty-eight grains daily; while in another, a girl, aged ten, it amounted to seventy grains a day, and the total quantity consumed between the 23rd of February and the 20th of March was one thousand and nineteen grains, or rather more than two ounces! The drug in these large doses did not occasion any feverish heat; the patients were pale, but in none was any rash observed; the pulse became very weak, and in some cases was quickened; there was no constipation, but, on the contrary, sickness and diarrhœa were ultimately produced; the tongue was always moist and unusually red; dilatation of the pupil was very uncertain, and did not reach the degree observed when a solution of belladonna is dropped into the eye; in two cases only did the least indistinctness of vision occur; in no case did the drug produce the slightest narcotic effect; in no instance was there any evidence of its accumulation in the system. Every possible means were taken to ensure the purity of the preparations of belladonna employed; the details supplied by the paper leave no doubt upon this point. Again, it was ascertained that the tolerance of the drug was not due to its decomposition in the stomach or to its non-absorption. Dr. Marcet and Mr. Kesteven detected considerable quantities of atropine in the urine.

It was found, however, that the tolerance was not in proportion to the severity of the choreic symptoms, and that its curative effect was very uncertain. In two cases it failed to exercise the slightest control over the spasms; while in three other cases it was doubtful whether the improvement ought to be attributed to its action. Again, it was found from experiments made upon children not affected with chorea, that the tolerance of the remedy was due not to the counteracting influence of the choreic spasms, but in some way to the age of the patient. Adults cannot tolerate the doses of the drug which can be taken with impunity by children; in children larger doses are well borne from the first, and a tolerance of the medicine is speedily established. From his investigations, Dr. Fuller concludes that full doses of belladonna are well deserving a trial in the treatment of whooping-cough, epilepsy, laryngismus stridulus, and in certain forms of dyspepsia connected with infra-mammary pain, flatus, and abdominal spasms.

XIX. *On the Reparative Process of Human Tendons after Subcutaneous Division for the Cure of Deformities, illustrated by a series of specimens from fifteen post-mortem examinations.* By WILLIAM ADAMS, F.R.C.S.—The principal conclusions which Mr. Adams arrives at are :—

1. That tendon is one of the few structures of the body capable of reproduction, and that the newly formed tissue acquires within a few months of its formation the structural characters of the old tendon so perfectly as to be under the microscope with difficulty distinguishable from it.

2. That the new tendon remains during life as a permanent tissue, and that there is no reason for believing that it has any disposition to undergo a process of gradual contraction. When recontraction of the foot takes place, and the deformity returns at a distant period after tenotomy, this does not depend upon absorption of the new material, but upon structural alterations in the muscular tissue.

XX. *Observations on the Medical Administration of Ozonized Oils.* By THEOPHILUS THOMPSON, M.D., F.R.S.—These ozonized oils were prepared by saturating different oils with oxygen gas, and then exposing them for a considerable time to the direct rays of the sun. The presence of ozone was indicated by the bleaching properties acquired by the oils, and by their action upon iodide of potassium and starch. Eighteen cases of phthisis are mentioned which were treated with these ozonized oils. It does not appear that the treatment was productive of any very lasting improvement; its chief effects seemed to be a slight increase in weight, and a remarkable diminution in the rate of the pulse. That this diminution of the pulse was due to the ozone, was rendered highly probable from the circumstance that the same result did not follow the use of the simple oils. In one case the simple and the ozonized oils were alternated three times, and on each occasion with a direct and remarkable alteration of the pulse. The subject introduced by Dr. Thompson is one which merits further investigation.

XXI. *On the Connexion between the Heat of the Body and the Excreted amounts of Urea, Chloride of Sodium, and Urinary Water during a fit of Ague.* By SYDNEY RINGER, M.R.C.S.—The observations forming the subject of this communication were made, at the suggestion of Dr. Parkes, upon two cases of ague, one quotidian and the other tertian, and upon a case of hectic fever occurring in phthisis. The patients were inmates of University College Hospital, where Mr.

Ringer was at the time a resident medical officer. The observations appear to have been conducted with great care and accuracy. No one who has not been engaged in similar investigations can fully appreciate the difficulties to be overcome in order to ensure success, or the great value of such complete records of even a single case as those now before us. For the minute details we must refer our readers to the original paper; we limit ourselves here to a simple statement of the chief results arrived at by the author.

The temperature began to rise previous to the cold stage as experienced by the patient, or before there was any feeling of cold or illness of any kind. The time before the cold stage at which the rise commenced varied. The temperature continued to rise during the entire cold stage, and the rise during this stage was greater than during any other. It reached its highest point during the hot stage, but fell again before the sweating stage, the fall being at first gradual, but during the sweating stage much more rapid. The rapidity of the fall was always in proportion to the slightness of the fit.

A close correspondence in every respect existed between the temperature and the frequency of the pulse.

The urea, chloride of sodium, and urinary water also began to increase in quantity before the commencement of the cold stage. They continued to rise rapidly, and became most abundant either at the termination of the cold, or at the commencement of the hot stage; they commenced to fall in amount before the temperature reached its highest point, and continued to fall, at first slowly, but during the sweating stage rapidly, the rapidity of the fall being proportionate to the slightness of the fit. These constituents always exhibited variations in amount corresponding to the variations in temperature. The variations in temperature often followed similar variations in the amount of urea, but never preceded them.

Quinine administered in a single dose of a scruple when the temperature commenced to rise, lowered the temperature and postponed the fit for an hour that day, but had no other effect on that fit, though it prevented its recurrence next day, another scruple having been taken in the meantime. It was found, however, that after the fit had been arrested by quinine, variations in the urea and chloride of sodium continued to occur at those periods when, had there been a regular fit, the temperature would have risen. The paper is illustrated by numerous tables and twelve charts, showing the relations between the temperature and the quantities of urea and chloride of sodium excreted.

The increase in the amount of urea and chloride of sodium during the cold and hot stages of ague, has been already affirmed by several observers; but Mr. Ringer appears to be the first who has traced any comparison between this increase and the rise in the temperature.

XXII. *Case of Paralysis as to Voluntary Power of the Limbs on one side of the Body, attended by Hyperæsthesia as regards the impressions of Pinching and Pricking on the corresponding side of the Face; being the result of Compression of certain lateral parts of the Brain from* *an Intra-cranial Aneurism; with Observations on "Induced" Cerebral Paralysis.* By JOHN W. OGLE, M.D.—This was an exception to the very general rule of paralysis being on the side of the body opposite to that of the cerebral lesion. The patient was a female, aged forty-six, who had been the subject of epilepsy, and whose symptoms were complete loss of sight, impairment of the senses of smell and taste on the *left* side, with partial loss of muscular power on the *left* side of the body, and contractile hyperæsthesia of the skin of the *left* side of the face and head. After death there was found an aneurism of the anterior cerebellar artery on the *left* side, compressing the *left* crus cerebelli, and the contiguous portions to a slight degree of the pons varolii, cerebellum, and efferent root of the fifth nerve. Fourteen cases have been collected by Dr. Brown-Séquard in which paralysis was observed on the same side of the body as that of the cerebral lesion, and it is a remarkable circumstance that in all these cases the lesion consisted in compression of precisely the same portion of the brain as in Dr. Ogle's case—namely, the inferior surface of the middle cerebellar peduncle. Dr. Brown-Séquard thinks that in such cases we must regard the paralysis as similar to what has been termed "reflex" paralysis, and due to the irritation of centripetal nerve fibres, rather than to any obstruction in the powers of transmission of the efferent nerve fibres—that, in short, the paralysis is due to an "excess" rather than to an "absence" of action. If this explanation be correct, we have an irritative action starting from the point of lesion, and so operating as in some manner or other to paralyse certain motor fibres in the opposite side of the brain, which, decussating at the anterior pyramids, affect the muscles on the side of the body corresponding with the original encephalic lesion.

XXIII. *The Sequel of a Case of Lithotrity, in which a Communication existed between the Bladder and Intestine.* By CHARLES HAWKINS, F.R.C.S.—This case was published in the preceding volume of the Society's 'Transactions.'* On February 2nd, 1858, the patient was reported to be quite free from calculus in the bladder, and there were no symptoms of stone from this time to that of his death, which took place on April 19th, 1859; but the patient continued to pass fæces occasionally with his urine. A fistulous communication was found to exist between the lower part of the posterior wall of the bladder and the sigmoid flexure of the colon.

XXIV. *A Case of Vesical Calculus of unusual size, removed by the Recto-vesical Operation.* By GEORGE SOUTHAM, F.R.C.S., Manchester. —The stone in this case had been growing for sixteen years. After its removal it had an irregularly-oval form, measured eight inches in circumference in one direction, and seven in the other, its extreme length being three inches and a third. It consisted for the most part of earthy and triple phosphates, imbedded in which at one extremity

* Vol. xli. p. 441; and British and Foreign Medico-Chirurgical Review, April, 1859, p. 446.

was a small round alternating calculus, made up of lithic acid and oxalate of lime. The patient was a male, aged twenty-one; the operation was successful, and after four months, all signs of the fistulous communication between the bladder and the rectum had disappeared.

XXV. *On different Forms of Primary Syphilitic Inoculation.* By HENRY LEE, F.R.C.S.—Mr. Lee in this paper mentions four cases in which patients labouring under primary syphilis were inoculated with matter taken either from their own sores or from those of another patient. The conclusion arrived at is, that *suppurating* syphilitic sores are readily inoculable with the point of the lancet upon the patients who bear them, but that the *indurated* sores—those affected with the specific adhesive inflammation, and which alone are capable of giving rise to secondary symptoms—are, as a rule, not capable of being thus inoculated. The author is inclined to believe that these suppurating and indurated sores have often been mistaken for each other, and in this way he accounts for the immunity from constitutional syphilis which has followed the ordinary practice of syphilization. Inoculation with pus from a suppurating sore would not be followed by constitutional syphilis under any circumstances, and although Dr. Sperino, the great advocate of syphilization, frequently asserts that the secretion was taken from an indurated chancre, yet Mr. Lee's experiments would show that the secretion from an indurated chancre is not inoculable upon the same individual. Hence it is inferred that what Dr. Sperino has designated indurated sores belonged in reality to the phlegmonoid or some other variety of suppurating syphilitic sores.

REVIEW VII.

The Nature and Treatment of Gout and Rheumatic Gout. By ALFRED BARING GARROD, M.D., F.R.S., Physician to University College Hospital, &c. &c.—*London*, 1859.

IT must be known to most of the readers of this Journal, that during a considerable period of time Dr. Garrod has been engaged in conducting a series of laborious investigations into the chemical pathology of gout and rheumatism. Some of the results of these labours have been published in the 'Medico-Chirurgical Transactions,'* and they are all embodied in the handsomely-printed and illustrated volume whose title appears at the head of this article. In this treatise Dr. Garrod has given a complete history of gout, commencing with an introductory chapter, in which reference is made to the opinions of some of the chief amongst the ancient writers on this interesting disease.

To attempt a full analysis of the volume is not consistent with our present design; our purpose is rather to direct attention to the chief facts which Dr. Garrod has succeeded in establishing, and to the doctrines, pathological and therapeutical, which he endeavours to build

* Medico-Chirurgical Trans., vol. xxxi. p. 83; vol. xxxvii. pp. 49, 181; vol. xli. p. 325.

upon those facts. Passing over the second and third chapters, in which the well-known outward phenomena of acute and chronic gout are described, we come to the fourth chapter, which treats of the "Blood in Gout."

The normal constituents of the blood in gouty patients are not necessarily changed, but when the disease has been of long duration, and especially when, as frequently happens, the kidneys are degenerated, the density of the serum appears to be somewhat lower than in health, the average density in cases of chronic gout being from 1027 to 1028.

But the great and the characteristic feature of the blood in gout is an excess of uric acid. We say an *excess* of uric acid, for Dr. Garrod has shown that the blood in health contains a trace of both uric acid and urea. The process for the determination of the actual quantity of uric acid in the blood is one which requires a considerable amount of time, care, and skill in practical chemistry. To obviate this difficulty, Dr. Garrod has devised a simple method of ascertaining the presence of an abnormal amount of uric acid, which is readily applicable for clinical purposes, and which requires the abstraction of only a small quantity of blood. This process, which he names the "Uric-acid thread Experiment," is thus performed:

"Take from one to two fluid drachms of the serum of the blood, and put it into a flattened glass dish or capsule; those I prefer are about three inches in diameter and one-third of an inch in depth, which can be readily procured at any glass-house. To this add ordinary strong acetic acid, in the proportion of six minims to each fluid drachm of serum, which usually causes the evolution of a few bubbles of gas. When the fluids are well mixed, introduce a very fine thread, consisting of from one to three ultimate fibres about an inch in length, from a piece of unwashed huckaback or other linen fabric, which should be depressed by means of a small rod, as a probe or the point of a pencil. The glass should then be put aside in a moderately warm place until the serum is quite set and almost dry. The mantelpiece in a room of ordinary temperature, or a bookcase, answers very well, the time varying from twenty-four to forty-eight hours, depending on the warmth and dryness of the atmosphere." (p. 110.)

In explanation of this process, it should be premised that the uric acid as it exists in the blood is combined with soda, and when it is present in quantities above a certain small amount to be presently noticed, the urate of soda being decomposed by the acetic acid, the uric acid thus set free crystallizes and collects on the thread, like sugar-candy upon a string. To detect the crystals, the glass containing the dried serum should be placed under a linear magnifying power of about fifty or sixty. The uric acid is seen in the well-known form of rhombs having a brownish tinge, the size of the crystals varying with the rapidity with which the drying of the serum has been effected, and the quantity of uric acid in the blood. To ensure the perfect success of this process, several precautions relating to the form of the glasses, the strength of the acetic acid, the quality of the thread, &c., are necessary, for the details of which we must refer to Dr. Garrod's book, p.111, et seq.

Degree of delicacy of the above test for uric acid.—The serum of healthy blood, as also that of patients suffering from most diseases, although generally containing a trace of uric acid, gives no indication of its presence by the "uric-acid thread experiment," and this absence of extreme delicacy is practically a valuable quality. By a series of experiments, Dr. Garrod has determined the proportion of uric acid which must exist in the blood before its presence can be thus demonstrated. For this purpose he added urate of soda in definite proportions to the serum of blood taken from a healthy subject, and in which the most careful chemical analysis could scarcely detect the existence of a trace. The result was, that an amount of uric acid equal to at least 0·025 grains in 1000 grains of serum, in addition to the trace existing in health, was required before the thread experiment gave any indication of its presence. Hence, the appearance of uric acid on the thread is complete evidence of an abnormal amount in the blood. Our author, after entering into all the needful details relating to the above test for uric acid, gives in a tabular form a brief report of forty-seven cases of gout, in all of which, by the process in question, the serum of the blood was found to be "rich in uric acid."

He then proceeds to show that by means of the same test, when the blood serum contains an excess of uric acid, this material may also be discovered in the fluid which is effused under the influence of blistering agents applied to the skin. There is an obvious advantage in this means of testing the composition of the blood without the necessity for the performance of venesection. In conducting the thread experiment on blister serum the same precautions are necessary as when examining the blood serum, and this additional circumstance requires attention, namely, that the existence of inflammation of a gouty character has the power of destroying the uric acid in the blood of the inflamed part; so that the serum drawn by a blister over an inflamed gouty joint will give no indication of the presence of the uric acid which is abundant in the blood serum of the same patient.

Another material which the blood of gouty patients often contains in abnormal quantities is urea. This fact was communicated by Dr. Garrod, in the year 1848,* and the observation has been confirmed by his own later observations, as well as by those of Dr. Wm. Budd.† In many cases, doubtless, this excess of urea in the blood is a consequence of the renal degeneration which is so frequent a result of chronic gout, but in other instances this explanation appears not to be admissible. Thus, Dr. Budd gives the particulars of two cases, and refers to nine others, in which he detected urea in the blood, or blister serum, or both, of persons suffering from acute gout, there being at the same time no albumen in the urine, no casts of the uriniferous tubes, nor any other indication of renal disease or obstruction.

The urine in gout.—The clinical examinations of the urine of gouty patients are divided by Dr. Garrod into three classes. The first includes analyses of the urine in cases of acute gout: the second, the results obtained in the chronic forms of the disease: and in the third

* Medico-Chirurgical Transactions, vol. xxxi. † Ibid., vol. xxxviii.

class are placed analyses of the urine of gouty patients during the intervals of the attacks.

Class I.—*Urine of patients suffering from acute gout.*—The analyses of seven cases of this class, occurring in hospital practice, are given. The general result is a decreased excretion of uric acid during the gouty paroxysm. Taking the average normal excretion of uric acid to be eight grains in the twenty-four hours, the details of the seven cases examined show that the highest amount reached was 8·12 grains, the lowest 0·425 grains, the average of the seven cases, 5·95—2·05—2·58—3·76—4·46—3·28—3·28 grains, and the total average derived from all the analyses, 3·62 grains in the twenty-four hours. The quantity of uric acid eliminated during different days by the same individual is liable to much variation. As a rule, in the earlier stages of an attack of acute gout the urine is scanty, and the uric acid, measured by the twenty-four hours' excretion, diminished; when the disease is passing off, the acid is thrown out in much larger quantities, so that the amount excreted may greatly exceed the patient's daily average; lastly, when the fit has terminated, the amount of uric acid is again lessened, though not to the extent observed prior to, or at the commencement of, an attack.

In one of the seven cases above mentioned, the daily amount of urea was found to be 320 grains, a fair average for a patient on a low diet. The excretion of urea appears not to be decreased in the same ratio as the uric acid, although, as before mentioned, an excess of urea in the blood has repeatedly been found during the attacks of acute gout.

In two of the above mentioned seven cases, a very distinct trace of albumen was found in the urine during the attack of gout. This is not a common occurrence during the earlier attacks of gout, but when the disease assumes a chronic form, the urine is frequently found to be albuminous during the paroxysm of gout, though quite free from that principle in the intervals of the attacks.

Class II.—*Urine of subjects affected with chronic gout.*—The particulars of seventeen cases of this kind are given. The results show a marked diminution of uric acid. Thus, in no case did the amount of uric acid in the twenty-four hours exceed 5·78 grains, and this on one day only; the next highest amount was 3·57 grains, and this occurred only on one exceptional day, the usual amount in that case being much smaller. The total average of all the analyses in the second class of cases was exceedingly low, in fact, much less than a single grain.

Several of these urines were also analysed for urea, with the result of showing that while the uric acid was deficient in quantity and subject to great variations from day to day, the amount of urea remained nearly constant, and but little below the normal average. In ten out of these seventeen cases, albumen was present in quantity sufficient to give a distinct haziness when the urine was boiled with the addition of nitric acid, and a notable precipitate after the tube was allowed to remain at rest for a short time. The frequent occurrence of albumen in the urine, and other signs of renal degeneration in cases of chronic gout, had been noticed by other observers, and is quite in accordance with our own experience.

Class III.—*Urine of patients who had suffered more or less frequently from attacks of gout examined at the time of complete freedom from the disease.*—The analyses of six cases of this kind are given by Dr. Garrod, and the result was, that in no one of the six patients did the amount of uric acid excreted in the twenty-four hours exceed the healthy average, while in the majority it was far below. From these and other examinations, Dr. Garrod is inclined to think that in individuals who have suffered frequently from gout, even though no visible deformity of the joints or deposits may have resulted therefrom, the kidneys lose to some extent their power of excreting uric acid; while the blood is contaminated by an excess of the acid, and the excretion of urea is comparatively undiminished.

Microscopic examination of the urine in gout.—In the early stages of gout occurring in individuals otherwise healthy, the microscopic examination of the urine is of little moment; but in the advanced stages of chronic gout, the urine often exhibits microscopic characters of great practical value, more especially with reference to prognosis. In such cases it frequently happens that along with the small quantity of albumen which has been referred to as of common occurrence, we find in the urine which has been allowed to stand for a few hours in a conical glass, a cloudy sediment, composed of casts of the tubes, generally of a granular character, and consisting of disintegrated epithelium, moulded into the form of the tubes. These casts, like the albumen, are sometimes present during the acute exacerbation, but absent in the intervals, and we have repeatedly found them in the urine of gouty patients when no albumen has been discoverable. This appearance of granular casts indicates that a degenerative process is going on in the lining membrane of the uriniferous tubes, a morbid change which is closely connected with deficient secretion of some of the elements of the urine. When the renal degeneration is still further advanced, the granular casts are mixed with others which have been called "large waxy casts," their diameter being equal to that of the uriniferous tubes. The presence of these casts is an indication that the tubes from which they come have been deprived of their lining of gland-cells. Dr. Garrod alludes to the fact that several of the cases of chronic desquamative nephritis related by Dr. George Johnson in his work on 'Diseases of the Kidney,' were patients who had been subjects of chronic gout. There is evidently then a close relation between chronic gout and this form of renal degeneration.

Another noteworthy circumstance is thus referred to by Dr. Garrod:—

"In the latter stages of gout, copious deposits of uric acid or urates are not often met with, and the urine becoming much brighter, and more normal in appearance, usually impresses the patient with the idea that an improvement in his state of health has commenced; whereas in fact it is an indication that the excreting power of the kidneys has become deficient, at least for uric acid." (p. 184.)

The morbid anatomy of gout.—One of the best known, and still one of the most remarkable phenomena of gout, is the frequent deposit of urate of soda in the cartilaginous and fibrous structures which have been the seat of the disease. Dr. Garrod describes these deposits with great care and minuteness of detail, dividing the cases into three classes—namely, 1st. Cases of chronic gout with extensive chalk stones. 2nd. Subjects exhibiting points of deposition on the ears alone, a small nodule on the cartilage of the external ear serving, in some cases, to decide the question of diagnosis between gout and rheumatism. 3rd. Cases in which no external appearance of deposit or deformity existed, but concretions were found in one or more of the joints.

Some beautifully executed woodcuts and chromo-lithographs are introduced to illustrate the appearances produced by the deposit of urate of soda in the ears, the articular cartilages, and other parts. In connexion with this part of the subject, Dr. Garrod considers that he has established the fact that gouty inflammation is *invariably* attended with a crystalline deposit of urate of soda; and the histories of two of his cases certainly afford remarkable evidence in confirmation of this doctrine, so far, at least, as regards the articular cartilages and ligaments. The first case (p. 217) was that of a man who, dying with valvular disease of the heart, was known to have had but two attacks of gout, each in the right great toe. After death a large white patch of urate of soda was found in the cartilage covering the head of the metatarsal bone of the right great toe, and a similar patch on the corresponding hollow surface of the phalangeal bone. There was, besides, a distinct sprinkling of the same substance upon the inner surface of the ligaments. The corresponding joint of the left great toe was healthy, as were all the phalangeal joints of both feet. The second case (Appendix, p. 559) was that of a man, aged 59, who died from the effects of an accident. It was ascertained from his wife that for the last ten years he had been subject to occasional attacks of gout, affecting principally the great toes and ankles, and now and then a joint of the upper extremities. He had also experienced *one slight attack in the left knee.* After death, the urate of soda was found encrusting the metatarso-phalangeal joints of both great toes, and also the left ankle-bones. No trace of deposit was found in the right knee; but in the left knee, which had been the seat of *one slight attack* of gout, there were distinct patches of urate of soda on the articular surface of the femur and patella.

A deposit of the kind here referred to has never been found in connexion with any other articular disease. It never occurs in acute or chronic forms of rheumatism, nor in the disease commonly known by the name of chronic rheumatic arthritis. The deposit is, in fact, a characteristic feature of the specific gouty disease.

The deposited matter is composed of pure urate of soda, it is crystalline, and interstitial in its position. By digestion for some days in warm water, the deposited material may be slowly dissolved out of the articular cartilage. As the solution of the urate proceeds, the deposit assumes the appearance of being composed of small masses of crystals separated from each other by clear interspaces; and after the further action of the water, it has been shown that such masses have occupied the situation of the nucleus cells of the tissue. Dr. Wm. Budd, in the paper before alluded to, pointed out that, in many instances at least, "the cartilage-cell is the focus of each individual deposit—the original centre, within and around which the crystallization occurs."*

Changes in the kidneys of gouty subjects.—Some years since, Dr. Todd drew the attention of the profession to a condition of the kidney which frequently occurs in cases of inveterate chronic gout, and to which he gave the name of "gouty kidney." This so-called gouty kidney has the following characters. It is usually much contracted, being often not more than one-half, or even one-third, the natural size, with a shrivelled appearance, the capsule thickened and opaque, and the surface granular. On section it is found that this decrease of size is chiefly at the expense of the cortical portion, which is sometimes so extremely wasted that the bases of the pyramids almost reach the surface of the organ. In the pyramidal portions of the kidney there are often seen white lines of chalk-like material taking the direction of the straight tubes. When examined under the microscope, this white material is seen to be crystallized in the form of prisms. Chemically tested, it is found to consist of urate of soda, and, in fact, it is identical with the so-called chalk stones in gouty joints. It is soluble to some extent in hot water, yields the murexide test when heated with nitric acid and ammonia, and forms crystalline rhombs when treated with a stronger acid.

The microscopic appearances of the kidney in this form of disease have been carefully examined by Dr. George Johnson, and fully described by him under the name of "chronic desquamative nephritis."

"The change in the early stage of the disease, when the kidneys are still of their natural size and weight, and present to the eye nothing abnormal, appears to consist in an altered condition of the epithelial cells of the convoluted tubes, which become opaque, and have an unusually fine granular appearance. Dr. Johnson states that 'in some tubes there is an appearance of entire cells having been shed, so as to fill the tubes and render them opaque; while in others there is an equal filling and opacity of the tubes, from their containing epithelium in a disintegrated condition, and which has become so, either from the crumbling of the cells while they are still attached to the basement membrane, or from the disintegration of the epithelial cells which have accumulated in the tubes after being shed by a process of desquamation.' There is often besides this an excess of oil in the epithelium.

"After a time the material contained in the tubes becomes disintegrated, and gradually removed by the watery secretion from the Malpighian tufts; and when this is effected, the basement membrane is seen to be left almost denuded, and being partly concealed by the surrounding fibrous rings, it gives to the section a somewhat vesicular appearance. . . . . After a time, and as a result of the removal of the epithelium, the tubes gradually become wasted and shrivelled, the Malpighian bodies consequently approach each other, and hence appear more numerous in the field of the microscope. This change I have observed in all the cases of advanced gout in which I have had an opportunity of examining the kidneys.

* Medico-Chirurgical Transactions, vol. xxxvii. p. 237.

"During the time the above changes are taking place in the urinary tubes, the bloodvessels of the kidney are undergoing a marked alteration; and this is more especially seen in the Malpighian arteries and capillaries, the coats of which are much thickened and hypertrophied. Dr. Johnson says that both the circular and the longitudinal fibres of the arteries become affected, but that the longitudinal (which are naturally thinner than the circular) are increased more than the other set, and hence when diseased they become of about equal thickness." (p. 243.)

The minute and, we believe, accurate description of the structural changes in the gouty kidney which we have here quoted at length, is illustrated by three woodcuts, and these are the only illustrations in Dr. Garrod's book which are not entirely successful. A linear magnifying power of 100 has evidently been insufficient to bring out the characteristic appearances in the tubes and bloodvessels of the kidney which the author desired to have represented.

This, then, being the condition of kidney which is commonly found in the subjects of inveterate chronic gout, the question arises—Is not the same state of kidney associated with other forms of disease than gout? Dr. Garrod admits that, "so far as the contracted or atrophied state of the organ is concerned, the question must be answered in the affirmative." A like contracted form of kidney, the result of chronic desquamative nephritis, is not unfrequently found unassociated with a history of gout. The urine, too, in these cases has been found to have the same physical characters, as to the low specific gravity, the pale colour, the presence of albumen, and likewise the occurrence of the granular and waxy casts of the uriniferous tubes.

At the time when the chapter on the Morbid Anatomy of Gout was passing through the press, Dr. Garrod was of opinion that the one characteristic feature of the gouty kidney was the presence of the white streaks of urate of soda in the medullary cones. But during the interval between this and the printing of the Appendix (p. 562), inquiries had been made, the result of which tends to show that the deposits of urate of soda in the cones are *not* characteristic of gout.

Mr. W. Hickman, late physician's assistant, now house-surgeon to University College Hospital, carefully examined the kidneys in 23 successive autopsies of persons dying from various causes, some from accident, but most from disease. One individual had suffered from gout, and in that case the deposits were found in the joints and in the kidney; in 19 out of the 22 remaining cases no deposits of urate of soda were discovered; in 3, however, in whose joints no evidence of gouty deposit was visible, crystals were seen in the kidneys, some composed of uric acid, others of urate of soda. Dr. Garrod considers that there are minute differences between the deposits in the two classes of cases; we think, however, that these observations show conclusively that a crystalline deposit in the medullary cones of a contracted kidney is no proof of the gouty origin of the renal degeneration.

Dr. Garrod is disposed to think that even in the very early stages of gout the kidneys begin to undergo considerable structural change; and in support of this opinion he lays particular stress on the case of a surgeon (p. 241), who had suffered from only eight attacks of gout, extending over a period of thirteen years. The kidneys were apparently healthy, each weighed four ounces and a half, and the capsule peeled off without difficulty; but when closely inspected, the white crystalline deposits were found in the pyramids. The microscope also revealed the commencement of other mischief; and a portion sent to Dr. George Johnson, who was unacquainted with the case, elicited the following remarks:—

"The epithelium in some of the tubes is very granular, and there are a few denuded tubes; the chief change in the kidney is an excess of oil in the epithelium; the outline of the tubes is very dark, depending on an accumulation of small oil globules in the epithelial lining."

Here, doubtless, we have evidence of considerable structural change in the kidney, but that this was simply a result of the eight attacks of gout, from which this gentleman had suffered, is by no means certain, nay, is highly improbable. On turning to the history of the case (at p. 215), we learn that "about six months before his death he began to suffer from symptoms of hepatic disease, and afterwards from ascites, for which he was tapped. His death resulted from the exhaustion of diarrhœa following the operation." With such a history of serious chronic disease within the abdomen, we should not have expected to find the kidneys healthy, nor were the structural changes which they presented greater than are commonly found in the kidneys of men who die after long continued cardiac or hepatic disease. In a case, therefore, of disease so complicated, it is impossible to estimate the amount of influence which the gouty element may have had in causing the renal degeneration.

That the kidneys are often very seriously diseased in the subjects of chronic gout is a notorious fact, and that this peculiar form of renal degeneration is most insidious in its origin and progress is unquestionable; but we see no reason to believe that the kidneys have commonly undergone any material structural change after a few attacks of ordinary acute gout.

Causes of gout.—The subject of the causes of gout, whether predisposing or exciting, is one respecting which an author in the present day has little prospect of being able to advance any novelty. The common observation and the accumulated experience of patients and practitioners have placed most of the facts in so clear a light that he who runs may read them. In a very large proportion of cases, about fifty per cent. of the whole, the sufferers from gout inherit a tendency to the disease from one or both parents. Women are much less subject to gout than men, for the reason that they are much less exposed to the influence of the most powerful of the predisposing and exciting causes of the disease. Youth enjoys almost complete immunity from gout. Dr. Garrod remarks with truth, that—

"Between genuine gout and true rheumatism, such as is typified in cases of rheumatic fever, a marked distinction is seen in the influence of age, the former occurring most commonly for the first time after thirty-five, the latter seldom met with when that period of life has been attained."

No truth in medicine is better established than that the free use of *alcoholic liquors* is the most potent of the predisposing causes of gout, the one without which it is probable that all other influences would have been insufficient to originate the disease. Nothing again is more certain than that the power possessed by fermented liquors as causes of gout is not simply in proportion to the amount of alcohol which they contain. The lower classes in Scotland and Ireland drink strong whisky in abundance, and fall victims to hepatic and renal disease, but very rarely do they suffer from gout; while the London draymen, and coal-porters, and ballast-heavers, imbibe their full draughts of porter, and have gout in its fiercest forms. Amongst wines, however, the stronger varieties, which are largely consumed in this country, such as port and sherry, are more powerful as predisposing causes of gout than the lighter and less spirituous French and German wines. The knowledge which we possess of the composition of the different kinds of fermented liquors throws no light on their varying powers of inducing gout. In reference to this subject, the following are the only conclusions which, as Dr. Garrod suggests, can be safely drawn—

"1. Diluted alcohol, in the form of distilled spirits, has little power in causing gout, at least in those who are not predisposed to it.

"2. Alcohol, when in combination with other substances, as occurs in wines and malt liquors, becomes a potent cause of gout, and the greater the amount of contained spirit, the more powerful the influence in producing the disease.

"3. Neither the acid, sugar, nor any known principle contained in these liquors, can as yet be proved to impart to the alcohol its predisposing influence; for wines the least acid, and liquors the least sweet, are often among the most baneful."

To which we may add with some probability of truth—

"4. Alcoholic liquids which have little tendency to cause dyspepsia, and those which act more especially as diuretics, can, as far as gout is concerned, be taken with greater impunity than beverages of an opposite character."

It is difficult to estimate the separate influence which dyspepsia, a superabundance of animal food, and want of exercise, may have as predisposing causes of gout, It is certain that some forms of dyspepsia which may seriously impair the general nutrition of the body, have little or no tendency to induce gout; and it seems probable that those varieties of dyspepsia which are attended with an excessive formation of uric acid in the system are the most prone to end in gout.

With regard to the influence of inactive and indolent habits in favouring the production of dyspepsia and gout, it is not to be doubted that this is considerable; but the case of the ballast-heavers in the Thames, referred to by Dr. W. Budd, affords an interesting illustration of the fact that no amount of labour will serve as an antidote against the immoderate indulgence in certain kinds of fermented liquors. The occupation of these men is a most laborious one, occasioning profuse sweating and much exhaustion. Each labourer drinks from two to three gallons of porter daily, and generally a considerable amount of spirits besides. The result is, that although not a numerous body of men, many of them, affected with gout, are yearly admitted into the Seamen's Hospital Ship. The influence of prolonged and severe study, of mental anxiety, and other depressing agencies, is notoriously great in favouring the operation of other recognised causes of gout. With regard to the influence of climate and season little need be said. The inhabitants of tropical countries are strangers to gout, they are equally strangers to the highly animalized diet, the strong wines and malt liquors, by the abuse of which Europeans have acquired the disease. That season has some influence in determining the period of the attack has been recognised since the time of Hippocrates, one of whose aphorisms is, "Podagrici affectus vere et autumno plerumque moventur." Dr. Garrod having examined a large number of gouty patients with reference to the time of the year when the attacks have been most frequent, finds that, in accordance with the opinion of the ancient writers, the earlier attacks most commonly occur in the spring; after a time, an autumnal fit is added; and when the disease has become inveterate, the returns of inflammation are more frequent, and the intervals irregular.

Influence of lead as a predisposing cause of gout.—In a paper which was published in the 'Medico-Chirurgical Transactions' in the year 1854 (vol. xxxvii. p. 211), Dr. Garrod alluded to the fact that a very large proportion, at least one in four, of the gouty patients who had come under his care in the hospital had at some period of their lives been affected with lead poisoning, and for the most part followed the occupation of plumbers and painters. The later experience of Dr. Garrod, Dr. Burrowes, and other physicians, is to the effect that workers in lead are in a more than ordinary degree liable to be affected by gout. The result of our own observations is quite in accordance with those of Dr. Garrod; but we dissent entirely from his statement that "there appears to be nothing in the habits of these men capable of accounting for their peculiar liability to gout, with the exception of their being exposed to the influence of lead." Our experience amongst workmen of this class has led us to the conclusion that they are, for the most part, large consumers of fermented liquors. Dr. Garrod does not venture to affirm that lead impregnation can induce gout, without the concurrent influence of fermented liquors; and he alludes to one circumstance which, as he admits, tends to show that lead alone does not very powerfully predispose to gout—namely, that women engaged in white lead manufactories, and who often suffer from colic, are not afflicted with gout in like ratio with men. Admitting, as we must, that slow poisoning by lead does in some way act as a powerful predisposing cause of gout, the question arises—What is its mode of operation? We venture to suggest that its action may in part be explained by its anæmiating and debilitating influence lessening the vital power in the body of resisting the effects of a too free indulgence in alcoholic liquors. Dr. Garrod has chemically investigated the action of lead as a predisposing cause of gout, by ascertaining, first, the condition of the blood and urine of patients under the influence of this poison; and, secondly, by determining the effect which lead, when administered medicinally, has upon the secretion of uric acid. With regard to the first part of the inquiry, he finds that an excess of uric acid is frequently, though not constantly, present in the blood in cases of lead poisoning, not only in those who have previously suffered from gout, but even when no symptoms of the disease had ever shown themselves. In the same cases, too, there appeared to be a diminished excretion of uric acid by the kidneys. In two cases the urine was repeatedly analysed while acetate of lead was being given in medicinal doses. In both patients a well-marked diminution of the excreted uric acid occurred under the influence of lead; in the first this was most decided, the amount excreted being less than one-half of that passed when the metal was not administered; in the second it was also evident, though not considerable. It would appear, therefore, from these observations of Dr. Garrod, that in the subjects of lead-poisoning the blood is apt to become loaded with uric acid in consequence of the imperfect excretion of that principle by the kidney.

Pathology and nature of gout.—We have now to inquire whether, by the aid of the facts which have been set forth, the phenomena of gout admit of a complete and rational explanation. Dr. Garrod's views respecting this interesting subject are succinctly stated in the following nine propositions, upon some of which we shall presently have to offer a few words of comment:

"First. In true gout uric acid is invariably present in the blood in abnormal quantities, in the form of urate of soda, both prior to and at the period of the seizure, and is essential to its production; but it can be equally proved that this acid may occasionally exist largely in the circulating fluid, without the development of inflammatory symptoms—as, for example, in cases of lead-poisoning, and a few other instances. Its mere presence, therefore, does not explain the occurrence of the gouty paroxysm.

"Secondly. The investigations recently made in the morbid anatomy of gout prove incontestably that true gouty inflammation is *always* accompanied with a deposition of urate of soda in the inflamed part.

"Thirdly. The deposit is crystalline and interstitial; and when once the cartilages and ligamentous structures become infiltrated, such deposition remains for a lengthened time, perhaps for life.

"Fourthly. The deposited urate of soda may be looked upon as the cause, and not the effect, of the gouty inflammation.

"Fifthly. The inflammation of gout tends to the destruction of the urate of soda in the blood of the part, and consequently of the system generally.

"Sixthly. The kidneys are implicated in gout, probably in its early, and certainly in its chronic forms; and this affection is not only functional, but subsequently becomes structural; the urinary secretion is also altered in composition.

"Seventhly. An impure state of the blood, arising principally from the presence of urate of soda, is the probable cause of the disturbances which not unfrequently precede the seizure, and of many of the anomalous symptoms to which gouty subjects are liable.

"Eighthly. The causes which predispose to gout, independently of those connected with individual peculiarity, are either such as produce an increased formation of uric acid in the system, or which lead to its retention in the blood."

"Ninthly. The causes exciting a gouty fit are those which induce a less alkaline condition of the blood, or which greatly augment the formation of uric acid, or such as temporarily check the power of the kidneys for eliminating this principle." (pp. 240-1.)

With regard to the first of the above propositions, we consider that Dr. Garrod has established its truth by a sufficient number of careful and accurate observations.

The three succeeding propositions must be taken in conjunction, and we think that the evidence respecting them is far from complete or satisfactory. The statement that gouty inflammation is *always* accompanied by a deposit of urate of soda in the inflamed part, is scarcely warranted by the fact, that acute gouty inflammation of a joint leaves a deposit in the extra-vascular cartilages and ligaments. Then there are weighty objections to the theory that the deposited urate of soda is the cause, and not the effect, of the gouty inflammation. The cartilages and ligaments being the only parts in which after a primary attack of gout in a joint we have any evidence of a crystalline deposit, is it conceivable that a deposit in those tissues can be the cause of the intense inflammation which affects the surrounding soft parts? Again, what explanation can be given of the fact that acute gouty inflammation almost invariably passes away in a few days, while its assumed cause, the deposited urate of soda, "remains for a lengthened period—perhaps during life?" After a careful study of Dr. Garrod's facts and arguments, we think that a mystery still hangs over the phenomena of the gouty paroxysm, which, with all his labour and ingenuity, he has not succeeded in penetrating, and which we shall not attempt to solve. And in opposition to Dr. Garrod's fourth proposition, we are inclined to the belief that the urate of soda in the cartilages is a consequence, and not the cause, of the inflammation: that it is, in fact, a deposit left by the ebbing tide of inflammation—a deposit which each successive flow tends to increase. Dr. Garrod suggests that the reason why deposits of urate of soda so constantly take place in ligaments and cartilages may be, first, that these structures possessing but little vascularity, the deposit is there placed beyond the further influence of the blood-vessels; and secondly, it is probable that the fluids of these tissues are less alkaline than those of many others, and certainly less alkaline than the blood itself.

The statement that the inflammation of gout tends to the destruction of the uric acid in the blood of the part, is chiefly based upon the fact before mentioned, that the serum effused under the influence of a blister on the inflamed part, gives no evidence of containing uric acid, when the blood of the patient and the blister-serum from another portion not so inflamed readily shows its presence. It would thus appear that the gouty fit, although productive of local mischief, is a salutary process, tending to rid the system of part of the accumulated uric acid.

Treatment of gout.—The important subject of the treatment of gout in its various forms is very fully discussed by Dr. Garrod, more than one hundred and thirty pages of his treatise being devoted to its consideration. Without attempting to follow our author through this part of his work, in which he displays the skill and judgment of an experienced and careful practitioner, we select for analysis and comment one or two of the more important subjects which are there treated of.

One of the most interesting chapters in the book is that which has for its subject the use and therapeutic action of *colchicum*.

It is admitted by most physicians of experience that colchicum has a powerful therapeutic influence, not only in the regular forms of gout, but also in some of the irregular and masked forms of the disease. It has a decided effect in lessening the pain of gout, and this without necessarily acting as a purgative. Moreover, brisk purging by other means has no such alleviating effect. Colchicum often produces a marked sedative influence on the heart and other parts of the circulating system, as is manifest from the slowness of pulse which it frequently causes. But that its action in gout is not simply sedative, is shown by the fact that in its power of controlling other forms of inflammation it is much inferior to other sedatives, such as tartar-emetic, while the latter drug, though a powerful sedative, is much inferior to colchicum as a remedy for gout.

If, then, the action of colchicum cannot be explained either by its purgative property or by its sedative influence on the vascular system, it is reasonable to inquire whether its effect on the kidneys and the urinary secretion will afford any clue to its *modus operandi*.

Dr. Christison found that after giving colchicum to a patient for two days, the quantity of urea in a given weight of urine was nearly doubled, and the urine was turbid with urates. This experiment would appear to show that colchicum has the power of increasing the elimination of urea and uric acid; but on further inquiry, a source of error is discovered. It appears that Dr. Christison took specimens of urine for analysis without reference to the quantity passed in the twenty-four hours, so that, although after the administration of the colchicum a given sample was richer in urea and uric acid, no proof was afforded that their total daily elimination was augmented; on the contrary, they may, as Dr. Garrod suggests, have been decreased, for a notable diminution in the quantity of the urinary secretion often occurs from the purgative action of colchicum.

Dr. Maclagan, who made similar experiments, arrived at like results, the analyses being made on specimens of urine passed at particular times of the day, with no attempt to show the daily averages of urea and uric acid excreted. Professor Chelius, of Heidelberg, made some experiments, the results of which are apparently in favour of the idea that colchicum increases the excretion of uric acid. He found in one case that the amount of this acid was nearly doubled; but Dr. Garrod states that the observation was made on a patient who was recovering from an attack of gout, when an increased excretion of uric acid is a common phenomenon without the administration of any medicine. The late Dr. Graves was of opinion that the power of colchicum was due to its lessening the formation of uric acid in the system, and not to any increased elimination by the urine.

With the view of ascertaining the action of colchicum upon the urinary secretion, Dr. Garrod made a considerable number of analyses of the urine in several cases, and he gives the general results of his inquiry in the following terms:

"1st. There is no evidence that colchicum produces any of its effects upon the system by causing the kidneys to eliminate an increased quantity of uric acid—in fact, when the use of the drug is continued for any lengthened time, it appears to exert a contrary effect.

"2ndly. From the observations above detailed, we cannot assert that colchicum has any influence upon the excretion of urea or the remaining solid portion of the urine.

"3rdly. Colchicum by no means acts in all cases as a diuretic, but, on the contrary, it often diminishes the quantity of urine, especially when it produces a marked effect upon the secretions from the alimentary canal. . . . . As the operation of colchicum can be explained neither by its purgative effects nor by its power of altering the character of the blood and urine, its real mode of action is still a subject for inquiry, and well worthy of occupying the attention of the therapeutist." (p. 403.)

It has been supposed by Dr. Todd and some other physicians, that while colchicum relieves pain and shortens the fit of gout, it also lessens the interval between the attacks. It seems doubtful whether a cautious and discriminating employment of the drug is attended with this unfavourable result; and with reference to this point, it should be remembered, that one of the most constant features in the natural history of gout, is that when the disease has frequently recurred, the duration of the fits is increased, and the intervals are shortened. Dr. Garrod refers to one case of fifteen years' duration, in which, the patient never having had recourse to medicines, this natural tendency in the gouty attacks to return with increasing frequency and severity was well illustrated. Dr. Garrod estimates the therapeutic value of colchicum very highly, and expresses his belief that it "possesses as specific a control over true gouty inflammation as cinchona barks over intermittent diseases."

Lithia salts in the treatment of gout.—One of the most remarkable properties of the fixed alkali, lithia, is its power of imparting solubility to uric acid, the urate of lithia being more soluble than any other urate. Some years since, Mr. Alexander Ure* instituted a series of experiments in order to determine the solvent powers of carbonate of lithia on uric acid. He found that when a solution of one grain of carbonate of lithia in distilled water was brought to a temperature of 98°, and pure uric acid added until it ceased to dissolve, the quantity taken up was 2·3 grains. He also ascertained that the solvent power of carbonate of lithia on uric acid is more than double that of carbonate of soda, nearly double that of carbonate of potash or borax, and about eight times that of bicarbonate of soda, which is the active ingredient of the Vichy water. Mr. Ure found that a human urinary calculus, composed of uric acid with alternate layers of oxalate of lime, when placed in a solution of four grains of carbonate of lithia, and maintained at a blood heat for five hours, lost five grains in weight. From these experiments he inferred that a solution of carbonate of lithia injected into the bladder might be used as a solvent for stone; but the extreme scarcity of the salt prevented him from carrying his proposal into practice.

* Pharmaceutical Journal, vol. iii. p. 71.

Within the last two years Dr. Garrod has made many trials of carbonate of lithia as an internal remedy, both in cases of uric acid gravel, and also in several cases of chronic gout, and he expresses himself "much satisfied with the results." He states that when given internally in doses of from one to four grains dissolved in water, and repeated two or three times a day, it produces no direct physiological symptom; but when patients are passing uric acid gravel it lessens or entirely removes the deposit. In many instances, too, in which he has administered this salt to gouty subjects, "the result has been to diminish the frequency of the attacks, and altogether improve the condition of the patient." No details of these cases are given; we are therefore not in a position to form an opinion as to the supposed curative action of the drug. *A priori*, we should not anticipate more benefit from the carbonate of lithia, than from somewhat larger doses of the far less costly carbonate of potash; but the question can be decided only by an appeal to careful clinical observation.

Rheumatic gout.—The concluding chapter in Dr. Garrod's work treats of the various forms of disease which have been included under the head of rheumatic gout, and the diagnosis of these affections. When either acute gout or acute rheumatism occurs in a typical form, the characteristic features of each disease are so striking that the diagnosis is attended with no difficulty. In the chronic stages of these maladies, however, the distinction is sometimes by no means easy. An investigation of the history will often throw much light on the question of diagnosis. If the disease originally commenced in the toe, and gradually in after attacks extended to other and larger joints, if it began about the middle age, and the patient had indulged freely in wine and malt liquors, then the case is in all probability one of gout. In many cases of chronic gout a small spot or two of urate of soda on the external ear will clear up the diagnosis. When there are no external signs of deposit, an examination of the blood or blister-serum may be made, bearing in mind that in gout the blood always contains an excess of uric acid, while in rheumatism no such excess has ever been found. A careful attention to the effect of drugs has often afforded additional aid in diagnosis, the inflammation of gout being relieved by colchicum in a much more decided manner than rheumatic inflammation.

Gout and rheumatism having been shown to be essentially distinct diseases, the question arises, can they co-exist, and is there a disease to which the term *rheumatic gout* can properly be applied? It is possible that rheumatic fever may attack an individual who is of a gouty diathesis, and a patient having suffered from rheumatic fever in youth may in after years be subject to gout, but Dr. Garrod denies, and we are disposed to agree with him, that a combination of the two diseases, as assumed by the title, is ever seen in nature. The term rheumatic gout is applied to different forms of disease—often to cases of true gout, when it has frequently recurred and affected the larger joints of the lower and upper extremities. Sometimes it is applied to cases of chronic or subacute rheumatism, more especially when not commencing with rheumatic fever.

But the name rheumatic gout is more frequently given by the profession to that distressing and intractable disease which has been fully described by Dr. Adams, of Dublin, under the title of chronic rheumatic arthritis. This disease having a peculiar pathology, in no way related to gout, and not necessarily to rheumatism, Dr. Garrod proposes to designate "*rheumatoid arthritis.*" It produces great distortion, and often nodosity of the joints, but is not associated with an excess of uric acid in the blood, or a deposit of urate of soda in the cartilages and ligaments. Dr. Garrod gives a full description of this disease, with woodcuts illustrating the peculiar deformity which it occasions in the hands. He also gives a table setting forth in a very clear and intelligible manner the differential diagnosis of gout, rheumatism, and rheumatoid arthritis. For this and for many other interesting details of which we have here made no mention, we must refer our readers to the work itself, of which we may say, in conclusion, that it is in the highest degree creditable to Dr. Garrod as a scientific pathologist and a practical physician.

REVIEW VIII.

Lectures on the Development of the Gravid Uterus. By WILLIAM O. PRIESTLEY, M.D., &c.—*London*, 1860. 8vo, pp. 110.

THESE lectures were first delivered at the Grosvenor-place School of Medicine, and were subsequently published in the 'Medical Times and Gazette.' The didactic rather than original character of the lectures, and their recent publication in a widely-circulating journal, obviate the necessity for very minute analysis here. These circumstances do not however detract from the merit or usefulness of the work. The lectures are carefully wrought, and present an excellent view of a subject upon which it is very essential for student and practitioner to be well informed. That the author should have had the courage to devote eight lectures, out of the narrow three-months' course into which the examining bodies have seen fit to compress all that they think need be known of obstetrics, to the development of the uterus, is a title to praise. It behoves the teacher at least not to be borne down by the examiner. It is his duty to rescue the student from that condition of ignorance to which the regulations of colleges would condemn him. If corporations discourage obstetric knowledge, it is the more incumbent on the teacher to exert himself in order to counteract the depressing influence. If the pure physicians and surgeons whose sway is paramount in colleges do not know how important is obstetric skill, not alone to the welfare of the community, but to the peace of mind and reputation of the practitioner, the obstetric physician has no such excuse for conniving at the slur that others would cast upon his department of medicine. These eight lectures, devoted to the fundamental subject of the development of the gravid womb, may be taken as a fitting protest against the absurd limitation of "Midwifery and the Diseases of Women and Children" to a three-mon hs' course. The subject is all-important, and the treatment cannot be called diffuse. It will not bear omission or curtailment. But if it receive the due share of attention, what time remains for the many other scientific and practical questions with which the rapidly progressive department of obstetrics teems? Resting, as it now does, upon a broad physiological basis, obstetricy is an eminently demonstrative art. Being surgical in its nature, calling for a high degree of manual and operative dexterity, it admits of and demands a course of minute physical demonstration.

We have followed our author by thus adverting to a mischievous defect in the regulation of those bodies which, having the control of the examinations, necessarily dictate so powerfully the mode and extent of education. We will now discuss the matter he has laid before us.

The lectures embrace the consideration of the changes wrought in the mucous membrane and muscular wall of the uterus by impregnation; the development, structure, and morbid anatomy of the chorion and amnion; the structure and physiology of the placenta; and the changes which mark the return of the uterus to its ordinary state after delivery.

With all precise observers who take the histological characters of the tissues for their guide, Dr. Priestley recognises the identity of the decidua with the mucous membrane. The recognition of this fact disperses a crowd of false and doubtful ideas. It settles the much litigated question as to the number of openings into the decidua. Has it one, two, three, or more? Are the uterine extremities of the Fallopian tubes and the os internum closed or open? If the decidua be merely the mucous membrane highly developed and hypertrophied, then it is undertood that the natural openings must remain open. And this indeed is what actual observation demonstrates. The testimony of an anonymous reviewer can of course command no greater respect than attaches to impersonality; but, giving it for as much as it is worth, we may say that, under careful observation, the continuity of the mucous membrane up from the cervical canal, along the uterine walls, and through to the Fallopian tubes, may always be traced. If there be any barrier at the passages, it is only apparent, an imperfect occlusion caused by the approximation or overlapping of the swollen folds of the membrane. Since the description given by Dr. Priestley agrees pretty closely with that now generally admitted to be correct, we need not follow him closely. In the interest, however, of justice, which the critic is bound to watch, we must check now and then the over-ready disposition of the admiring pupil to attribute to Professor Simpson more of obstetric progress than is his due. In speaking of the nature of the dysmenorrhœal membrane, Dr. Priestley says that "Professor Simpson first showed that it consisted of the exfoliated mucous membrane itself, and was in all respects identical with the newly-formed decidua, except in the absence of impregnation as an exciting cause."

We were under the impression that the very accurate researches and demonstrations of Dr. Oldham, first published in the 'Medical

Gazette,' 1846, but previously described in his public lectures, and subsequently recorded in reviews in this journal, were antecedent to those of Dr. Simpson. A leading idea of Dr. Oldham was, that the dysmenorrhœal membrane is, like the true decidua of pregnancy, equally called into development by the stimulus of ovarian excitation. We do not see how the analogy can be carried further, or what there was left for any one else to work out in this direction. Agreeing generally with the views of Dr. Oldham, we may however express an opinion that a certain degree of inflammatory action of the lining membrane of the uterus is present in these cases, and that the shedding of the membrane is much facilitated by the readiness with which the inflammatory exudations enter into fatty degeneration.

With reference to the decidua reflexa—the nature and mode of formation of which are yet more keenly disputed than are those of the decidua uterina—Dr. Priestley objects to the view maintained by Coste, Breschet, and Sharpey—namely, that the ovum enters the cavity of the decidua, and sinking into a fold of the already hypertrophied membrane, thus swelling into convoluted rugæ, carries this fold before it, which, continuing to grow, forms the decidua reflexa. The author urges:

"A great objection (to this) exists in the fact pointed out by Weber and Robin, that when examined, the decidua ovuli exhibits over its entire outer surface, or at least the greater portion of it, the same pits or depressions which are found on the opposed or inner surface of the decidua uteri, and which there indicate the orifices of glandular tubules. Moreover, the canals which these depressions lead to have many of them a direction at right angles to the surface, and they are paved with nucleated epithelium, as in the outer decidua. It is quite incomprehensible that the entrance of the canals should have this relation to the surface, and occupy so large an extent of it, till the decidua begin to vegetate round it from a small and localized spot. Besides, so far as I am aware, no one has ever observed the ovum resting in a cup-shaped growth of the primary decidua, or in any of the intermediate stages of growth, short of complete inclusion, if we except a small deficiency described by M. Coste as sometimes found in the decidua ovuli at its most prominent portion, and which may have been produced by the distension from within. I feel disposed, therefore, to accept as more probable a suggestion thrown out by Weber, and which corresponds closely with the opinion formed by Mr. Goodsir, that the decidua reflexa or ovuli is actually the primary lamina secreted before the ovum enters the uterus, which separates in two-thirds of its extent from the layers immediately beneath it, to adhere to the ovum and retain it in position, the remaining third not separating, but remaining as a centre of nutrition by its union with the womb."

Now, to this it may be answered, that if we admit, as we must do, that the decidua reflexa *grows* with the growth of the ovum, as the decidua uterina grows with the growth of the uterus, there is no difficulty in acceding to the view of Sharpey and Coste. At any rate, we may observe that, since both parties agree in assigning to the decidua reflexa a uterine origin, the dispute is one rather of physiological nicety than of practical import. The pathology of the decidua reflexa is not affected.

Although it is a departure from the author's order, we think it convenient here, in order to trace connectedly the decidua throughout its history, to examine his views as to what becomes of the decidua, and how the mucous membrane of the uterus is restored after impregnation. His observations confirm the opinion of those who maintain that the mucous membrane does not wholly fall, leaving the muscular walls bare. He disputes the accuracy of the comparison made by Cruveilhier between the uterus after delivery and an amputated stump. The following, as a good anatomical observation, is worth extracting:

"The uterus was contracted to about the size of a man's fist, its cavity was nearly seven inches long, its walls were thicker than in the unimpregnated condition of the organ, and looser and softer in texture; the veins were very large and numerous, but generally empty; the os uteri was irregularly ecchymosed, and its mucous membrane fissured; the internal surface of the uterus was covered everywhere with dark coagulated blood, but on carefully removing this and passing over the preparation a gentle stream of water, the uterine parietes were exposed. The interior of the uterus might then be described as consisting of three portions, each presenting different appearances. One portion near the fundus, equal to about a third of the whole, was recognised as the placental spot. It was more or less circular in form, slightly elevated above the rest, and darker in colour; its surface was lacerated and uneven, and shreds of tissue floated loosely out from it when placed in water. The orifices of the vessels which conveyed maternal blood to and from the placenta, and which were torn across in its separation, were readily distinguishable, dark clots plugging them, and projecting into the uterine cavity.

"The second portion comprehended the remainder of the inner plane of the uterus as far downwards as a line corresponding to the os uteri internum, and the third lay between this and the os uteri externum. The former of these divisions had a reddish colour, was everywhere irregular, as though torn, and when the organ was immersed in water, exhibited innumerable flocculent processes or shreds attached to it, similar to those on the placental spot, indicating the recent tearing off of a superimposed layer. Here and there could be seen bundles of muscular fibres, not unlike the columnæ carneæ of the heart, elevated above the surface, and sometimes quite smooth in outline, as though bereft of all covering. No vascular apertures were visible here, as over the placental spot, nor was it possible to detect any of the pits or depressions indicating the follicular apertures. The flocculent processes from both the above-described divisions were found under the microscope to consist of cellular and fibro-cellular structures, largely mixed with fat granules, identical in form with those composing the deep layers of the decidua uteri in the latter months of pregnancy. None of the floating shreds consisted of lacerated bundles of muscular fibres, and even where muscular columns projected above the surface and were apparently denuded, a delicate investing layer of nucleated particles was invariably present. That portion of the interior corresponding to the cervix uteri presented very different characters. There was a mucous membrane as distinct as before pregnancy commenced; its rugæ were, however, unfolded, only slight traces of the arbor vitæ being discoverable, and lacerations appeared here and there. A copious viscid secretion covered the surface, and the mucous follicles were detected even with the naked eye. The epithelium forming the membrane was traced as a continuous layer to the commencement of the body of the uterus, where it encountered the flocculent remains of the decidua, and seemed to terminate."

This description is fully borne out by our own observations. We may, however, observe, that in the examination of aborted ova we have not seldom seen muscular fibre-cells adhering to the outer surface of the decidua. In abortion, the process of disruption from the uterus is generally more violent than after natural labour, and it is thus intelligible how the casting-off of the ovum may entail at spots a detachment of the entire thickness of the mucous membrane. Quoting the opinions of M. Robin, and supporting them by observations of his own, Dr. Priestley maintains that a new mucous membrane begins to be formed in the later months of pregnancy between the decidua and muscular coat. It undergoes a rapid development after the uterus is emptied of its contents, and as it is gradually perfected, it assumes the function of the original mucous coat.

Concerning the structure of the chorion the author follows generally the views of Goodsir; and with regard to the peculiar hydatiginous affection of this tissue he adopts the description of Mettenheimer, and that of Dr. Barnes given in this journal.

Coming to the structure of the developed placenta, we find Dr. Priestley's conclusions to correspond with those of Goodsir; but he describes a particular vascular development of the young placenta as a new observation. He examined a perfect ovum in the second month of gestation. The placenta was in course of formation, and all the vessels were gorged with blood. In repeated sections through the circumference of the placental portion of the ovum, he found tufts of the chorion deeply rooted in the decidua, and around each separate villus was thrown a maternal vascular loop, formed of a spacious capillary, distended with fluid blood. Loop was connected with loop in such a way that a plexus was formed, the vessels thus enclosing spaces which were occupied each by a separate villus. Fœtal capillaries were already formed in the interior of the villi themselves.

Passing on to the application of the anatomy of the placenta to the physiology and pathology of the organ, we find that Dr. Priestley still adheres to the conventional, but perfectly gratuitous, relation between the acknowledged course of the blood-circulation from uterus to placenta and back, and the cause of the flooding in cases of placental presentation. The following passages contain a summary of this theory of hæmorrhage:

"If the placenta be partially detached from the uterus, blood will still be poured into the organ by the arteries, supplying the portion not separated, and will escape from the torn sinuses of the portion detached, union with the uterine veins at this point having ceased to exist. . . . . *Generally* the more the placenta is detached, up to a certain point, the greater will be the uterine hæmorrhage. . . . . It is *pretty generally understood* that when detachment is complete, hæmorrhage, *as a rule*, ceases, the vascular supply being entirely cut off."

Hence the proposal that

"We should, *in certain perplexing and dangerous cases*, attempt to save the life of the mother by completely separating the placenta from its connexion with the uterus before the birth of the child."

It is remarkable that even those whom experience had not convinced of the erroneous clinical basis of this doctrine, should not have been struck with its logical inconsistency. No one has yet shown that the hæmorrhage in cases of placental presentation proceeds from a different source than the hæmorrhage after labour when the placenta had been separated partially or entirely from the fundus. It is unphilosophical to attribute the hæmorrhage to different sources in the two cases. The only difference is one of place—from the fundus uteri in one case, from the cervical region in the other, but in both cases from the uterus. Without denying that some blood may escape from the placental surface, we have enjoyed the clearest clinical proof that the main source—the dangerous source—is the uterine surface. But the qualifications of the doctrine, as we have italicised them in the preceding quotations, are sufficient to overthrow it. A rule so impaired by exceptions, can hardly be called a rule. If the hæmorrhage be occasionally, or "generally," stayed when the placenta is completely detached—that is because, as the observations and analysis of Dr. Barnes have proved, the uterus contracts. In præ-partum, as in post-partum, hæmorrhage, the source of blood is the same; and in both contraction of the muscular walls is the agent of arrest. Dr. Priestley has in this work shown such excellent powers of independent observation, that we trust he will apply them to the investigation of this subject, and not be content with swearing to the words of his master.

We have marked for correction at page 70, a passage in which it is represented that Dr. Barnes and Dr. Druitt maintain that the placenta undergoes a normal atheromatous change preparatory to its being cast off. "It must be remembered, however," the author says, "that the placenta, as a respiratory organ, is as active in function up to the moment of delivery, as at any previous period; and that any considerable alteration in its essential structures must act deleteriously." So reasoned Dr. Barnes in 1852, and further stated it as a matter of observation that, normally, no sensible amount of fatty degeneration of the placenta at term was found. The merit of the theory, that the normal detachment of the placenta is prepared for by fatty degeneration, belongs to Dr. Druitt.

We have now followed Dr. Priestley pretty fully in the discussion of most of the important topics examined in his book. We find in it much to commend, both in the matter and in manner of treatment. If it contain no very original contributions to the subjects treated, it is certainly marked by a praiseworthy spirit of personal investigation. If he have not suggested any essential modifications in doctrines previously ennunciated, he has at any rate verified the observations of others by his own researches. The work is illustrated by very faithful and neatly-executed wood engravings. As a compendious and methodical description of the uterus, as affected by impregnation, the work is well deserving of perusal, and may constitute a useful guide to the obstetric student.

Review IX.

1. *De l'Anesthésie Hypnotique.* ('Gazette Hebdomadaire de Médecine et de Chirurgie,' 16 et 30 Déc. 1859.)

On Hypnotic Anæsthesia.

2. *Note sur le Sommeil Nerveux, ou Hypnotisme.* Par le Dr. Azam, Professeur suppléant à l'école de Médecine de Bordeaux, &c. ('Archives Générales de Médecine,' Janvier, 1860.)

Note on Nervous Sleep, or Hypnotism. By Dr. Azam.

3. *Études sur le Somnambulisme envisagé au point de vue Pathologique.* Par le Dr. E. Mesnet, Médecin de Hôpitaux. ('Archives Générales de Médecine,' Février, 1860.)

On Somnambulism examined in reference to Pathology. By Dr. E. Mesnet.

4. *Neuro-Hypnotism, or Artificial Nervous Sleep.* By R. T. Dwight, M.D. ('Charleston Medical Journal and Review,' Sept. 1850.)

If some future nosologist should attempt to classify aberrations from mental health on the principles which obtain for bodily ailments, he will have to arrange many of them in a class of epidemics. Foremost in such a class will stand an order of Parapeithiæ, or perversions of belief, on which all the characters of their bodily prototypes will be demonstrated. Complaints of this kind are never wholly absent from a community, but break out at times with unusual severity, and attack every sex, age, and class of society. They are maladies which assuredly propagate themselves after the manner of ferments, and little alteration will be needed in the existing hypothesis of zymosis to account for their progress. Seas do not hinder them, though some lose their virulence out of their own latitudes. Their tendency is often to travel westward, their manifestations in England being preceded by outbreaks in France or Germany. The different forms assumed by these complaints at various times will be accounted for by the doctrine of "epidemic constitution," a something in the atmospheric, sidereal, or lunar arrangements determining the particular perversion which prevails. To complete the resemblance, we have improved in the treatment of these maladies as a class; jugulating measures are no longer employed, but the complaint is left to die out under a system of careful watching, and especially of wholesome ventilation.

In a conspicuous place among perversions of belief, we shall be called on to recognise a very common one, standing near the boundary between health and disease, a species of breaking-out that shows richness of blood, or rather in some sort a mental ague, endemic, apt to recur, a disease while it lasts, but often "physic for a king" in its results. It is that exaggerated belief which takes hopes for demonstrations, and may be called by our transcendental nosologist Boulopeithia, or belief in what one wishes. The amiable form of this complaint, magnifying exceptional facts into grand discoveries, which are to bless and regenerate the world, is to be met with every day among kindly people of every calling, and notably in our own profession of physic. An instance of this sort is at present before us in the recent advocacy by our French brethren of hypnotism as a substitute for chloroform in surgical operations.

It can give us no surprise that medical observers, even of ordinarily well-balanced minds, finding themselves in possession of a new experience, should claim for it an importance and scope beyond what is accorded by subsequent investigation. Such a fictitious halo almost necessarily surrounds any central truth, and such exaggerated confidence of an honest observer has only the good effect of drawing a wider attention from others to his pretensions, and thus of disseminating the essential facts on a larger scale. Thus, on our present subject of Hypnotism, an epidemic influence is at present abroad, and notice is being attracted, by the very exaggerations which prevail, to a class of phenomena which have not yet obtained sufficient recognition.

"L'ordre du jour," commences a weekly Paris contemporary, "appelle la question de l'hypnotisme ; suivons donc l'ordre du jour." Confining our observations by this rule, we do not at present propose to consider at any length the general nature, affinities, phenomena, and explanations of the condition thus designated; but only that part of them which is at present claiming the most notice in the medical world—the application of this condition to the purposes of the surgeon. We shall only recapitulate so much of the other phenomena and history of hypnotism as is needed to show the estimation in which its anæsthetic powers have been held by its earlier sponsors.

Twenty years ago, when more was heard of animal magnetism and mesmerism than now-a-days, and when a high party feeling existed between the converts and the sceptics of these occult sciences, Mr. Braid, a surgeon of Manchester, had the boldness to undertake an independent inquiry into the phenomena alleged to be produced by their influence, and to attempt a separation of the true from what was fancy or imposture. His results, first published in 1843, professed to investigate "the Rationale of Nervous Sleep," or, as he named it, "Neuro-hypnotism," or "Hypnotism." Always observing and reflecting on his favourite subject, he produced in the next few years various essays and papers on "Trance," and "Electro-Biology ;" and again, in a little book "on Hypnotism," in 1852, he recapitulated his experience, and defined and defended his position.

The phenomena actually produced by the mesmerists, odylists, and others, as contrasted with the alleged effects whose reality could not be maintained in the face of his criticism, are grouped by Mr. Braid under the term "hypnotic." He proved them to be producible quite independently of any magnetic influence or of any emanation from a second individual. He found in them no indications of any new or hidden power in nature, but only some unexpected manifestations of well-recognised physiological laws ; and he bestowed on them the name they now bear, rather by way of avoiding cavil than of expressing their nature in a definition. The "hypnotic" phenomena, indeed, may be held to comprise all those which the disciples of Mesmer, Reichenbach, and others did actually succeed in evoking, the mode of their production, however, being reduced to its simplest form. Nothing more was demanded for the state of hypnotism than the concentration of the attention, and this was assisted by the steadfast direction of the eyes upon some small meaningless object. The whole of the results were thus shown to be essentially subjective, having their origin in the mental condition of the patient. They were independent of any "operator," though previous training, sympathy, or state of expectancy, communicated by another, affected the readiness and intensity of their production. For example, out of fourteen male adults experimented on together, ten were thrown into the hypnotic sleep by directing their gaze steadily on corks bound so as to project from their own foreheads. On another occasion twenty-two persons placed in a semicircle, taking each other's hands, and bidden to watch the result, all passed without exception into the hypnotic state; the effect here being favoured, Mr. Braid shows, by the force of imitation.

What the phenomena were that resulted from these and other proceedings are by no means completely detailed, probably from their great variety in different subjects. The more usual manifestations of the nervous sleep, however, appear to divide themselves into two series, the one series, according to Mr. Braid, being normally antecedent to the other. Both series or stages are characterized by this observer—and herein he follows closely the explanation by Dr. Carpenter of the phenomena of electro-biology—as consisting in an intense mental abstraction. The one series of conditions observed in the so-called sleep, seems to point to a suspension of the functions of the reasoning powers and of the will, simple consciousness remaining. The ideas of a patient in this condition flow in one direction only, and with a force proportionate to the narrowness of their channel. The course of ideas, however, can now be varied by a second person, by appeals to the memory, imagination, or emotions, through suggestions addressed to the special senses or to the muscular sense. In this stage, the appearances result which are familiarly known under the name of electro-biology. The reality of these appearances, apart from the absurd hypothesis involved in the name, is now placed beyond a doubt by the testimony of a multitude of undeniable observers. The applicability of these conditions to the cure and alleviation of human suffering has been of course a matter of confident prediction by those whose labours have brought the subject to its present point. This question, however, though of the highest interest for the physiologist and practical physician, as well as for the speculative student of mental science, is not that which the "ordre du jour" calls on us now to investigate.

There is another series of phenomena in the application of which it is the operating surgeon who claims the greatest and most direct interest; a condition apparently opposite to the foregoing, in which intensity of consciousness and acuteness of response are replaced by an insensibility to external impressions, and this may amount, it is stated, to an anæsthesia so complete as to allow of the severest inflictions being practised without the knowledge of the sufferer. This is the form of the hypnotic sleep which is at present the subject of gossip and speculation in the *salons* of the Paris hospitals. It is our present object to examine into the phenomena of this condition, and to inquire into the extent to which it can be made practically useful.

But in attempting to ascertain the simplest facts as to the existence, appearances, and frequency of hypnotic anæsthesia, we meet with the ordinary difficulty of defining actual truths amid the glare that has been thrown around them by excitement and enthusiasm. When we read that in the practice of Dr. Esdaile in India, a few years ago, "three hundred persons laid under the mesmeric sleep were subjected to various capital operations, many of these being of the most terrible description," yet that pain was in all cases wholly avoided by this means, it is impossible to accord full credit to the statement. Either European science must have fallen far behind that of Asia, or else those who travel across the sea must after all change their minds, and their bodies too, that such results should be the rule in India and be unknown to the great mass of surgical practitioners at home. It is hard to say that Dr. Esdaile's testimony is not to be believed on a matter which appears so simple, as whether three hundred persons subjected to torture felt, or did not feel, the injuries inflicted; but we must confess that his evidence carries materially less conviction when we find he goes on to record "that the mesmeric virtue can be communicated to inanimate matter is a physical fact of which I am as well convinced as of my own existence. It was my common hospital practice to entrance patients for the purpose of having their sores burnt with nitric acid, by giving them mesmerized water to drink." Add to this, that this writer believes in "community of taste" and "thought-reading," and sober minds in England, denying Dr. Esdaile's powers of philosophical observation, will be compelled to seek for other evidence than his of the power and value of mesmeric anæsthesia. The most ardent hypnotist of England, whose views have been already quoted, recounts Dr. Esdaile's alleged experiences with every respect for their narrator, and with an implicit credence in their reality, but goes on to say:

"Most sincerely do I wish that the like processes could be rendered equally available in this country as they were in India, for annulling the pain incident to surgical operations. I am well aware from my own experience, as also from what I have read as the results with others, that mesmerism, or hypnotism, may be successful for this end with *some* patients, but I fear it will not become so generally successful in this country as in India, or as chloroform has been proved to be in Europe. Mesmeric anæsthesia, however, has this great superiority when it succeeds—that no injury has ever resulted to the patient from its use; for the recoveries under mesmeric surgical operations in Dr. Esdaile's hospital were far beyond the average success. Why, therefore, should it not be fairly tried in this country as well as in India, when no danger to the patient can result from such trials? My own convictions, from trials of both methods, are in favour of chloroform, as being *more speedy and certain in its effects with patients in this country*, and I know that, with due care and caution, it may generally be used with safety in surgical operations as well as in midwifery; but I should think it interesting, in every point of view, that the mesmeric or hypnotic modes should be fairly tested, so that we may know how far Dr. Esdaile's method can be made available in this country, with British subjects on British soil, for suspending the anguish of mind and body, during surgical operations, by such a safe and simple process."

The highly sanguine tone of this extract increases the force of Mr. Braid's evidence against a universal susceptibility to the hypnotic anæsthesia In 1852, he "fears it will not become generally successful in England," but "he would think it interesting that the mode should be fairly tested." Were he interrogated in 1860, he would probably own with regret that his fears had been too true, and that he had also been cured of the too absolute belief which his over-hopefulness had led him to grant to Dr. Esdaile's Indian marvels.

In truth, well-authenticated cases of actual operations performed in the mesmeric or hypnotic condition are by no means common; they are not to be numbered by hundreds, but by units. It is plain that records of such cases can furnish the only valid proof of the anæsthetic value of the process. The production of minor degrees of the hypnotic state would not justify us in the conclusion that a surgical operation could be borne without pain. We have already noticed the almost uniform "hypnotism" of the patients subjected to the procedures of Mr. Braid, but they were very far from losing their sensibility with equal uniformity. In many no effect was produced beyond an inability to open the eyes, others professed themselves "insensible to pain from the prick of a pin." But the anæsthesia which ignores the prick of a pin is perhaps not only different in degree, but even in kind from that required by the surgeon. Professor Nélaton, in trying the hypnotic method the other day, succeeded in ten minutes in producing a condition such that the two arms raised one after the other remained fixed in the horizontal position. The operation was just about to begin, when, in order further to test the state of the patient, the surgeon asked, "Can you put your arms down?" Instantly letting them fall, "To be sure I can," said the patient, "but I thought you wanted them held up for the operation." And much in the same way many a patient, if he did not merely regard the pinch or prick as a challenge to his stoicism, would be likely enough to confide in his operator so far to think it a part of the business, and would probably say in all sincerity that the pain he experienced was less than what he would have expected. But passing over this doubtful ground, there are other instances where persons have passed into a state designated "cataleptic" by the procedures of hypnotism, and have in that state been subjected to burns and other really severe injuries. Cases of this sort, however, are distinctly fewer in number, and those instances of a further stage of insensibility where the automatic functions became torpid are notably rarer still. Such torpidity accompanying a peculiar cataleptic state was witnessed in a patient of Dr. Azam's, whose case is related in the January number of the 'Archives Générales de Médecine.' Here ammonia to the nose and tickling the nostrils and the soles of the feet produced no sign whatever. Such an insensibility as this, obvious sources of fallacy being duly avoided, appears to furnish decisive proof of true anæsthesia, and would be the most available evidence of a condition which would bear a surgical operation. These cases, however, produced by any modification of the hypnotic or mesmeric method, and recorded in a way to carry conviction of their reality, are not to be met with in any large number, and we are thrown back after all to what constitutes the only proof of the applicability of the hypnotic sleep to operative purposes, an examination of the actual cases of its successful employment with this object; and these, in the practice of credible observers, dwindle down, as has been said, to a very inconsiderable number.

In passing over the old standard cases recorded by Cloquet, Topham, Toswill, Elliotson, and others, in which painless operations were performed in a variety of strange mental states induced by the mesmeric procedure, and in again observing the evidence of their exceptional nature furnished by the still small number of such cases, we would refer those of our readers who are unacquainted with those *cas remarquables* to an article in a former volume* of one of our predecessors. We prefer now to illustrate and discuss this application of hypnotism on the basis of the recent experience of the French capital, recounting as a starting-point the history of the operation which has brought the subject newly into prominence, and which is one of the most carefully narrated cases, as well as one almost typical in its phenomena. The following narrative is condensed from the report of M. Broca read to the Paris Surgical Society:—

"A woman, twenty-four years of age, had been admitted into the Necker Hospital at Bordeaux, on account of a large burn on the back, and the right arm and leg, besides which she was affected with a very large and painful abscess situated at the margin of the anus. An incision into this abscess was imperatively required, and this operation the patient, a very timid person, had a great dread of. She was told, however, that she would be put to sleep before the operation. A simple reflector of plated metal was placed six inches in front of the root of the nose, at the extreme limit of distinct vision, in such a manner that the patient was obliged to squint strongly inwards to keep her eyes on the object. Immediately her pupils contracted; her pulse, which was before somewhat rapid, at first rose a few beats, but almost immediately after became more feeble and much less frequent. Two minutes later, the pupils began to dilate. The left arm was then raised from the bed, and was found to remain immovable in its new position. About the fourth minute, questions were answered slowly and with difficulty, but yet with perfect good sense. Respiration was very slightly enfeebled. At the end of five minutes the left arm, remaining in its elevated position, was pricked by M. Follin without producing any movement. A second and deeper prick, bringing a drop of blood, was equally unperceived. On raising the right arm it remained in the same fixed attitude as the left. The bedclothes were now raised, the lower limbs separated, and the abscess brought into view; the patient submitting, but remarking the while, in a quiet way, that no doubt they were going to hurt her now. At last, seven minutes after the experiment began, M. Broca continuing to hold the shining object before the eyes, M. Follin made a large

* British and Foreign Medical Review, vol. xix. p. 428.

incision into the abscess, and gave exit to an enormous quantity of fœtid pus. A slight cry, lasting less than a second, was the only sign of consciousness given by the patient. There was not the least quivering of the muscles of the face, nor of those of the extremities; the two arms remained without any deviation whatever from the position they had now kept for several minutes.

"Two minutes later, the same attitude persisting, the eyes were wide open and a little injected, the features fixed like a mask, the pulse exactly as before the experiment, the breathing perfectly easy; but the woman continued insensible. The left heel, raised from the bed, remained suspended, the two upper limbs being steadily in their former position. Yet in the morning, the patient, tormented by her burns, had not been able to move in her bed except with much pain and difficulty.

"The bright object being now removed from before the eyes, the state of insensibility and cataleptic rigidity still persisted. Friction and a breath of cold air being used to the eyelids, the patient made some little movement; she was asked if she felt anything, and answered that she knew nothing about it. For the rest, her three limbs remain always suspended and fixed in the positions that had been given to them, the left arm having now been rigid for the space of full thirteen minutes. A puncture which brought a drop of blood was made into this arm, but no sign of sensibility was given, and the fingers preserved their complete immobility.

"At last, eighteen or twenty minutes from the beginning of the experiment, and more than twelve minutes after the operation, M. Broca again employed friction over the eyes, smarter than before, and blew over the face a large quantity of cold air. This time the patient woke up almost suddenly. Her two arms and her left leg became relaxed together, and fell instantly upon the bed. She then rubbed her eyes, and recovered consciousness absolutely. She remembered nothing, and was astonished to hear that the operation was over. Her state was to be compared, up to a certain point, with that of persons awaking from a sleep produced by ordinary anæsthetics, with the difference, however, that her waking was much more sudden, and was without excitement or loquacity.

"After a few moments, she complained of a little suffering in the wound, but the pain was very trifling. The whole period of insensibility lasted at the least twelve minutes or a quarter of an hour."

The notoriety which has attended this case of M. Broca's has led all Paris to set spoons before the eyes of the sick, and multitudes of results are being announced. Many persons are put to sleep, dogs, cats, and chickens, as well as men and women. There are pricks on many Parisian skins, and bruises on many Parisian arms. M. Velpeau is ostentatiously announced as a convert to the "reality of hypnotism." But when we come to inquire into the practical issue to which all this enthusiastic experimenting acknowledgedly tends—the application to surgery—we find M. Broca's operation standing almost alone. A most persevering French surgeon — M. Richet, of Saint-Louis — failing utterly to produce any anæsthetic effect upon three patients in whom operations were needed, had recourse to the assistance of the most eager hypnotist of France, M. Azam. Two young females were chosen, the one with an anchylosed elbow, the other with a tumour at the wrist. In each case the patients performed most docilely the requisite act of gazing upwards at a polished spatula placed six inches in front of the forehead, the one girl for ten, the other for eleven minutes, but in neither case was the slightest symptom of insensibility evoked. In all, six cases for operation were tested by this surgeon without the slightest result. In the hands of Prof. Denonvilliers no better success has attended the method: a few nervous symptoms were indeed provoked, but for the purposes of the surgeon, not the slightest effect. Again, M. Forget having chosen a very nervous, excitable subject—a young man of eighteen years old—practised on him the usual experiment for *twenty-two* minutes, producing, as we may readily believe, contraction and dilatation of the pupils, twitching of the eyelids, and a few other trifling nervous symptoms, but of anæsthesia absolutely nothing.

In short, with a view to immediate operative procedure, we have sixteen instances of the faithful employment of the hypnotic method, in not one of which was the slightest result obtained. Another case is indeed put forth as a successful example of complete unconsciousness to pain "from hypnotism." Here Dr. Guerineau, of Poitiers, amputated the thigh of a man who had suffered for two years from a white swelling of the knee, and had tubercles also in his lungs. The only record we have seen of this case, professing to come from the pen of the operator, is so wonderfully imperfect, that beyond the fact of insensibility we are perhaps not justified in drawing any conclusion as to the state of the patient, but such as it is, the narrative leaves on one's mind a conviction that the patient was in a state of syncope at the moment of the operation, and that there was no direct affection whatever of the nervous system.

These being the facts concerning the success and failure of the method as an anæsthetic power, distinguished from the epidemic belief in "Hypnotism" at present prevalent in French medical circles, let us now examine the received explanation of the nature of this condition, and try to obtain therefrom some assistance in forming a judgment as to its practical utility. Let us see what probability there is in the statement, confidently made by the devotees of hypnotism who acknowledge how often it disappoints the operating surgeon, that by modifications in the present procedures or otherwise, a larger number of persons will be ultimately rendered susceptible to its anæsthetic influence. An explanation of the nature of the hypnotic anæsthesia—assigning to this condition its right place among other similar conditions already understood and classified—is little more than an application of the views which are now pretty generally received as to the nature of the hypnotic state as a whole. Still a recapitulation of these views with special regard to the phenomena of insensibility will be found to lead directly to an estimate of the practical worth of hypnotism as an anæsthetic.

As opposed to ordinary insensibility from syncope, coma, or chloroform, where the whole of the mental faculties are for a while dormant, there exist a well-known series of conditions, in which objective evidence of pain is lost, and in which its perception is largely impaired, but in which, nevertheless, other mental faculties remain more or less active. Somnambulism, catalepsy, hysteria, and ecstasy, in their spontaneous manifestations, may be regarded as consisting of certain mental and bodily conditions resulting from a disturbed relation between the will, the emotions, and simple idea; the common feature of these states being an intense absorption of the faculties, apparently consequent on the possession of the mind by some single idea or train of ideas. These disorders run into one another by indefinable transitions, but in their most typical forms they may perhaps be distinguished as much by differences in the accompanying mental state as by varieties in their bodily manifestations. Thus the condition of spontaneous somnambulism differs from that of hysteria or ecstasy in being less dependent on the emotional state associated in these latter disorders with the absorbing idea. The somnambulistic state would seem to consist in an involuntary flow of simple ideas, the one suggested by the preceding or by some external impression, and giving rise directly to appropriate muscular actions without the intervention of the will or of the emotions. Close to this comes the condition of spontaneous catalepsy, in which the idea and its muscular manifestation are single and immovable, and in the production of which some emotional state usually plays a considerable part. Then follow the states of ecstasy and hysteria, having a still more uniform connexion with the feelings, but in their development being also under some influence (at least for control) of the will.—In the same series we recognise the hypnotic state, distinguished from the rest almost solely by the mode of its induction and by the possibility thence arising of its phenomena becoming available for therapeutical purposes. It differs from the other conditions enumerated by its dependence at the outset upon an operation of the will, dwelling deliberately and of purpose on a single idea, and encouraging the spread of its fascination over every faculty. It is not very plain whether this result is obtained immediately by the simple concentration of the will, which then becomes fixed at the point of regard, and cannot be extricated by any action of its own; or whether it comes indirectly through the supervention of some emotional state, which is able to bind and overmaster the will. From the facility with which the hypnotic state is developed in persons of susceptible feelings, and in a condition of body favourable to emotional exaggeration, it might be presumed that the state of abstraction voluntarily induced served only to prepare for the onset of an emotional condition like that under which the hysterical fit is evolved; but on the other hand, the frequent induction of this condition in individuals of the opposite temperament and of robust bodies, renders it probable that the requisite concentration and abstraction are producible by a direct act of volition.

If it be agreed that the hypnotic condition is thus related in its symptoms to somnambulism, hysteria, and the rest of these very inconstant conditions, and differs from them in little else than the degree to which the will and the emotions are concerned in its production, we should be led *prima facie* to fear that its development, like its symptoms, could never be sufficiently certain and constant to allow of its general employment as a therapeutic agent. But supposing it to be producible at pleasure, in a large proportion of cases, we should further be led to expect that patients preparing for surgical operations would be the most unlikely of all persons to be susceptible of its influence. For the essential condition demanded for the hypnotic sleep is a concentration of thought upon some one object, to the exclusion of all other ideas. Now, in the presence of disease, and in the face of an impending operation, the matter uppermost in the patient's brain can scarcely fail to be his present suffering and the expectation of acuter pain. Concentration of thought on these topics would be of little avail in lessening his pain. We can indeed conceive that by the supervention of an intense emotional excitement so profound as to dissociate itself from its cause, there would be exceptional cases where intense dread of suffering might bring on a state of unconsciousness, but that this should be voluntarily induced by any procedure of the patient would seem to the highest degree improbable. In employing his will to produce abstraction, our aim would be to take his consciousness as far as possible from the operation and himself, and to fix it vividly on some other subject. But supposing we have done so, and that the earlier phenomena of hypnotism are elicited, the success of the sleep for the operating surgeon demands that it shall not be disturbed by the arrival of any new impression. Now, if the somnambulist be abruptly addressed, his trance is usually broken; if some unexpected or distasteful proceeding is used to a person in an hysterical fit, she is probably aroused from it; and in the hypnotic state itself there are few phases in which clapping of the hands or fanning of the face will not suffice to break the sway of the dominant idea, and to restore the ordinary waking state. How little, then, does analogy give us any right to expect that the state of hypnotic anæsthesia should remain unbroken, when any wandering idea, or the recurrence of any timidity, would at once bring the patient face to face with his disease and the operation!—But even the actual passage of the knife through his tissues must not recal the patient's sensibility! That any voluntarily induced condition should be capable of resisting this test, we can only conceive to result from the superinduction of some other state wholly beyond the control of the will; and it must appear extremely improbable that such a total abeyance of volition should be a sequel to any sort of proceeding in which the will is concerned. Yet the distinguishing mark of the hypnotic sleep, and all that gives it a chance of usefulness for the surgeon, consists in this very production of it at pleasure, by a process which involves certain voluntary acts of the patient.

These are the considerations, derived from what we hold to be the true interpretation of the hypnotic sleep, which we would place by the side of the experience of the Paris hospitals, in estimating the probability of the dominion of chloroform being shaken by the pretensions of hypnotism. Those who hold other ideas as to the nature of the hypnotic phenomena may still find in the views we have put forward, some reason to modify a too ardent hope of the anæsthetic value of the method. For the rest, the negative character of the facts recorded by very competent observers demand from all a present scepticism. M. Azam, who has been before referred to as an earnest student of the system, and who regards the cerebral symptoms as being directly connected in some way with the convergence upwards of the optic axes, must adduce many more successful applications of hypnotism to the purposes of the surgeon, before he justifies to others his belief "that some day we shall achieve the discovery of a ready and easy means of operating thus at our pleasure upon every one alike."

At the same time we do not doubt that other instances of the surgical use of hypnotism will be put on record, and these may possibly be of increasing number as the predisposition to an hysterical condition grows in the community by force of example and faith. But soon will come a time when the epidemic influence will fade, and the sporadic cases of belief in the method as an anæsthetic will be again for a while confined to the "seekers after many inventions," and these will be found to furnish the requisite mental condition for an attack as numerously and as readily under the sky of London as of Paris.

REVIEW X.

Eight Cases of Ovariotomy, with Remarks on the Means of Diminishing the Mortality after this Operation. By T. SPENCER WELLS, F.R.C.S., Lecturer on Surgery at the Grosvenor-place School of Medicine, Surgeon to the Samaritan Hospital, &c. &c.—*Dublin*, 1859. pp. 42.

OF the eight cases of ovariotomy reported by Mr. Spencer Wells in the body of this pamphlet, only two proved fatal; this remarkable success is somewhat diminished by the addition of two other fatal cases, which have occurred more recently, and to which the author adverts in a prefatory notice; but with the two successful cases which have also fallen to Mr. Wells' lot since the publication of his paper, the general results are still sufficiently satisfactory to arrest attention and to force upon surgeons the duty of reconsidering the whole question of ovariotomy. Mr. Wells, we think very reasonably, draws a parallel between the operation for lithotomy and ovariotomy. In both there is a disease causing great pains or discomfort, which may continue for a greater or less period, without inducing death; but which must inevitably prove fatal after protracted misery and suffering. The removal of the stone or the tumour may hasten death, or it may establish a perfect cure; and as the mortality after ovariotomy appears, even under the most unfavourable view of the opponents of the operation, not to be very much greater than after lithotomy in the adult, Mr. Spencer Wells argues that we are not justified in discarding the former. On the other hand, he quotes the results obtained by Dr. Clay, of Manchester, who out of 87 cases has only lost 27, or about 1 in 3; and he also points to his own operations in proof of the fact that the mortality is capable of a much further reduction.

The following are the principal features of the eight cases now before us—

1. A single woman, aged twenty-nine, had an ovarian tumour of four years' growth, for which she had been tapped seven times; her health was failing when Mr. Wells determined upon operating. A three-inch incision was made below the umbilicus; old and firm adhesions had to be broken down by the hand; the peduncle, which was on the left side, and of the breadth of three fingers, was transfixed, and tied in three portions by whipcord ligature. The peduncle was too

short to be fastened in the wound, but the ligature was passed through the lower part of the wound; the edges of the wound were brought accurately together by superficial and deep silk sutures, and the abdomen was supported by a broad flannel bandage. The ligature, with a large slough attached, came away on the twelfth day, from which time the patient gradually improved. The recovery was complete, and the patient has since emigrated to Australia. We should add, that the cyst, with its contents, weighed twenty-six pounds.

2. A married woman, aged thirty-eight, had an ovarian tumour, probably of twenty-eight months' growth; she had been thrice tapped before Mr. Wells removed the tumour. Owing to the extensive adhesions, a larger incision was necessary than in the first case; the peduncle was fixed between the edges of the wound, and the wound carefully closed with harelip pins. The peduncle had completely sloughed through on the ninth day, except at one spot, which was secured by a fresh ligature. From the time at which this ligature came away, the patient rapidly recovered. The weight of the cyst, with its contents, was thirty pounds fifteen ounces. This patient, from later accounts, proved to be perfectly cured.

3. A married woman, aged thirty-three, dated the formation of the ovarian tumour from her last pregnancy, three years before Mr. Wells saw her; she was then fifty-seven inches in girth at the umbilicus, owing to a complication of ascites with the ovarian disease. After drawing off fifty-seven pints of ascitic serum, Mr. Wells removed the ovarian tumour, which was irregularly lobulated. Alarming hæmorrhage occurred in this case after the peduncle was divided, though secured between the blades of a metal clamp. The bleeding proved to come from a large vein running along the brim of the pelvis, which had been torn; it was taken up by a toothed forceps, and a lateral ligature applied. The case progressed favourably, without a bad symptom; the clamp came away on the 8th, the ligature on the vein on the ninth day. The patient left, cured, four weeks after the operation.

4. A single woman, aged thirty-nine, had an ovarian multilocular tumour of nearly two years' growth; the removal was effected with great facility as there were no adhesions; the operation was performed as in the previous cases, and things went on well at first, but death took place suddenly at the end of thirty-nine hours. There was evidence of recent peritonitis, and the peritoneal cavity contained a considerable quantity of very acrimonious fluid, which the author regards as having been of a poisonous character and one of the causes of death.

5. A married woman, aged forty-three, with an ovarian tumour of four years' growth, and suffering also from ascites, was operated upon; forty-nine pounds ten ounces of the ascitic fluid having been removed a fortnight previously. Only one or two slight omental adhesions had to be separated, and the tumour was readily extirpated, the pedicle, which consisted of the right Fallopian tube, the broad and round ligaments, being fixed externally by the clamp. The stump of the pedicle separated on the ninth day, and in twenty-seven days after the operation the patient returned to her home.

6. A married woman, aged twenty-nine, with an ovarian tumour of

ten months' growth, solid, and freely moveable, and also affected with ascites, was operated upon on June 17th, 1859. Numerous adhesions had to be broken down, the tumour was so closely attached to the uterus that it was with difficulty that a clamp could be applied; unfavourable symptoms set in, and death ensued forty hours after the operation, from peritonitis and hydrothorax.

7. A lady's maid, single, aged twenty-nine, with an ovarian tumour of eighteen months' growth, after being twice tapped, was operated upon by Mr. Wells, as in the previous cases, the peduncle being secured externally by a clamp; "a fortnight after the operation, she was sitting up in bed, eating and sleeping well, almost free from pain, pulse eighty, and the wound quite healed, except at the little spot where the peduncle had passed; here there was still a little fœtid, purulent discharge." She left the hospital in perfect health, four weeks after the operation.

8. A married woman, aged forty-seven, with an ovarian tumour of nearly two years' growth, after repeated tapping, was operated upon by Mr. Wells in the usual way; extensive adhesions had to be broken down, as the cyst was attached to the omentum, small intestines, broad ligament, sigmoid flexure of the colon and Fallopian tubes. After securing the pedicle by a clamp, and cutting away the cyst, there was considerable hæmorrhage, and ten vessels had to be tied. The tumour consisted of one large cyst, with numerous small ones, and semi-solid, pseudo-colloid substance in its walls. The clamp was removed on the fifth day, and the patient did perfectly well.

In the remarks which the author appends to his interesting and instructive cases, he dwells upon the importance of establishing the diagnosis before proceeding to operate, a point that can scarcely be sufficiently urged, when it is remembered that the operation has been often commenced where pregnancy, hepatic, peritoneal, or uterine disease simulated ovarian dropsy, or where there was no tumour at all. Nor should organic disease of other parts of the body be overlooked, as it should counter-indicate the operation. Mr. Wells next discusses the period at which the operation should be performed; he does not consider the question settled, but points out that in all his successful cases the disease had advanced very far. After alluding to the importance of attending to the warmth of the patient, and of maintaining a high and equable temperature in the room in which she is operated upon, he insists upon the value of anæsthetics in relieving the patient's apprehension, and in preventing the shock caused at the operation. Numerous valuable suggestions are made in regard to the proceedings to be adopted during the operation. When the cyst contains fluid a large trocar is recommended in order to save time, and the removal is facilitated by attaching an elastic tube to the instrument, so as to convey it away without wetting the patient. For this purpose Mr. Wells employs a trocar with an opening at the lower part of the canula, to which the tube may be fastened.

The advantage of securing the pedicle, external to the wound, by a clamp, has been rendered apparent by the cases; in the first two only were ligatures alone employed; in two, ligatures became necessary in addition to the clamp, as a portion of the peduncle had slipped from its grasp. Mr. Wells is of opinion that the écraseur may supersede both the ligature and the clamp. But when, with all the care of scientific forethought, and with the skill and dexterity of an experienced manipulator, the operation itself, one of the most frightful in surgery, has been performed, then the tact of the practitioner is called into play in regulating the after-treatment, a matter of no less importance than the operation itself. The following are some of Mr. Wells' very judicious remarks on this important point:

"The great objects appear to me to be to keep the patient perfectly quiet, free from pain, and thoroughly clean; the surgeon should not be led by the fact of the pulse being very rapid to fear peritonitis, and adopt active measures. It has seemed to me that the rapidity of the pulse is owing, in a great measure, to the return of the heart to its normal situation; but whether this be so or not, it is certainly no ground for active measures. With the exception of opium, given with the precautions I have already indicated, I am disposed to attach far more importance to hygienic measures than to medicinal treatment. A well-ventilated room, warm, but not unpleasantly so to the patient, the air comfortably moist, and perfect quiet, seem to me to be the chief requisites. With the object of maintaining rest, the patient should not get up to pass water, but the catheter should be used every six hours. One good effect of the opium is to keep the bowels quiet, but if there be much flatulence I do not think it is well to keep up the state of constipation too long. I have never seen any good done by O'Beirne's tube in relieving flatulence. A little soda in peppermint water, or chloric ether with aromatic confection, seems to give as much comfort as anything; but this flatulence is always a troublesome symptom for some days. The intestines seem to miss their accustomed support or pressure. The application of warmth and moisture to the abdomen, by means of a warm linseed-meal poultice, is particularly grateful to the patient, and I feel disposed to regard it as of considerable importance in the after-treatment. As to food, I have not been disposed to urge it until there was a decided appetite, but to leave beef-tea, arrow-root, with or without wine or brandy, rice-water, and barley-water, or a little tea and dry toast, with the nurse, to be given very much as the patient wished for them, and, after a few days, some solid food. The fœtid stump I have kept covered by a muslin bag of peat charcoal, changed once or twice a day, in order that the patient might not be nauseated by the fœtid odour which would otherwise proceed from it. I have not disturbed the wound until union was perfect; and if gilded pins or metallic sutures be used, they may be left with safety for many days; though I believe the best plan is to remove the pins on the third or fourth day, leaving the superficial sutures three or four days longer. So soon as the patient is able to get about pretty well, country air is the best restorative."

We have said enough of this contribution to surgery by Mr. Spencer Wells, to show that it evidences a mind capable of appreciating all the difficulties that beset the surgeon who wishes to deal with ovarian tumours by radical operations, but capable also of devising for the cunning hand the suitable means of conquering those difficulties. We cannot but urge upon the profession to take the whole question into their serious consideration; those who are disposed to investigate the subject and to perform the operation, cannot do better than take Mr. Spencer Wells for their guide, because his practice has amply justified his theory.

PART SECOND.

Bibliographical Record.

Art. I.—*A Manual of Operative Surgery on the Dead Body.* By Thomas Smith, F.R.C.S., Demonstrator of Anatomy and Operative Surgery at St. Bartholomew's Hospital, and Surgeon to the Great Northern Hospital. With Illustrations.—*London*, 1859.

Mr. Smith's object in this publication is thus expressed in his preface:

"The design of the present manual is to give to students a practical guide to the performance of operations on the dead body, and to lighten the labours of teachers, by enabling them to dispense with much oral instruction; and to substitute the same kind of supervision that is ordinarily exercised in the study of practical anatomy."

This object, we think, has been attained by a very clear and intelligible description of the series of operations usually performed in the dissecting room—a series, it must be remembered, which differs in several of its particulars from that which forms the usual routine of the operating surgeon. Thus, the student's knowledge of anatomy and manual dexterity may be usefully tested by demonstrating the ligature of the lingual or peronæal artery; and accordingly we find these operations prescribed in Mr. Smith's course; while some other operations—such as that for strangulated hernia—although most common and most important in practice, can hardly be illustrated on the subject, unless in exceptional cases, and are therefore not noticed in this treatise.

It is very difficult, however, when thus putting in some things practically useless, and excluding others of the greatest utility, to satisfy everybody; and accordingly we confess that some things strike us as being unnecessarily passed over, while other operations, although not essential, could be named which have as good right to a place as several of those which Mr. Smith has admitted.

If Mr. Smith's book comes (as its merits fairly entitle it) to future editions, we hope to see lithotrity described in it—an operation which has the better title to a place, since students in general have few opportunities of becoming familiar with the use and manipulation of the lithotrite, and still less with the method of readily seizing the stone, on which so much of the success of the expert lithotritist depends; while the operation can be practised without defacing the body, and the fragments can be used to practise the student in the succeeding operation of lithotomy. Again, we can see no good reason why the very common operations for phymosis and circumcision are not demonstrated; nor, if the ligature of the peronæal artery is described, why that of the gluteal and some others of the same class should be omitted. Perhaps these were left out because the requisite incisions would stand in the way of more important operations; but as Mr. Smith has adopted the very useful method of giving the student at the commencement a table of the operations which it is most important for him to practise when he has only a single subject available, this need not have been considered. On the exclusion of all the operations affecting the globe of the eye, except its excision, opinions may differ; at any rate, Mr. Smith's evident wish to produce a good and useful book, free from all matter which is not absolutely necessary, is so honourable to him as almost to disarm criticism. It is something, in this age of book-making, to find a book of which the principal fault is, that it is not quite long enough.

The same may be said as to the illustrations. They are few in number, but had they been more numerous, the book must of course have been much more expensive; while it may be fairly doubted whether it would have gained much in practical value. "The woodcuts," we are told in the preface, "were, with one or two exceptions, traced from photographs taken during the actual performance of the operations."

Of the sufficiency of the descriptions we have the best testimony from students into whose hands we have put the book, and who have used it in place of a demonstrator; in fact, it is hardly possible to exceed the clearness and succinctness of Mr. Smith's description. We shall quote, both as an example of Mr. Smith's style, and because the method appears to us less known than it deserves, his account of Mr. Lloyd's operation for stone.

"The body being prepared, and a staff introduced, as in ordinary lithotomy, a metallic speculum* is oiled, and introduced into the rectum; the vacant space in the instrument being turned upwards towards the pubes, it is retained in position by an assistant, who should stand on the right-hand side of the body. The operator now introduces a sharp-pointed narrow knife in the median line of the perinæum, an inch in front of the anus, and cutting downwards through the vacant space in the speculum, he divides the external sphincter, and terminates his incision in the cavity of the rectum. The speculum is now withdrawn, and the finger placed in the bowel to guide the knife, which by cautious and successive cuts must make its way into the membranous portion of the urethra, which is opened in its whole extent, the prostatic urethra remaining uninjured. Mr. Lloyd now introduces a pair of forceps,* with their blades closed, along the grooves in the staff, until they enter the bladder; disengaging them from the grooves of the staff, he now expands their blades, and passing the forefinger of the left hand between them into the bladder, he withdraws both staff and forceps, and dilates the urethral wound until of sufficient size to admit the lithotomy forceps, and allow of the extraction of the stone. . . . . We have now before us two stones from Mr. Lloyd's collection successfully extracted by this operation—the one measuring two and a half by two inches, and the other three by two inches." (p. 58.)

In conclusion, we have no hesitation in recommending Mr. Smith's book to those for whom it is intended, as a most valuable and succinct treatise on the subject of dissecting-room operations.

* These instruments are figured in the text.

ART. II.—*Essays on Medical Science.*—1. *On the Nature of Inflammation.* 2. *On the Encephalic Circulation and its Relation to the Physiology of the Brain.* By JAMES CAPPIE, M.D.—*Edinburgh,* 1859. pp. 103.

THIS little book is the production of an earnest and logical mind, and well merits, not only perusal, but careful study. The first essay, on inflammation especially, is calculated to attract attention, and we regret much that it had not reached us in time to examine it conjointly with the doctrines of Professor Virchow on Cellular Pathology,* as both authors have, by different paths, arrived at similar conclusions on some important points in pathology. We are quite unable to do justice to Dr. Cappie's views at present, and we only trust that the brief jottings we can offer our readers may induce them to examine his arguments for themselves and in detail.

Dr. Cappie makes healthy nutrition his starting-point, and finds it to consist of three series of phenomena ; first, the changes in the tissue-elements ; secondly, the circulation in the adjoining capillaries ; thirdly, the interchange of fluids between these vessels and the extra-vascular spaces. In the changes that take place in the tissues, we have to deal with the removal of effete matters, and with the supply of fresh material ; and these two processes, which are mutually subservient to each other, are accomplished by endosmosis and exosmosis between the fluid within, and the fluid external to, the capillaries. The capillary circulation itself depends not only upon the heart, but in a great measure upon the tissue changes ; the more actively these are carried on, the more active will be the capillary circulation—

"In physical science," Dr. Cappie remarks, by way of illustration, "the phenomena of capillary attraction and those of combustion afford instances of a current of fluid being produced, independently of the influence of gravitation, or any obvious mechanical arrangement. The conditions in which nutrition is carried on, may be regarded as involving a combination of the causes that operate in these two instances."

After having analysed the phenomena of nutrition, the author proceeds to examine in what way the successive symptoms of inflammation may be referred to a disturbance of the normal sequence of nutritive changes. The increased amount of blood in the capillaries, and the greater rapidity in the local circulation of an inflamed part, are the first steps of inflammation, and are attributable to excitement in the capillary circulation ; the next step is the increased production of fibrine, which Dr. Cappie regards, with Zimmermann, Bennett, and Simon, as excrementitious. The disintegration of tissue leads to the formation of fibrine, which is introduced into the circulation, and is there subjected to further reduction. The author assumes the larger quantity of fibrine found in the blood in inflammation to be due to a larger quantity being primarily formed, and not to an arrest in its removal from the blood.

* British and Foreign Medico-Chirurgical Review, Oct. 1859.

"We are led, then, to suppose, that some excitement must exist in the process by which fibrine is usually produced, and a more particular inference at once forces itself on our judgment. As inflammation is essentially a local disease—as it is constituted by changes occurring in a circumscribed portion of the system, and the increased production of fibrine, therefore, must depend on local causes—the fibrine itself must be elaborated in greater quantity at the seat of inflammatory action. It is naturally derived from the intervascular spaces; these, where inflammation exists, must furnish more than their usual amount. Its increase in the circulation will be the natural and necessary result of its increased local production."

These remarks will prepare our readers for Dr. Cappie's objection to the prevailing views on exudation. He holds, that the fibrine is the product of an excessive metamorphosis of the tissues, resulting from a modification of the nutritive process; that this is accompanied by all the evidences of greater activity, and that the increase of heat, tension, and vascular excitement of inflammation, are the necessary accompaniments or sequels of the process.

In applying his theory to the treatment of inflammation, Dr. Cappie observes, that debilitating influences disturb the balance of the circulation, and thus favour a determination of blood to the seat of inflammatory action.

"A constant antagonism is exerted between all parts of the body in relation to the circulation. If A and B are supplied with blood from one vessel, the amount which A will receive will not altogether depend upon the amount of stimulus applied to it, but also on the intensity of the attractive force operating at B, and on the facility, therefore, with which the latter will permit itself to be deprived of a portion of its supply. And so in the general system, it is not only the amount of stimulus applied to an organ or texture which determines the amount of blood it shall receive, and consequently the effect that will be produced upon nutrition: the result will greatly depend on the degree of force required to derive that fluid from other parts of the body."

The more vigorously the general process of nutrition is carried on, the greater will be the difficulty with which the blood will be drawn to a diseased part; while a languid state of the nutrition will allow the blood to be more readily determined to a part upon which morbid influences have been brought to play. We will not examine in detail the author's application of this doctrine, because we have not room for the controversy which it suggests, nor can we follow him in his discussion of the views held by various writers of high authority, in regard to the intimate nature of inflammation. We will only add, that the paper on Inflammation is interesting and suggestive; and that the same may be said of the essay on the Encephalic Circulation, though it is not of that heretical character which gives a peculiar pungency of flavour to its predecessor.

ART. III.—*Notes on Nursing. What it is and what it is not.* By FLORENCE NIGHTINGALE.—*London.* pp. 79.

THIS is a small and unpretending volume, but one nevertheless, in our opinion, destined to exercise a wide influence. The principles it inculcates have in various forms often been offered to the public, but the public has never thought it necessary to imbibe and, generally, at least, to act upon those principles. Miss Nightingale's book is a code of sanitary ethics, which comes with all the more force from her, that we know her to have had larger opportunities than any woman ever had before of testing the correctness of the advice she gives, and of the system she advocates. We might exhaust ourselves in the commonplaces of praise, without conveying to our readers our exact sense of the qualities of the little volume. But this is the less necessary, as it is certain of a large circulation, and we would specially urge the members of the profession to promote its diffusion, because they will find no better friends and assistants than among those of the laity who are duly imbued with correct hygienic principles. Let it be distinctly understood, that Miss Nightingale does not supply us with a manual of nursing such as Dr. Thomson's 'Management of the Sick Room,' or Mr. Barwell's 'Care of the Sick.' Her volume gives rather the sanitary than the sanatory directions which should guide the attendant upon the sick.

Ventilation and Warming, Health of Houses, Petty Management, Noise, Variety, Taking Food, What Food?, Bed and Bedding, Light, Cleanliness of Rooms and Walls, Personal Cleanliness, Chattering, Hopes and Advices, Observation of the Sick, are the successive headings of different sections, and will suffice to give our readers an idea of the scope of the work. That such a book had been long wanted appears evident from the avidity with which it is caught up. There is a reality and truthfulness about all the suggestions it contains, which appeal to the heart as well as to the understanding, and although we might object to one or two slight exaggerations, we most heartily wish the book God-speed, feeling, as we have already hinted, most fully assured that it is destined to produce a more permanently salutary effect in all homes where English women reign, than the whole array of excellent physiological treatises that have been compiled for their benefit.

ART. IV.—*On Chronic Alcoholic Intoxication, or Alcoholic Stimulants in Connexion with the Nervous System; with a Synoptical Table of Cases.* By W. MARCET, M.D., F.R.S., Fellow of the Royal College of Physicians, Assistant-Physician to the Westminster Hospital, &c. &c.—*London,* 1860. pp. 172.

THIS little book is not an inquiry into the general question of alcoholic poisoning, social, moral, and pathological, nor does it even embrace more than a small portion of the morbid effects traceable to the abuse of fermented beverages. It is devoted to the consideration of the assemblage of symptoms manifested, especially in the nervous system of the individuals who have been the victims of intemperance, and to which the term "chronic alcoholism" has been applied. The prominent symptoms upon which our author dwells, are want of sleep, tremors, vertigo, headache, hallucinations, such as muscæ volitantes, spectres, imaginary noises, great debility, and dyspnœa; the state of the chylopoietic and other viscera in this affection is not or but very cursorily alluded to.

The author's main object is to direct the attention of the profession to the value of oxide of zinc as a remedy in the cases in which the above-mentioned complex of morbid phenomena presents itself. He gives the remedy in doses of from two to six grains two or three times a day, and he finds that it induces improved sleep, arrest of the hallucinations and tremors, and a restoration of vigour of body and mind.

ART. V.—1. *Transactions of the Medical Society of the State of New York for the Year* 1859.—*Albany.* 8vo, pp. 454.

2. *Extracts from the Records of the Boston Society for Medical Improvement, published in the Boston Medical and Surgical Journal.* By FITCH EDWARD OLIVER, M.D., 1856–9. Vol. III. With Supplement, pp. 202.—*Boston,* 1859. 8vo, pp. 312.

MORE than one-half of the former volume is taken up with an Annual Address, and with accounts of the private business of the Society from the year 1807 down to the present time, subjects of a purely local interest, if deserving publication at all. The rest of the volume is made up of twenty-four communications by members of the Society. The more important of these are:—1. A Paper on the Prognosis in cases of Fracture of the Neck of the Femur within the Capsule, by Dr. Hamilton, of Buffalo, in which the author has collected a number of instances in which ossific union appears to have taken place. 2. An Account, by Dr. C. E. Isaacs, of a large Encephaloid Tumour originating from the under surface of the Dura Mater, and projecting from the upper and back part of the Cranium, and where an attempt at removal had been made during life with a fatal result! 3. Two cases of Ectopia Cordis, by Dr. Alden March, in both of which the infants were born with their hearts projecting through an opening between the ensiform cartilage and the lower end of the sternum proper, and lying quite naked and exposed. In both cases the heart continued to beat for about two hours after birth; but in neither were any observations made as to the phenomena of the cardiac motions. 4. An Account of an Epidemic of Diphtheritis in Albany, accompanied by great prevalence of cynanche tonsillans.

The volume of Records of the Boston Society is of a far more valuable and practical character. This, indeed, promises to be one of the most important periodical publications issued by the American Medical Press. The first part of the work is made up of brief abstracts of papers read at the Society between May, 1856, and December, 1858, having reference for the most part to interesting cases and questions in practice. Among these we may mention particularly:—A Case of Aneurism of the Ascending Aorta, opening into the Pulmonary Artery. (p. 17.) A Case of Impacted Fracture of the Neck of the Femur. (p. 29.) An Account of a

Breech of a Gun extracted from the Superior Maxilla, after a lodgment of eight years. (p. 32.) Four cases are recorded in which ovariotomy was performed, pp. 40, 44, 84, 78, and 262. In all of these the operation proved unsuccessful; as also in two other cases alluded to in a discussion upon the subject. Six cases of tubular pregnancy are recorded (pp. 103, 183); but in none is any reference made as to whether or not the ovum was enveloped in a decidual membrane. Case of adhesion of the gall-bladder to the duodenum, followed by ulceration, the passage of a gall-stone into the intestine, obstruction, and death. (p. 106.) Three cases of bronzed skin (pp. 114, 185, 186); in one of which the supra-renal capsules were diseased (tubercle); in a second they were normal; and in a third they were atrophied, although "it could not be said that they were diseased." Case of typhoid fever, with perforation of the gall-bladder, and escape of the contents into the peritoneal cavity. (p. 120.) Case of typhoid fever, with perforation of the intestine about the twelfth day, and death forty-eight hours after the perforation. (p. 308.) Here, as is very frequently the case, the perforation occurred while the symptoms of the disease were by no means severe. A collection of 104 cases of imperforate anus, with absence of the rectum. (p. 156.) Case of cancer of the stomach, followed by adhesion to the abdominal parietes, and the formation of an abscess in the latter. An account of cases which were thought to prove the inoculability of secondary syphilis. (pp. 199 and 283.) A very interesting discussion upon this subject is recorded, which merits the attention of those interested in the subject. A case of ovarian dropsy, in which tapping was performed thirty-one times between April, 1847, and December, 1858, and 2002 pounds of fluid drawn off. (p. 305.)

The Supplement is made up of twenty-five original papers, of which the more important are the following:—On Inflammation and Abscesses of the Lung caused by Closure of the Primary Bronchus, by Calvin Ellis, M.D. On the Use of Stramonium in Puerperal Convulsions, by R. H. Salter, M.D. Account of a Case in which numerous Lumbrici were ejected from the Mouth, and of another in which 365 Lumbrici were found in the Small Intestine, besides 8 or 10 in the Large Intestine, by William Morland, M.D. An Account of a Pustular Eruption on the Skin, in which the pustules contained the larvæ of a species of fly, by Dr. Mitchell. Four Cases of Croup, which recovered after Tracheotomy, by George H. Gay, M.D. On the Influence of the Placenta upon the Development of the Uterus during Pregnancy, by William Read, M.D. Two successful Cases of Excision of the Knee-joint, by Drs. Warren and Cabot.

Altogether, the volume is one which contains a great fund of useful information, and is highly creditable to the medical profession of Boston.

ART. VI.—*Stricture of the Urethra: its Complications and Effects. A Practical Treatise on the Nature and Treatment of those Affections.* By ROBERT WADE, F.R.C.S., &c. Fourth Edition, considerably enlarged.—*London*, 1860.

MR. WADE'S name has been, rightly or wrongly, so associated with one particular method of treating stricture, that our first impression on receiving this new edition of his book was to see what he had to say about *potassa fusa*. As it is only right that surgeons who differ in opinion from the majority about any special question should be protected as far as possible from having their opinions exaggerated, we will at once give what publicity we can to Mr. Wade's own statement in this respect. (p. 120.)

"It is the misfortune of all who strongly recommend any particular method of treatment, to have their views either misunderstood or misrepresented. They are all supposed, in vulgar parlance, 'to ride their hobby.' My only preference for the treatment by *potassa fusa* of the more intractable forms of stricture, is simply that I have found it generally to answer my purpose in effecting their satisfactory dilatation, besides being so entirely free from injurious effects as never in a single instance to have caused me the least anxiety for the safety of a patient. With my knowledge of the efficiency of the *potassa fusa* in some of the worst forms of stricture, I could not conscientiously resort to perineal section until the potash had failed after a proper trial, unless the case was one in which the caustic was evidently inadmissible. . . .

"I take the present opportunity of once more observing that if the *potassa fusa* is to be employed as an effective remedy for the more obstinate forms of hard gristly strictures, it must be as an escharotic."

Mr. Wade then complains of misrepresentation in "a late work on stricture," where he is described as recommending *potassa fusa* in all cases of stricture except the mildest. The author referred to is Mr. Thompson,* whose name Mr. Wade seems to have a horror of mentioning, to such an extent, indeed, that in describing the usual situation of stricture, although he avails himself of the far less extensive researches of Mr. Henry Smith, he quite ignores those of the former gentleman, although they are now allowed to be decisive on this point. It is in the same spirit that Mr. Wade attributes to Mr. Thompson "presumption" in affirming that *potassa fusa* ought not to be used as a caustic. The presumption consists in his having given this opinion while, as Mr. Wade says, "his practical experience in the effects of *potassa fusa* in stricture is evidently most limited." But it is evident that no one is likely to have extensive experience of the effects of a remedy of which he theoretically disapproves, while he is in possession of others by which he is enabled to treat the disease to his own satisfaction; and most other surgeons are in the same case as Mr. Thompson, though, of course, with less ample experience than his. We would not willingly lay ourselves open to the charge of "presumption," and are quite free to confess that our experience of Mr. Wade's method of treatment does not justify our pronouncing a decided opinion as to its safety or efficacy. We are only glad to

* See his work On Stricture, p. 226, ed. 1858.

hear from Mr. Wade so satisfactory an opinion on the first of these points: as to the second, there is no doubt that most of his cases would have been cured without any unusual method; whether equally 'tuto, cito et jucunde' is another matter. In short, the objections urged by its opponents against Mr. Wade's favourite plan may be thus put: that it is inapplicable to many of the difficult cases, and of this little notice is taken by our author; that it is impossible to limit its action, and therefore, that there is much danger of making false passages; that the destruction of tissues will lead to cicatrization and thus to worse stricture, but these two last assertions are denied by our author from the experience of forty-eight cases; and lastly, which is to our mind far the strongest objection to it, that we are in possession of more easily manageable and equally efficient methods. It would occur perhaps to Mr. Wade's opponents to observe that the great number of cases in which he has used the caustic potash almost justifies in itself Mr. Thompson's statement, that he "regards it as a remedy for all varieties of stricture except the mildest;" since it is hardly conceivable that one surgeon, however extensive his experience, could have met in his own practice with a much larger number in which operative interference was really required; and the cases which Mr. Wade gives seem to us to confirm, at any rate, most of the varieties of complicated stricture being headed "irritable," "impassable," "stricture from injury," "hæmorrhagic," "impassable with fistulæ," "irritable with constitutional disturbance on passing instruments," &c.; we cannot therefore profess ourselves quite satisfied with the candour of Mr. Wade's method of argument, nor with the fulness of his exposition of his theories. Still the fact remains, that on the authority of a surgeon of credit, forty-eight cases of the application of *potassa fusa* to the urethra have occurred in succession without any uncomfortable symptom. Mr. Wade's testimony is strong and direct upon this point: "it has never, in a single instance, caused me the least anxiety for the safety of a patient." It appears that after this distinct statement, surgeons need have no hesitation in following Mr. Wade's plan in the cases to which it would probably be almost restricted—viz., those of irritable, undilatable stricture, seated too far back to admit with safety of internal division.

On the subject of internal division, Mr. Wade frankly confesses that he has had no experience of the only mode which is much practised nowadays—viz., that of dividing the stricture from behind forwards; and he therefore supplies this part of the subject by a series of quotations from the French surgeons; nor does he appear, as far as we understand his statements, ever to have seen occasion to practise Syme's operation, notwithstanding which he is a firm, and we think judicious, opponent of the extensive use of that proceeding; although possibly in this instance Mr. Syme would be inclined to retort his own method of reasoning on him, and urge the presumption of giving a decided opinion on a method of treatment of which his experience is limited.

"Quam temere in nosmet legem sancimus iniquam!"

We are not, however, of this way of thinking; and we accept Mr.

Wade's reasonings and facts on this head, although not original, as very valuable. Indeed, the whole tone of the book, when Mr. Wade is not "riding his hobby" (we beg him to pardon the phrase, which is anything but vulgar, being drawn from one of the great classics of our language), is very judicious. There is perhaps rather too much of quotation: thus a whole chapter is imported from Simpson, and page after page quoted from Brodie and Civiale; but this is better than an affectation of originality on points which have been already settled by great authorities; and if we think that Mr. Wade is rather too zealous in his advocacy, and too liberal in his use of the caustic potash, still we cannot help admitting the great weight of the evidence which he gives in its favour; we wish that some surgeon (why not Mr. Wade himself?) whose opportunities enable him to do so would attempt fairly to compare its action with that of the remedies that might be used instead of it, in a sufficient number of parallel cases of the various more difficult forms of stricture.

Until something of this kind has been done, we can make little progress towards a judgment, nor can we unreservedly admit the claims of any peculiar method of treatment, while it has in its favour only the testimony of those who, by invention or by special predilection, have become peculiarly identified with it.

ART. VII.—*Summary of New Publications.*

IF the last quarter was less productive of medical works than usual, the present quarter has more than balanced the deficiency. Our table groans under the heavy load of books claiming larger notice than it will be in our power to bestow; though we intend to return more fully in future numbers to many of the works of which we can now indicate little more than the titles.

It is due to the memory of the eminent physician, who has recently and so suddenly been summoned from this life, first to mention the work 'On Acute Diseases,' by Dr. Todd, in which the stimulant plan of treatment, which he so largely adopted, is fairly set before the medical world. Dr. Inman, of Liverpool, offers us 'A New Theory of the Practice of Medicine;' while from Italy we receive, in a new dress, Sir John Forbes' theory, under the well-known title, 'Nature and Art in the Cure of Disease.' Strange to say, the translator makes the author, in both the introductory notices, sign himself John Jones! Again, Dr. Spurgin explains, that in his view 'The Cure of the Sick' depends not upon "homœopathy, not allopathy, but judgment." The abuse of mercury in the treatment of disease, is well and forcibly expounded by Dr. Habershon, in a little book that we strenuously recommend to the perusal and consideration of all believers in that drug. Insanity, in its various aspects, has two powerful exponents in the persons of Dr. Morel and Dr. Moreau, with whom we may also mention M. Delepierre, who offers the 'Histoire Littéraire des Fous.' We hope soon to be able to do ample justice to the works of these gentlemen. Dr. Bushnan examines the recent religious revivals, especially in their relation to nervous and mental diseases. The periodical literature of insanity also continues to bring interesting contributions on this department of medical science. Diseases of the organs of respiration receive special attention from Drs. Timms and Hogg, each of whom devotes a volume to Consumption; while Dr. Gibb's name is associated with an inquiry into the 'Diseases of the Throat, Epiglottis, and Windpipe.' The difficulties of Digestion are investigated by Dr. Leared, with whom we may suitably mention Dr. B. W. Richardson, as the author of a series of lectures 'On the Diseases of Teeth in their purely Medical Aspect.' These lectures were delivered before the members of the College of Dentists, in 1858–59. Dr. Granville has published an entertaining and instructive work on the well-known alkaline waters of Vichy, to which we shall recur in our next, together with Dr. Willemin's treatise on the same subject. Dr. Humphry, in an interesting pamphlet, gives cases and arguments to show that the coagulation frequently observed to take place in one or more veins during life, depends not upon disease of the bloodvessels, but upon a preternatural tendency in the blood to coagulate. Dr. John Struthers argues, from experimental and anatomical grounds, for the necessity of jugular venæsection in asphyxia as early as possible, the chief reason being, that by this means the right side of the heart is relieved, and an obstacle to its action removed. The treatment of paralysis by the "Movement-cure," a system of active and passive muscular exercises, is advocated by Dr. Roth; while Dr. Noble places before us certain exaggerations and misapprehensions of sanitary reformers.

Surgery is represented by several works of importance. From America we have received a large volume 'On Fractures and Dislocations,' by Dr. Hamilton, which we hope shortly to review, together with a German work 'On Fractures,' by Dr. Gurlt. Mr. Toynbee brings his minute knowledge of the pathology and therapeutics of the ear before us in the form of one of Mr. Churchill's manuals; while among the new editions and reprints, we have to mention the well-known works on the Rectum, by Mr. Ashton, and on Hæmorrhoids by Mr. Henry Smith; Mr. Spencer Wells' painfully-interesting paper 'On Cancer and Cancer Curers;' Mr. Bryant's 'Injuries and Diseases of the Nervous System,' and Mr. Pemberton's remarks 'On Excision of the Knee-joint,' and a new edition of Dr. Kidd's book 'On Anæsthetics.' We would draw special attention to the researches of Professor Giraldès, 'On Mucous Cysts in the Maxillary Sinus.'

A large work 'On Materia Medica,' by Dr. Stillé, of Philadelphia, discusses the uses of drugs in great detail, in the order of their supposed therapeutic action; our Transatlantic brethren also send us the 'Transactions of the American Medical Association for 1859,' while we are indebted to the Lady Managers of the Female Medical College of Pennsylvania, for a copy of the Introductory Lecture delivered by "Ann Preston, M.D., Professor of Physiology and Hygiene," at the opening of the tenth annual session.

Obstetric science demands the mention of a work by M. Voisin, 'On Retro-uterine Hematocele,' with Dr. Duncan's paper 'On the Development of the Female Pelvis;' and as the diseases of women and children are commonly thrown together, we may take the opportunity of introducing here to our readers Mr. Chavasse's 'Advice to a Mother on the Management of her Offspring,' a fifth edition of which is before us. The information it contains is conveyed in the form of answers to queries addressed by an intelligent mother to her medical man.

The chief contribution to Anatomy is made by Mr. Lockhart Clarke, who, with his acknowledged success, continues to investigate the minute structure of the nervous centres. Want of space alone has prevented our already presenting to our readers a summary of his results; the same remark applies to Dr. Waters' interesting researches into the anatomy of the human lung. From Germany, the first number of a new physiological work, by Professor Vierordt, has come to hand; in Physiology, we have also to mention an essay by Professor Martins, of Montpellier, entitled 'Da Froid Thermométrique et de ses Relations avec le Froid Physiologique dans les Plaines et sur les Montagnes;' as well as a work by Dr. Tüngel, entitled, 'Klinische Mittheilungen von der Medicinischen Abtheilung des Allgemeinen Krankenhauses in Hamburg.' New York sends an illustrated work 'On Pathological Micrology,' translated from the German of Gustaf von Düben, by Professor Bauer.

The New Sydenham Society continues its labours, and has just issued its fifth volume, containing three translations of German monographs of a somewhat incongruous character—viz., Kussmaul and Tenner 'On Epileptiform Convulsions,' Wagner 'On Resection of Bones and Joints,' and Graefe 'On Iridectomy.' Dr. M'Cormac favours us with aphorisms, bearing upon the social, intellectual, and moral relations of man, entitled, 'Aspirations from the Inner, the Spiritual Life, aiming to reconcile Religion, Literature, Science, Art, with Faith and Hope, and Love and Immortality.' As extremes touch, we may here advert to Dr. Wallace's paper 'On the Great Mortality of Greenock,' in which the author dwells upon the physical causes conducing to so melancholy an effect with the means of removing the former.

A review of the examinations instituted by our various medical corporations, which is unavoidably postponed till our July number, will enable us to say a few words about Mr. Dale's 'Present State of the Medical Profession in Great Britain and Ireland.' The last work with which we conclude our summary, is Dr. Husband's 'Exposition of a Method of Preserving Vaccine Lymph Fluid and Active,' in which he particularly advocates the use of the capillary tube for securing a proper supply of lymph. Though not a work in editorial terminology, it would be wrong not to say a word in praise of the Visiting-list which the great stationers of Long-acre, Messrs. Smith, have this year again prepared for the medical public, and which we, like so many of our brethren, could now scarcely do without, as it has become, from its convenient arrangement and portability, the doctor's *sine-qua-non.*

PART THIRD.

Original Communications.

Art. I.

On Contraction and Obliteration of the Aorta near the Junction of the Ductus Arteriosus. By THOMAS B. PEACOCK, M.D., F.R.C.P., Assistant-Physician to St. Thomas's Hospital, and Physician to the Hospital for Diseases of the Chest, Victoria Park.

THE occasional occurrence of great contraction or entire obliteration of the aorta at or near the point at which the ductus arteriosus is united with that vessel, has been known to pathologists since the end of the last and the earlier portion of the present century.

In 1828,* M. Reynaud, in recording a case of the kind, offered some general remarks on the nature and cause of the defect. In 1841, it was made the subject of a memoir by Dr. Craigie,† in which, in addition to the description of a case which had occurred in his own practice, he gave full reports of nine cases, nearly the whole of those which had up to that time been placed on record, and very ably illustrated the general results. Tiedemann,‡ in his work on the narrowing and closure of arteries, published in 1843, quoted, in addition to the cases collected by the former writer, two other instances of the same kind; and Rokitansky,§ in a note to his 'Pathological Anatomy,' which appeared in 1844, added the particulars of four others. Hamernjk,‖ writing in 1844, and Dr. Chevers¶ in 1845, referred to 12 cases as then recorded; Van Leeuwen** in 1850, and Lebert†† in 1852, mentioned 18; Rokitansky, in his monograph on Diseases of the Arteries,‡‡ published in the latter year, stated that the cases then known could not be less than 26 in number; and Leudet,§§ in a

* Journal Hebdomadaire de Médecine, tome i. p. 161. 1828.

† Edinburgh Medical and Surgical Journal, vol. lvi. p. 427. 1841.

‡ Von der Verengung und Schliessung der Pulsadern in Krankheiten. Heidelberg und Leipzig, 1843; critical analysis in Edinburgh Medical and Surgical Journal, vol. lxv. pp. 111, 413. 1846.

§ Handbuch der Pathologischen Anatomie, Band ii. p. 587. Wien, 1844. Sydenham Society's translation, vol. iv. p. 306. 1852.

‖ Prager Vierteljahrschrift, p. 41. 1844.

¶ London Medical Gazette, vol. xxxvi. (new series, vol. i.) p. 187. 1845.

** Nederlandsch Lancet, second series, fifth year, No. 2.

†† Archiv für pathologische Anatomie, von Virchow und Reinhardt, s. 327. Berlin, 1852.

‡‡ Ueber einige der wichtigsten Krankheiten der Arterien. Wien, 1852.

§§ Comptes Rendus et Mémoires, tome iv., deuxième série, p. 63. Paris, 1858.

memoir read at the "Société de Biologie" in 1857, referred to 30 cases. At the present time, the published cases of obstruction of the aorta at the point named amount to about 40. In the following paper I propose briefly to abstract the particulars of the different published cases, and to offer some general remarks on the nature of the defect, and the symptoms by which it is characterized during life.

1. In 1791, M. Paris* found in the body of a female, about fifty years of age, injected for dissection, the portion of the aorta beyond the origin of the left subclavian artery or at the point of insertion of the arterial ligament, so much contracted that it had only the size of a writing quill, and from the thickening of the coats, the cavity of the vessel was extremely small. The portion of the aorta above the seat of constriction was scarcely dilated, and that below resumed its ordinary capacity, and there were no changes detected in the condition of the parts immediately adjacent to explain the peculiarity. The carotid arteries were of natural size, but the subclavian, internal mammary, intercostal and epigastric arteries, and their branches, were greatly dilated and tortuous.

2. In 1814, Dr. Graham† related the particulars of the case of Henry Freer, aged fourteen, who died in the Royal Infirmary of Glasgow, after an illness of four months and a half, with symptoms of disease of the heart or large vessels. On examination after death it was found that the aorta, after giving off the vessels to the head and upper extremities, became contracted and diminished, till, after its union with the canalis arteriosus, it became completely impervious. The obliteration was about a line in length; a probe could be passed along the canalis arteriosus to the obstructed part of the aorta, but from its thickened condition it did not seem probable that much communication had been allowed through that vessel. The aorta was expanded at its origin but its coats were not diseased, except that about half an inch below the obliteration, there was an elevation about the size of a split-pea. The arteries given off at the arch and their branches were dilated, and the left ventricle of the heart was hypertrophied. Of this case an account was published by H. Rainey, a gentleman present at the examination, in the Journal of Corvisart, which corresponds with the report of Dr. Graham, except in stating that the ductus arteriosus was impervious.

3. Shortly after the publication of the last case, Sir Astley Cooper,‡ in the surgical essays published conjointly with Mr. Travers, referred to the case of a gentleman, a patient of Mr. Winstone's, fifty-seven years of age, who was healthy, except that he suffered from chronic asthma, when he was suddenly seized during the night with dyspnœa, pain at the sternum, and faintness, and died in a few hours. On examination, blood was found in the pericardium, and was traced to a rupture of the right ventricle involving one of the coronary veins; and the aorta at the part where the arterial duct is inserted was found so

* Journal de Chirurgerie, par M. Desault, tome ii. p. 107. Paris, 1791.
† Medico-Chirurgical Transactions, vol. v. p. 287. 1814.
‡ Surgical Essays, by A. Cooper and B. Travers, part i. p. 103. 1818.

contracted from the thickening and ossification of its coats, that it with difficulty admitted the little finger.

4. In 1820, Dr. A. W. Otto* reported the case of a girl, seventeen years of age, and regarded as healthy, except that she occasionally experienced uneasiness in the chest, who was seized during the night, after exposure to the cold, with violent pain in the region of the heart, followed by loss of intelligence and excitement of pulse, and died suddenly the following morning. On dissection, the pericardium was found to contain blood, and this proved to have proceeded from a dissecting aneurism which derived its origin from a rupture of the internal coats of the aorta near the origin of the left subclavian artery. It was further ascertained that the aorta gradually became narrowed till it reached the point of junction with the closed ductus arteriosus, where it was so small as barely to admit the passage of a quill. Beyond that point it expanded to its proper size.

5. In 1827, Albert Meckel† published an account of a miller's labourer, aged thirty-five, who, when conveying a heavy sack, suddenly felt pain, and became incapable of moving. He partially recovered in about an hour and had left his bed on the sixth day, when he suddenly fell lifeless. The post-mortem examination was performed by M. Herman, and the pericardium was found distended with blood, which proceeded from a rent in the right auricle. The body was injected, and it was found that while the lower vessels were filled, the aorta at the seat of the duct was reduced to the size of a straw.

6. In the following year (1828), M. Reynaud‡ described a similar condition which he had met with in the body of a shoemaker, ninety-two years of age, who had been paralytic, and died with symptoms of fever, sloughing of the back, &c. In addition to old apoplectic extravasations, the aorta immediately beyond the origin of the left subclavian artery exhibited a very considerable contraction of a circular form, as if produced by a cord tied round it. The vessel shortly after became dilated, and then resumed its natural size. The coats of the aorta were healthy, though somewhat thickened, and the body having been injected, it was found that a free collateral circulation had been established.

In M. Reynaud's paper, the cases of M. Paris, Sir Astley Cooper, and Dr. Graham, and the latter as reported by M. Rainey in 'Corvisart's Journal,' under the supposition of its being a distinct case, are referred to.

7. In the year in which the last case was published, a second was reported by M. Pellétier.§ The patient, a man forty years of age, under the care of M. Trouvé, of Caen, died after an illness of nineteen months, characterized by dyspnœa, palpitation, &c. The heart was greatly enlarged, and its cavities dilated. The commencement of the aorta was also dilated, and its coats contained osseous plates, and at

* Neue seltene Beobachtungen, p. 66. Berlin, 1824.

† Meckel's Archiv für Anatomie und Physiologie, p. 345, 1827; Tiedemann von der Verengung, etc., der Pulsadern, p. 15.

‡ Journal Hebd. de Méd., tome i.

§ Archives Générales de Médecine, tome xviii. (sixième année) p. 205. 1828.

the point of origin of the ductus arteriosus there was a contraction which would barely admit the forefinger. The abdominal aorta was natural.

8. In 1831, Mr. Jordan, of Manchester,* related the case of a man, twenty-one years of age, who dropped down dead in the street, and no other information of his previous state of health was obtained than that he had been subject to fits. He was of intemperate habits. The pericardium was distended with blood, which had escaped from the sac of a dissecting aneurism of the aorta. The aorta three lines below the insertion of the ductus arteriosus was completely obliterated; its coats were healthy. The collateral vessels were greatly dilated, and the abdominal aorta retained its natural size.

9. In 1834, a full report of a very interesting case of contraction of the aorta was published in Dublin, by Mr. Nixon.† The patient was a medical man, twenty-seven years of age, who had from early life been subject to attacks of pain in the right side. Latterly he had a tumour in the left hypogastrium, with symptoms of cardiac disease, and a *bruit de soufflet* was audible in the course of the aorta. He died after an illness of nine months. The heart was found larger than natural, and there was great hypertrophy and dilatation of the left ventricle, with disease of the aortic valves and constriction of the orifice. The aorta, opposite the point where it united with the arterial duct, was contracted as if by a sharp instrument having been pressed upon its upper surface, so that its calibre was reduced to one half. The coats of the aorta were not diseased; the arterial duct was pervious. The liver was diseased, but the cause of the hypogastric tumour noticed during life was not ascertained. In the remarks appended to the report of this case, the cases of M. Paris, Mr. Winstone, M. Reynaud, Mr. Jordan, and Dr. Graham, and the latter from the Report in the French journal, are referred to.

10. During the same year in which the last case was reported, another was placed on record by M. Le Grand,‡ in a tract published in Paris. The patient, a man forty-eight years of age, was first seen by M. Le Grand and M. Kapeler two years before his death, and then presented symptoms of plethora, under which he had laboured for two years. He subsequently suffered from violent palpitation of the heart, and of the innominate, carotid, and subclavian arteries, and a *bruit de soufflet* was heard in the course of the aorta. These symptoms were followed by anasarca, and the usual evidences of cardiac asthma. His death was accelerated by an undue dose of morphia. The examination of the body was performed by M. Amussat. The heart was found very large, and the left ventricle especially greatly hypertrophied and dilated. Immediately beyond the origin of the left subclavian artery, where the aorta becomes descending, there was a slight contraction, followed

* North of England Medical and Surgical Journal, from Aug. 1830, to May, 1831, vol. i. p. 101.

† Dublin Journal of Medical Science, vol. v. p. 386. 1834.

‡ Du Rétrécissements de l'Aorte, &c., Paris, 1834, noticed in the Gaz. Méd. de Paris, 1834; and Arch. Gén. de Méd., deuxième série, tome viii. p. 528. 1835.

by a swelling, which was again succeeded by a still more marked contraction, as if the vessel had been included in a knot ; beyond this point the vessel dilated, but did not attain its usual size. In the seat of the greatest contraction, the interior of the vessel displayed a circular partition with an aperture only one and a-half line in diameter.

The memoir of M. Le Grand contains abstracts of five of the previously published cases, the error being again committed of relating the case of Dr. Graham from the report in Corvisart's Journal, as well as from the original source. The same mistake was also committed by M. Barth, who published a memoir on the different forms of aortic obstruction and obliteration in successive numbers of the 'Presse Médicale,' also in the year 1834.

In 1839, two cases of this form of aortic defect were published, one by M. Mercier,* the other by M. Roemer.†

11. The subject of the first case, a patient at La Charité, was a man, thirty-eight years of age, who suffered from vertigo, with epistaxis, followed by pain in the region of the heart, inability to lie on the left side, undue pulsation of some of the intercostal arteries, and deficiency of pulsation in the vessels of the lower extremities, and a bruit was heard at the lower angle of the left scapula. He died in three months. The aorta proved to be dilated to beyond the origin of the left subclavian artery, it then became greatly contracted so that its cavity would only admit the passage of a blunt probe, and was filled with coagulated blood. The heart was enlarged, and the aortic valves diseased.

12. In the case related of M. Roemer the patient was an Austrian officer, who had seen much service in the wars of the French Revolution. He died suddenly, in his fiftieth year, and had previously been under the care of M. Eichler, suffering from dyspnœa, hoarseness, and a troublesome cough. The heart proved to be greatly enlarged and hypertrophied, and the coronary arteries were ossified, but the valves were free from disease. The ascending aorta was greatly dilated, and at its point of union with the ductus arteriosus its calibre was entirely obstructed for the space of half an inch. The lower portion of the vessel had only the size of the aorta in a boy of ten or twelve. The primary vesssels and their branches were greatly dilated.

13 and 14. In the plates of 'Pathological Anatomy,' which appeared between 1835 and 1842, Cruveilhier‡ has figured a specimen in which the aorta at the usual situation would only admit a fine stilet, the coats of the vessel being otherwise natural and the large branches dilated; and in the 'Catalogue of the Pathological Museum of the Royal College of Surgeons of Ireland,' published by Dr. Houston in 1840, a similar preparation,§ removed from the body of a soldier, a patient of Dr. Hargraves, is described. The arch of the aorta was dilated, and beyond the origin of the left subclavian artery it became

* Bulletin de la Soc. Anat., quatorzième année, p. 158 1839.
† Med. Jahrb. des Oesterreich. St., 1839 ; and Tiedemann, op. cit., obs. l.
‡ Liv. xl. pl. 3, fig. 3. § B. c. 183.

abruptly contracted, and for the space of half an inch was only capable of admitting of the passage of a goose-quill. It then equally abruptly dilated. The lining membrane of the aorta was atheromatous, and the ventricles of the heart dilated. The condition was not suspected during life.

15. Dr. Craigie,* in the very able memoir previously alluded to, has given the particulars of the case of Sarah Lyon, seven years of age, who died under his care in the Royal Infirmary of Edinburgh, in 1841, after an illness, apparently ordinary fever, of seven days' duration. The heart's action was observed to be peculiarly violent, and a thrilling sound was heard, intermediate between a bellows murmur and the sharp blow of a hard body inside the chest. The heart was much enlarged, and weighed ten ounces; the walls of the right ventricle were firm; those of the left ten lines thick. The valves were also firm. The arch of the aorta was dilated, and its lining membrane thick and irregular. Three-quarters of an inch beyond the origin of the left subclavian artery, the vessel, for a space of one-eighth to a quarter of an inch, was nearly impervious. It resumed its proper size below, but the coats were thin and flaccid. The pulmonary artery adhered firmly to the aorta at and slightly above the seat of obstruction. The ductus arteriosus was ligamentous.

16. In 1842, Dr. Fletcher, of Birmingham,† published the case of a female, nineteen years of age, in whom the heart was twice its natural size, the pulmonary vessels were diseased, the vessel was dilated so as to form a large pouch, and the septum of the ventricles was incomplete. In addition to these defects the aortic valves were not perhaps quite competent, the ascending aorta was dilated, and the descending aorta where it is joined by the ductus arteriosus was so constricted that it had only one-third the capacity of the ascending portion. The arterial duct admitted the passage of a hog's bristle as far as the coats of the aorta, where its cavity was closed by a transparent membrane. During life the cardiac dulness had been increased, and liquid pulsation and a purring thrill were observed between the second and third cartilages, with a loud rasping, systolic sound. Death occurred somewhat suddenly.

17. A case of this form of disease also occurred, in the year 1842, in Calcutta, and is recorded by Dr. Wise.‡ The subject of the case was a middle-aged native, who fell down while walking, and immediately expired. The arch of the aorta was enlarged, and the vessel, after giving off the left subclavian artery, became suddenly contracted, as if from a ligature having been tied tightly round it, and beyond the union with the closed ductus arteriosus became completely impervious. It then quickly recovered its natural capacity. The left ventricle was much hypertrophied, and the sudden death had resulted from the rupture of a dissecting aneurism of the ascending aorta.

* The cases quoted in this memoir are those of Paris, Graham, Winstone and Cooper, Otto, Meckel and Herman, Reynaud, Le Grand, Jordan, and Nixon.

† Medico-Chirurgical Transactions, vol xxv. p. 282. 1842.

‡ Transactions of the Medical and Physical Society of Calcutta, vol. viii. p. 384. 1842.

18. In the same year, Tiedemann* narrated a third case in his elaborate work 'On the Contraction and Obliteration of Arteries.' It occurred in a pensioner, sixty-nine years of age, who served for a short time in the army, and after exposure to the weather became dropsical, and was discharged in consequence. He had for many years been addicted to habits of intemperance, and died with symptoms of cardiac disease. On examination after death, in addition to great enlargement of the heart, and especially hypertrophy of the left ventricle, the ascending aorta was found dilated and its coats thickened, and beyond the origin of the left subclavian artery there was a firm cartilaginous septum in the cavity of the vessel, which left only an aperture half a line in diameter at the point of insertion of the obliterated arterial duct. Beyond this point the aorta dilated, and a free collateral circulation had been established.

In his memoir Tiedemann quotes all the instances previously noticed in this paper, but classes them with other cases of aortic obstruction and obliteration evidently of a different character.

19. In 1844, Hamernjk† published a memoir on this subject, in which he enumerated 11 other cases, and added one which had fallen under his own notice. The patient, a man, forty-two years of age, enjoyed good health till he sustained a fracture of the clavicle from the upsetting of a carriage, and he suffered ever after from palpitation, headache, epistaxis, &c. He died of pneumonia after an illness of ten days. A clear blowing sound was heard, somewhat after the systole, in the region of the heart, and in the course of the aorta and its branches. The arteries of the upper part of the body were obviously distended, and pulsated strongly. About an inch beyond the origin of the left subclavian artery there was a sudden circular contraction of the aorta, more especially at the back, which reduced the vessel to a diameter of 5‴ in one direction, and of 4‴ in the other. Above the contraction the aorta was somewhat swollen and ossified, and at the point of contraction there was a biconcave transverse wall, from 1 to $1\frac{1}{2}$ line in thickness, which entirely closed the tube. Below the obliteration the vessel became dilated, and then resumed its proper size. Two shrivelled vessels forming culs-de-sac, supposed to be the remains of the ductus arteriosus, existed on the concave side of the contracted part.

20. In the year following the publication of the last case, Dr. Chevers,‡ in the course of some remarks on the nature of these cases, referred to a preparation existing in the museum of Guy's Hospital, in which the ductus arteriosus was open. The preparation was removed from the body of a young man who died of pneumonia.

In 1845, Dr. Bochdalek,§ of the University of Prague, communicated two cases.

21. The first was met with in the body of a child which had harelip and fissure of the palate, and died of inflammation of the lungs when

* Opus citatum, Beobachtungen, ix. p 15. † Prager Vierteljahrschrift, p. 41. 1844.
‡ London Medical Gazette, vol. xxxvi. (new series, vol. i.) p. 187. 1845.
§ Vierteljahrschrift, p. 160. 1845.

twenty-two days old. The aorta, from its origin to the point of departure of the arteria innominata, was somewhat wider than natural. At a point two lines before the opening of the duct, and for a space of nine lines, the diameter of the aorta was reduced to four lines. The coats in this situation were thickened, and the calibre of the vessel was entirely obstructed by adherent lymph. Beyond the contraction the aorta resumed its natural dimensions. The ductus arteriosus Botalli was closed at the pulmonic extremity, and filled by a plug of adherent lymph. The heart was not materially enlarged, but the left ventricle was slightly thickened and its cavity dilated. The valves were natural; the foramen ovale was open.

22. The second case was that of a female child which died of tuberculosis when four years of age. The heart was somewhat increased in size; the walls of the left ventricle were greatly hypertrophied, so that at its mid point it measured six Vienna lines in width, and its cavity was enlarged. The right ventricle was thicker and firmer than natural; the auricles were dilated; the pulmonary artery was narrower than the aorta, but was disproportionately large as compared with its proper size; the duct of Botalli was completely obliterated; the ascending aorta was large. Immediately below the insertion of the ductus arteriosus, and seven lines below the origin of the left subclavian artery, the vessel was reduced to two lines in diameter, and continued so for the space of a line. The cavity was so small, that a sound three-quarters of a line in diameter could barely be passed through it. Below the constriction the aorta rapidly dilated and became unusually large, and was again reduced in size in the abdomen. The subclavian and intercostal arteries and their branches were greatly dilated, and had maintained a collateral circulation.

23. The volume of 'Guy's Hospital Reports for 1847' (p. 453) contains the account of a case of great obstruction of the aorta, by Mr. Muriel. The man who was the subject of the disease died at the age of twenty-five, and nine years before had been supposed to labour under disease of some of the large vessels in the chest. He recovered from those symptoms, but was never capable of any laborious occupation. He was suddenly taken ill after lifting a heavy weight, and died in a week. The heart was large. In the usual portion of the descending aorta there existed a constriction, almost amounting to obliteration; it admitted the passage of a probe, and might possibly have allowed the transmission of a small quantity of blood; a dense glandular tumour existed in the same part, and had occasioned the absorption of the bodies of some of the vertebræ.

24. In 1848, Dr. Blakiston* related two cases of this description. The first† of these was that of a female, twenty years of age, who died of tuberculosis. There were only two aortic valves, and the orifice was contracted, and in the aorta, beyond the origin of the left subclavian artery, there was also a constriction, and the coats were attenuated.

* Practical Observations on certain Diseases of the Chest, p. 98. 1848.

† Case xxix. p. 134.

25. In the other case,* that of a gentleman, forty-eight years of age, who died with the usual symptoms of cardiac disease, the heart was hypertrophied and dilated, but the valves were healthy. Where the aorta was joined by the ductus arteriosus, the coats of the vessel were so thickened that its capacity was reduced to half its natural size.

26. In 1848, the particulars of two cases which occurred in the "clinique" of Professor Oppolzer of Prague, and in which he effected a diagnosis of the affection during life, were published by M. Hamernjk.† One of these, in which the diagnosis was confirmed on post-mortem examination, is as follows:

An apothecary, aged twenty-five, presented himself at the "clinique" with the symptoms of tuberculosis and diabetes. In the abdominal aorta no pulsation was detectable, and in the crural artery it could with the greatest difficulty be discovered. The pulsations were somewhat more distinct on the back of the feet: they were a little later than those of the radial artery; there were enlarged vessels on the back, chiefly on the left side. The enlarged arteria transversalis colli was visible in the left supra-clavicular region; and on both sides of the inner margin of the scapula, particularly the left, and also at the side of the third dorsal vertebra, under the left scapula, a large artery was seen and felt. At the sides of the sternum the diffuse murmur of the internal mammary artery and that of the intercostal branches at the back could be heard. The subclavian and right carotid arteries were dilated and elongated. On post-mortem examination, the aorta was found constricted in the usual situation.

27. The year after the appearance of the last case, Dr. Van Leeuwen‡ published the account of a case which occurred at Utrecht, and in which he diagnosed the affection during life. A boy, aged fourteen, enjoyed good health till two months before he came under observation, when a tumour, which proved to be an aneurism, appeared in the left arm. When the boy was examined, pulsation was seen in the region of the heart, in the intercostal spaces, at a distance of half an inch from the edges of the sternum, under the clavicle, particularly towards the axilla, and above the clavicles at their inner extremities, and in the suprasternal fossa. The pulse of the radial artery was evidently later than that of the heart; a purring tremor was felt in the right subclavian artery above the inner extremity of the clavicle; the subclavian, the transversalis scapulæ, and transversalis colli arteries, were unusually large and tortuous; the carotids were less altered. The impulse of the arch of the aorta was violently felt, while that of the abdominal aorta and the iliac artery could not be perceived, and that of the crurals was scarcely detectable. The percussion sound was somewhat defective at the upper part of the sternum. In the whole pericardiac region two sounds were heard, accompanied by murmurs, which became as it were rasping between the third and fourth right cartilages, and there was a strong systolic souffle along the subclavian and transverse scapular arteries. There was no murmur in the abdomen, but it existed along

* Case xxx. p. 135. † Vierteljahrschrift, p. 40. 1848.
‡ Nederlandsch Lancet, second series, fifth year, No. 2.

the whole vertebral column posteriorly. The patient died with symptoms of purpura, and on examination, in addition to various exudations of bloody serum, &c., the aortic orifice was found to have only two valves, and was somewhat narrow. The bulb of the aorta was dilated, but at the origin of the arteria innominata the vessel began to diminish in size; beyond the left subclavian artery it was still smaller, and about an inch lower down there existed a constriction, as if caused by a ring, which left an opening only admitting a quill, or six millimetres in diameter. This orifice had a triangular form, and was situated at the point of insertion of the obliterated duct. The vessel beyond the constriction became dilated, and its inner surface was covered by a fibrinous exudation. The walls of the heart were flaccid, and its cavities dilated.

In addition to this case, Dr. Van Leeuwen appended abstracts of the cases of Mercer, Hamernjk, and Oppolzer, and entered into an investigation of the signs by which similar defects are detectable during life.

In 1852, M. Lébert* published a memoir on this condition, illustrating it by reports of two cases, one observed by himself in the practice of M. Louis, the other which occurred to M. Barth.

28. M. Lébert's patient was a young man, twenty-two years of age, who had formerly suffered from epistaxis and sponginess of the gums; for five years he had been subject to headache and to pains in the fleshy part of the legs, and the limbs were occasionally swollen. From early life his breathing had been difficult on exertion, and for two years he had decided symptoms of cardiac disease. He was admitted into the Hôtel Dieu for a purpurous affection. The impulse of the heart was powerful, and a murmur which immediately followed the systole was audible about the left nipple; it became sharper and louder in the course of the aorta and subclavian and carotid arteries, and was most intense under the left clavicle. It was also heard along the spine posteriorly, and especially on the left side, but decreased in intensity from the infra-spinous fossa. It was inaudible in the crural arteries. After his admission, this patient became subject to violent attacks of dyspnœa; the pulse, previously slow, became accelerated; his ankles were swollen, and the urine became albuminous. He does not appear to have suffered from palpitation. He was seized with pleuro-pneumonia, and died in ten days.

On examination, the heart was found moderately hypertrophied; the aortic valves were somewhat roughened; the arch of the aorta was healthy, and the vessels arising from it large; beyond the origin of the left subclavian artery the aorta became narrow, and in the neighbourhood of the ductus arteriosus was only twenty-five millimetres in circumference. Below that point the vessel expanded, and formed a sac-like pouch. In the interior of the artery at the commencement of the constriction there was a transverse septum which occupied half the convex side of the vessel; beyond this the coats were very much attenuated, and at the point of union with the ductus arteriosus there

* Archiv, &c., von Virchow und Reinhardt, s. 327. Berlin, 1852.

was a second septum, which entirely closed the cavity of the vessel. Immediately around the entrance of the duct there was a mass of condensed areolar tissue; the internal coats were thickened in the seats of constriction.

29. The case of M. Barth was that of a man, thirty-two years of age, who had enjoyed good health till he suffered from transient rheumatic pains three years before. Two years and a half before he began to have dyspnœa especially on exertion, palpitation, cough and expectoration occasionally streaked with blood, and these symptoms were followed by tinnitus aurium, lividity of lips, and œdema of the ankles. The cardiac dulness was increased in extent, and a systolic murmur was heard, most intensely, at the point of pulsation of the apex. He died with symptoms of acute laryngitis. The heart was found very large, the aortic valves were very immovable and displayed vegetations, and the orifice was narrowed. The aorta was small at the ascending part of the arch, and at the commencement of the descending portion there was a constriction, after which the vessel regained its size, but again became contracted so that a little below the ductus arteriosus it had only a diameter of two lines. The vessels at the arch were enlarged; after the aorta resumed its size, it then diminished, and remained small in the abdomen. The constriction was formed by a sharp projection of the internal coats.

30. In addition to other cases which have already been referred to, M. Rokitansky, in his 'Pathological Anatomy,' quotes a case which had occurred to M. Dlauhy. The patient, a man, twenty-seven years of age, had suffered for a year from palpitation increased on active exertion, and he died with dropsy and other cardiac symptoms. The heart was twice its natural size, and the walls of the left ventricle were an inch in thickness. The ascending portion of the arch was large, but after giving off the arteria innominata, the vessel diminished, and at the part where the obliterated ductus arteriosus was inserted, it had only a diameter of 3‴, and was becoming obliterated. At the lower part the constriction was so great that the canal had only a diameter of 1‴, and was obstructed by a plate of white opaque deposit. The primary vessels and their branches were dilated. The same author, in his monograph "On Some of the more Important Diseases of the Arteries," has detailed two other cases of contraction of the aorta.

31. One of these was observed in Professor Skoda's wards in the Vienna Hospital, and is reported by M. Löbl.* The patient, a man, twenty-nine years old, was first under care for symptoms of insufficiency of the aortic valves in 1841; in 1844 he had pneumonia, and in 1848 died with symptoms of fever, vomiting, dyspnœa, and cyanosis. Fluid was contained in the pericardium, and the heart was covered by a pseudo-membranous exudation. The organ was very greatly enlarged. The walls of the right ventricle were three lines in thickness, and those of the left ten lines; and the conus arteriosus of the right ventricle was enlarged. The aortic valves were incompetent. The ascending aorta was greatly dilated, and beyond the truncus anony-

* Obs. 20, Tab. xiv. B.

mous, it became contracted; and the size diminished still more beyond the origin of the left subclavian artery, till at the entrance of the ductus arteriosus it had only a diameter of one line, and its cavity would barely admit of the passage of a small probe or sound. Beyond this point the vessel again dilated. The branches from the arch of the aorta were dilated, and the ductus arteriosus was pervious for half its length from the aorta.

32. The subject of the second case was an ecclesiastic, who died, at the age of fifty-seven, with symptoms of plastic effusion and dropsy in the abdomen, after an illness of thirteen days.* The heart was very large, and the left ventricle greatly dilated, and its surface covered by tendinous false membrane; and an exudation also existed on the lining membrane of the left auricle and ventricle. The aorta was large, and the vessels arising from it dilated. Beyond the left subclavian it became very narrow, so that it was at first only 10‴ in circumference, and after a course of six lines, became only from one and a half to two lines wide; and above and below the insertion of the obliterated ductus arteriosus, its cavity was entirely obstructed. The descending aorta was small.

33. In 1853, Mr. Sydney Jones exhibited at the Pathological Society, and described in the 'Transactions'† a specimen of obliterated aorta, which had occurred in an injected subject in the dissecting-room of St. Thomas's Hospital. Of the subject, a man, forty-five years of age, no other history was obtained than that he had suffered from symptoms of chest disease. The aorta at the usual point was reduced to a ligamentous cord for about half an inch. The ascending aorta was greatly dilated, the coats somewhat ossified, and a free collateral circulation existed; so that the descending aorta soon resumed its natural size. The preparation of this case is retained in the Museum of St. Thomas's Hospital.

34. In the same year, another case was published in Germany. A female, fifty-six years of age, came under the care of Dr. A. Härlin, of Stuttgart,‡ labouring under difficulty of swallowing, orthopnœa, vomiting, pain in the epigastrium and faintness, dry, morbidly red tongue, and feeble and irregular pulse. There was powerful pulsation in the epigastrium, greatly increased extent of dulness on percussion, and a rubbing sound with the systole of the heart, and bronchitic signs. On dissection the heart was found very large. The left ventricle especially, hypertrophied and dilated. The aortic valves were thickened, ossified, and incompetent. The origin of the aorta was dilated to twice its normal capacity, but at the part where the large vessels were given off, there was a stricture which would scarcely admit the passage of the little finger, and a second at the point of entrance of the ductus arteriosus. Immediately beyond the latter contraction, the vessel became unusually large. The coats of the aorta were atheromatous, and near the seat of contraction much thickened.

* Obs. 21, Tab. xv. † Path. Trans., vol. viii., 1856–7, p. 159.
‡ Archiv, &c., von Virchow, p. 273. Berlin, 1853.

In 1856 and 1857 two cases were made the subject of memoirs addressed to the Société de Biologie.

35. The first of these was related by M. Dumonpallier, an interne of the Hôpital La Riboisière,* and the case occurred in the practice of M. Voillemier. A female, thirty-nine years of age, was admitted into the hospital for an abscess probably connected with disease of the hip joint. After puncture she had feverish symptoms, followed by œdema of the extremities, with dyspnœa, lividity, and a *bruit de soufflet* was perceived with the systole of the heart: she died in two weeks.

The ascending aorta and vessels at the arch were large; beyond the origin of the arteria innominata the artery diminished in size, and behind the left subclavian artery had only a diameter of thirteen millimetres. At the point of insertion of the ductus arteriosus, there was a circular constriction, with a kind of diaphragm internally, produced chiefly by the middle coats, which left only a triangular aperture 0·012 in diameter. The aorta below soon recovered its normal size, and the vessel was otherwise free from disease. The collateral vessels were greatly dilated.

With the account of this case a report of others is appended; though the old error being committed of quoting Dr. Graham's case from the two different sources, the number, exclusive of the case of M. Dumonpallier himself, is really only nine cases. An able analysis is also given of the cases quoted.

36. The other case was brought before the Society by M. Leudet,† in whose practice it occurred, at the Hôtel-Dieu of Rouen. The subject of the case was a female, thirty-seven years old, who had suffered for nineteen months with dyspnœa, increased on exertion, difficulty of swallowing and especially of speaking, pains in the chest, and œdema of the extremities. A *bruit de soufflet* was heard at the base of the heart; the cardiac dulness was extended, and the vessels on the left side, especially around the scapula, were enlarged. The symptoms suddenly became aggravated: she threw up a large quantity of blood, and rapidly sank. Some old adhesions and a white patch were found on the heart. The left ventricle was somewhat dilated, but the valves were healthy. The arch of the aorta was of natural size, and the coats free from disease, but the primary vessels were somewhat enlarged. Immediately below the origin of the left subclavian artery, there was an infundibular-formed contraction, which reduced the calibre of the vessel so greatly that the opening would only admit a blunt probe. There was no induration or ossification of the coats at the seat of the contraction, but the middle and internal tunics were hypertrophied. Below the constriction there was a large, irregular, aneurismal tumour, with osseous and cartilaginous plates in its walls, and which opened freely into the left bronchus. The collateral vessels were enlarged. The account of this case is accompanied by an enumeration of nearly

* Comptes Rendus des Séances et Mémoires, tome iii., deuxième série, année 1856, p. 273, Paris, 1857; and Gaz. Méd. de Paris, vingt-septième année, troisième série, tome xii. pp. 52, 188, 295.

† Ibid., tome iv., deuxième série, année 1857, p. 63, Paris, 1858; Gaz. Méd. de Paris, vingt-huitième année 1858, p. 44.

all the cases of contraction of the aorta published at the time of its occurrence, and by some valuable remarks and inferences as to the nature and peculiar characters of the affection.

In 1858 two cases of aortic obliteration were exhibited at the Pathological Society.

37. The first of these was presented by Dr. Wilks.* The subject of the case was a man, twenty-two years of age, a patient of Dr. Rees, in Guy's Hospital. He was well developed and strong-looking, but was never capable of active exertion, and died with the usual symptoms of cardiac disease, after an illness of six weeks. The heart was hypertrophied and dilated on both sides. At the situation of the ductus arteriosus, the aorta appeared as if a ligature had been placed round the vessel, and the cavity was reduced so greatly that it only admitted the passage of a probe. The thoracic and abdominal portions of the aorta were small, but the vessels arising from it were large. The aortic valves were only two in number.

38. The second specimen was exhibited by Mr. John Wood.† It was removed from the body of a man of remarkable muscular development. The contraction existed in the usual situation, and had apparently occasioned complete obstruction. It appeared as if a piece of packthread had been tied lightly round the vessel. The heart was somewhat enlarged, and a free collateral circulation had been established.

During the present year two cases have been placed on record. For a knowledge of the first one, which occurred to Hr. Kjellberg, and of which the specimen was exhibited at the Swedish Society of Physicians, I am indebted to Dr. Moore, of Dublin. I am also under great obligations to that gentleman for the translation of the paper of Dr. Van Leeuwen, and of the cases of MM. Lebert and Barth.

39. Hr. Kjellberg's‡ case is as follows:—Mademoiselle H——, aged twenty-eight, had from infancy been weak and sickly, and up to two years could not be got to repose in the recumbent position; so that she required to be constantly carried in the arms. At that age she had a bad attack of nervous fever, after which her health improved, and she gradually became a healthy and cheerful child. Subsequently, with the exception of suffering for a short time with ague, her health continued good. She had neither headache nor cold feet, but her hands were usually hot, and her cheeks hot and livid. She died instantaneously, while stooping to put on her boots. Immediately beyond the left subclavian artery, there was a constriction so considerable that the bore of the vessel was scarcely a line in diameter. In the immediate vicinity of the constriction there were some atheromatous deposits, and below it, as far as the bifurcation, the vessel was small. The left subclavian artery was greatly dilated. The right ventricle of the heart was almost wholly changed to fat, and the left ventricle was concentrically hypertrophied, and some small deposits existed in the aortic and mitral valves.

40. The last case to which I have to refer is one which has very

* Trans. of Path. Soc. of London, vol. x., 1858-9, p. 77. † Ibid., p. 97.
‡ Hygeia, Medicinsk och Farmaceutisk Mundschrift, vol. xxi. No. 6, p. 376.

recently occurred to Dr. Barker, at St. Thomas's Hospital, in a man twenty-four years of age. In this instance the obliteration was very nearly complete; there being only a small aperture capable of admitting a probe in the septum, which occasioned the obstruction. The ductus arteriosus was open at its aortic extremity, but closed at the pulmonic end. There were only two valves at the aortic orifice. Death was occasioned by the formation of a dissecting aneurism of the ascending aorta, and the rupture of the sac into the pericardiac cavity. The case will be reported at length to the Medico-Chirurgical Society.

REMARKS.

Seat, form, and extent of the obstruction.—Of the cases detailed, if we except one in which the obstruction is loosely described as situated at the arch, in all the most decided contraction is stated to have been at or near the point of junction of the ductus arteriosus with the aorta. In 6 cases, it is reported to have been situated beyond the origin of the left subclavian artery; in 1, above the point of insertion of the ductus arteriosus; in 16, at that point, and in 13, below it. In 4 cases, in which there were two marked constrictions, the first was situated in three instances beyond the origin of the left subclavian, and in the fourth case at the seat of junction with the duct. The lower stricture was in 1 of the cases situated at the duct, and in the other 3, below it.

In 22 cases the obstruction appears to have commenced more or less abruptly; and in 15 the aorta gradually diminished in size, or after having become increasingly contracted for a longer or shorter space, suddenly displayed a more decided constriction. In 7 of the latter cases, the contraction commenced beyond the origin of the brachio-cephalic trunk; in 7, beyond the left subclavian artery or at the descending portion of the arch; and in 1 instance the aorta was smaller than usual throughout its whole extent.

When the obstruction was abrupt, it appears to have generally been the most marked on the outer or convex side of the vessel, and is described, in different instances, as having been as if produced by a knife pressed on the coats of the vessel, a string tied tightly round it, or a ring passed along it. Internally, the constriction was often considerably greater than appeared from the size of the vessel externally; and this was generally due to the contraction and thickening of the internal tunics; but in 8 cases, the calibre of the vessel was obstructed by a septum, apparently formed by a duplicature of the internal coats, which either, as in 1 case entirely closed the canal, or left only a small triangular, oval, or rounded aperture, generally on the concave side of the vessel. In 1 case this septum is described as having been very thin, and as having become still more attenuated towards the aperture; in two instances, it was thicker, and had a biconcave form; in another, it presented two projecting lips, and in a fifth case, the septum was protruded forwards in the course of the vessel, so as to form a funnel-shaped aperture.

The degree and extent of the obstruction differed in different cases:

in 10 instances, the canal of the vessel was entirely obliterated; while in 30 the obstruction was incomplete. In the cases of entire obliteration, the obstruction was, in 1 instance caused, as before mentioned, by a septum extending across the cavity of the vessel; in another, the calibre of the artery was contracted, and the cavity closed by adherent coagula; and in a third, fibrinous material was infiltrated into the coats. In the remaining 7 cases, the defective portion of the vessel was converted more or less completely into a ligamentous cord.

In the 30 cases in which the obstruction was incomplete, the degree of contraction varied greatly. Thus, the vessel would only admit a small flat or blunt probe in 8 cases; a straw in 1 case; a writing-quill in 3 cases. It is stated to have had only a very small cavity in 1 case; it would barely admit the little finger in 2 cases, the index finger in 1 case, and was only one-third its natural size in 1; one-half its natural size in 2; it was greatly contracted in 2; had a diameter of six millimetres in 1 case (·23622 E.I.), eight millimetres in 1 case (·31496 E.I.), thirteen millimetres in 1 case (·51181 E.I.), half a line in 1 case (·0444 E.I.), three-quarters of a line in 1 case (·0666 E.I.), one line in 1 case (·0888 E.I.), one and a half line in 2 cases (·1332 E.I.), and 2 lines in 1 case (·1776 E.I.). In one instance the aperture was still further diminished by vegetations around the edges.

In the cases in which the vessel was completely obliterated, the space over which the obstruction extended varied from half a line to a quarter, and, in 1 instance, half an inch; in 3 cases, in which its cavity was only diminished, the extent of the contraction was greater, or half an inch in several cases, nine lines in one instance, and one inch in another.

The *condition of the vessel above and beneath the seat of stricture* was also somewhat varied. Most generally the ascending portion of the arch appears to have been dilated, and the coats thickened, atheromatous, or osseous; and not unfrequently these changes extended to a larger portion of the arch, and in some instances to near the seat of constriction. Thus, deducting the cases in which the condition of the vessel is not specially named, in 11 it was dilated, but the coats do not appear to have been materially diseased; in 10 cases the dilatation appears to have been greater, or to have extended over a larger space, and the coats were extensively diseased; in 3 cases the vessel was dilated, and the coats thinned; and in 1 case there was a decided pouch in the ascending portion. In 1 case the vessel was small throughout its course, and in 3 cases it was of natural size, and free from all appearances of disease.

Below the seat of stricture the vessel generally quickly dilated, and the dilatation occurred sometimes quite abruptly, the vessel not unfrequently being at first larger than natural, and then becoming gradually contracted. Sometimes the vessel continued of smaller size than natural, and not only immediately beneath the stricture, but throughout its whole course. The coats were frequently thinner than natural, and in 1 case there was a large aneurismal tumour of the descending aorta.

Effect produced on the adjacent vessels and on the heart.—It appears that in most of the published cases, while the blood had been incapable of being conveyed adequately, or at all, by the trunk of the aorta from the upper into the lower portion of that vessel, the circulation had nevertheless been maintained with considerable freedom in the lower parts of the body by means of a compensatory collateral circulation. The mode in which this was accomplished was very evident when, as in the case of M. Meckel and in the preparation preserved in the museum of St. Thomas's Hospital, the body had been injected previous to dissection. From these specimens it appears that the channels of communication were the branches of the subclavian arteries and the aortic branches below the seat of constriction on both sides of the body.

1st. The transversalis colli artery derived from the thyroid axis, which through its posterior scapular branch anastomoses with the posterior branches of the aortic intercostal arteries.

2ndly. The superior intercostal artery derived from the subclavian, which anastomoses with the aortic intercostals.

3rdly. The internal mammary, derived also directly from the subclavian artery, which by means of its anterior intercostal branches communicates with the anterior aortic intercostal artery, by its musculo-phrenic artery unites with the lower intercostals, and by the superior epigastric artery with the epigastric derived from the external iliac artery. The thoracic branches of the auxiliary artery appear also to have assisted in conveying the blood into the descending aorta, and all these vessels become very greatly enlarged, elongated, and tortuous.

While, however, the blood was thus capable of being transmitted from the arch of the aorta into the descending portion of the vessel, such collateral channels afford a less free passage than that of the aorta itself, and we therefore find, as would naturally be expected, that the obstacle so occasioned produces similar effects upon the heart to those which result from any other form of aortic obstruction. The cavities of that organ generally become dilated and their walls increased in thickness, and when the defect has been very marked and of long duration, the whole organ becomes greatly enlarged. Thus, in 10 cases there was a moderate amount of hypertrophy and dilatation involving chiefly or only the left ventricle; while in 18 instances the right ventricle was also affected and the enlargement was general and extensive. In 1 case it is stated that the cavities were dilated, and their walls flaccid; in 1 case the right ventricle was very greatly overgrown with fat, and in 2 instances the organ is stated to have been quite healthy.

More or less marked aortic valvular disease existed in 12 of the cases, and mitral valvular disease in 1, and the endocardium was thickened and opaque, or more extensively diseased, in other instances. In some of the cases of aortic valvular disease the orifice was contracted from independent disease, but in other instances, it was dilated so that the valves were rendered incompetent; and this apparently resulted from the dilatation of the ascending portion of the aorta, involving the fibrous zone which forms the orifice of the ventricle.

Independently of the results of recent disease which were frequently found in the cavity of the pericardium, as blood, serum, or fibrinous exudations, resulting from the rupture of the aorta, or of pericarditis which had occurred in the latter periods of life, that membrane displayed in several cases the evidences of former inflammation, in the form of decided adhesions, or of the usual white or milk spots on the surface of the heart.

Age, sex, symptoms during life, and cause of death.—From the annexed table* of the ages of the persons who were the subjects of aortic obstruction or obliteration, it will be seen that the defect has occurred at all periods of life—from a child twenty-two days old to a man of the age of ninety-two; but the largest proportion of persons were middle-aged. Thus, of 38 whose ages are reported, 23, or 60·5 per cent., were from twenty-one to fifty years of age inclusive, and another person is stated to have been middle-aged. The defect also is more common in males than in females—38, or 73·7 per cent. having been males, and only 10, or 26·3 per cent. females. These facts to a certain extent associate this affection with the more ordinary morbid changes in the large arteries, which, as is well known, are of much the most common occurrence in men during the active periods of life, when they are most exposed to sustain accidents or to suffer from over-exertion. On the other hand, the occurrence of aortic obstruction or obliteration in infants and young people—as in a child twenty-two days old, 2 boys of fourteen, and 5 girls of four, seven, seventeen, nineteen, and twenty years of age—is opposed to the view that the change can be entirely of accidental origin, but rather indicates its dependence on some congenital imperfection, the effects of which become developed or aggravated, during the progress of life and from the ordinary causes of disease.

* Age and sex of patients affected with aortic contraction or obliteration:

Males.		*Females.*	
14	2	4	1
21	1	7	1
22	2	17	1
24	1	19	1
25	2	20	1
27	2	28	1
29	1	37	1
32	1	39	1
35	1	50	1
38	1	56	1
40	2	Total	10
42	1		
45	1		
48	2		
50	1		
57	2		
69	1		
92	1		
Young	1		
Middle-aged	1		
Not stated	1		
Total	28		

Age and sex not stated, 1; child, 22 days. Ages of patients in whom the aorta was obliterated, 22 days, 7 years, 14, 21, 42, 45, 50, 57; middle-aged, not stated.

The cases in which this defect has been observed, so far as their *symptoms and history* are concerned, may be classed into four groups:

1stly. Those in which no history of the symptoms during life is recorded, or in which the patients died suddenly or of some disease not connected with the condition of the aorta, and the defect was only detected on examination after death.

2ndly. Those in which the subjects of the affection came under the observation of the reporters for some acute or chronic disease of an independent character, and in which the evidences of some defect in the vascular system were detected, as it were, accidentally.

3rdly. Those in which the patients, after having enjoyed good health, and been equal to active exertion for a long period, manifested the symptoms of cardiac disease; such symptoms often appearing after some obvious exciting cause, and gradually advancing till they assumed the usual features of cardiac asthma and dropsy. In these cases the interval which elapsed between the first occurrence of the symptoms and the fatal termination varied greatly;—in some cases it was only two, three, or four months, in others five and eight years.

4thly. Those in which the patients had always been delicate, manifesting especially, symptoms of feebleness of the circulation, breathlessness, lividity, chronic cough, &c.

Of these groups, about 12 of the published cases may be referred to the first; 6 to the second; the largest number, or 18, to the third; and only 4 to the fourth.

As to the *causes which immediately occasioned the deaths* of the persons in whom the aortic defect was found, they may be divided into:—1stly. Those in which the patients died of acute or chronic diseases, but little or only indirectly, connected with the morbid condition of the vascular system. Of this description there were 11 cases; the causes of death having been hemiplegia connected with disease of the brain and complicated by sloughing of the integuments of the back, in 1 case; cerebral symptoms terminating in coma, in 1; acute pneumonia, in 2; acute pleurisy, in 1; chronic bronchitis, in 1; fever and bronchitis, in 1; and tuberculosis (with diabetes in 1), in 4 cases.

2ndly. In another set of cases, death occurred suddenly and was directly traceable to the condition of the aorta. Of the eight or nine cases of this kind, blood had escaped into the cavity of the pericardium from rupture of recent dissecting aneurisms of the ascending aorta, in 3 instances; a dissecting aneurism of the arch of the aorta had ruptured into the pericardium in 1; the right ventricle and one of the coronary veins had been ruptured in 1; the right auricle had given way in 1; a large aneurism, originating from the descending aorta below the seat of the constriction, had burst into the left bronchus in 1 case; and death occurred from syncope, connected with fatty degeneration of the right ventricle, in 1 case. In this group should also probably be placed a case in which death is reported to have occurred instantaneously from apoplexy, but more probably from syncope.

Lastly, in the largest proportion of cases, 16 out of the 36 in which the cause of death is clearly stated, the patients sank with the ordinary symptoms of cardiac asthma and dropsy, complicated in different instances by bronchitis, pneumonia, pleurisy, pericarditis, erysipelas and sloughing, purpura, &c.

Mode of production.—The cases of aortic contraction or obliteration which have been briefly quoted, differ essentially from those in which the vessel is extensively diseased, or its cavity obstructed by tumours, coagula, or inflammatory exudations, such as the cases of Goodisson,* Schlesinger,† Maigne,‡ Stentzell,§ Barth,‖ and Duncan,¶ in the circumstances that the defect is situated always at or near the same part of the vessel, and that the coats of the artery are not necessarily diseased. Writers have therefore generally regarded them as originating in, or connected with, some error in the original conformation of the vessel, or as they have been termed, as '*quasi congenital.*' That this view is correct can scarcely be doubted; for it not only affords the only satisfactory explanation of their mode of production, but is in accordance with the ascertained frequency of other unquestionably congenital defects in such cases. Thus, out of the 40 cases collected, in 5 the semilunar valves at the aortic orifice were defective in number; in 1, the columnæ carneæ of the mitral valves were wanting; in 1, the septum of the ventricles was imperfect; in 1 (a child twenty-two days old), the foramen ovale was unclosed; in 3, the ductus arteriosus was more or less pervious throughout its whole extent; and in 4 it remained open for a portion of its aortic extremity but was obliterated at the pulmonic end; in 1, there was hare-lip and fissure of the palate; in 1, hypospadias, and in 2, defective conformation of the lower extremities; an amount of errors of development which would scarcely be met with in an equal number of cases of any form of accidental disease.

While, however, it has been generally admitted that the aortic defect is in some way dependent on the faulty development of the vessel, the precise mode in which it is produced has been variously explained by different writers. First:—It has been supposed that the obstruction of the vessel may be caused by a thrombus formed in the process of closure of the ductus arteriosus being prolonged into the cavity of the aorta.

Secondly:—That the process of obliteration of the arterial duct may be propagated to the coats of the aorta, so as to involve that vessel; and

Thirdly:—That the defect may originate in the faulty development of the portions of the branchial arches which form the continuation of the aorta from the origin of the left subclavian to beyond the insertion of the ductus arteriosus.

In reference to the first supposition, it may be remarked that the

* Crampton: Dublin Hospital Reports, vol. ii. p. 193. 1818.

† Quoted from Caspar's Wochenschrift, 1835, in Tiedemann, op. cit., Beob. xlvi. p. 59.

‡ Bullet. de la Soc. Anat. douzième année, p. 219. 1837.

§ Haller's Dissertationes ad Morborum Historiam et Curationem, tom. ii., Lausannæ, 1757; and Tiedemann, op. cit., Beob. xxix. p. 43.

‖ Arch. Gén. de Méd., deuxième série, tom. viii. p. 26, 1835; and La Presse Médicale, obs. xiv. p. 491. 1837.

¶ London and Edinburgh Monthly Journal, vol. iii. p. 81. 1843.

process of obliteration of the arterial duct after birth, does not ordinarily depend on its cavity being closed by a thrombus, but consists in a gradual contraction and thickening of the coats, commencing at the aortic end and gradually advancing towards the union with the pulmonary artery; and that the coagula which form are not of large size and simply depend on the stasis of the blood.

The second theory, which is that advanced by Dr. Craigie, is opposed to the fact, first specially mentioned by Dr. Chevers, that the aorta is found contracted and obliterated in cases in which the ductus arteriosus is still pervious, as in the cases related by Dr. Graham and Mr. Nixon, and that alluded to by Dr. Chevers himself. It also does not accord with the mode in which the duct is, as previously observed, ordinarily obliterated: viz., by a process of contraction commencing at the aortic and advancing towards the pulmonic end; and to the circumstance that, in 4 cases in which the duct was incapable of transmitting blood, it yet remained pervious over a larger or smaller portion of its aortic extremity.

The third theory, which is that suggested by M. Reynaud, and supported by Rokitansky, there can be little doubt, furnishes the true explanation of this condition. In the adult, it frequently happens that the aorta between the left subclavian artery and the union with the ductus arteriosus, is found smaller and its coats thinner than either above or below those points; and occasionally, as stated by M. Reynaud, there is a marked contraction in some portion of that space. In children and infants this peculiarity is more obvious, so that the vessel often considerably increases in size beyond the point of entrance of the duct. The contraction, indeed, often attains to so great a degree as to prevent the closure of the duct after birth, and to make that passage the channel by which the blood, wholly or in part, reaches the descending aorta. Cases of this kind are known to pathologists under the terms of "*the descending aorta given off from the pulmonary artery*," and have been frequently described.

In such instances, the portion of the aorta beyond the left subclavian artery and above the union with the duct, may be only moderately contracted, very small, wholly impervious, or entirely wanting. Specimens exhibiting the slighter degrees of contraction in children have been exhibited at the Pathological Society by Dr. Chevers,* and by myself in a preparation removed from the body of a child under the care of the late Dr. G. A. Rees;† and an instance of the kind has been recorded by Mr. Barrett.‡ Of the more marked degree of contraction, a case described by Dr. Farre and Sir Astley Cooper, and of which the specimen is preserved in the Museum of St. Thomas's Hospital, affords an example.§ Another specimen, described by the same authors, and also in the St. Thomas's Museum, furnishes an illustration of entire closure of the post-subclavian portion of the aorta.|| And of the complete defect, in which there is one vessel—the aorta—arising from the left ventricle, and giving off the branches

* Path. Trans., vol. i. p. 55. † Ibid., p. 203. ‡ Lancet, vol. i. p. 349. 1834-5.
§ Farre on Malformations, &c., p. 15. || Ibid., p. 13.

to the head and upper extremities; and a second,—the pulmonary artery,—entirely unconnected with the former, arising from the right ventricle and giving off the descending aorta, the cases recorded by Steidele,* Gibert,† and Mr. Struthers and Dr. Gleig,‡ may be referred to as illustrations. The same condition, in its slighter form, is also occasionally seen in adults, as in a case which recently occurred to Dr. Barker,§ at St. Thomas's Hospital; and one which was exhibited at the Pathological Society by Dr. Babington and Dr. Barlow.‖

The form of defect which is the subject of this paper is certainly very closely allied to the class of cases now named; and it probably differs from them only in the degree in which the aortic obstruction exists at the time of birth. In the cases in which the ductus arteriosus remains more or less pervious, the contraction was then probably sufficient to occasion some obstruction; while in those in which the duct is found wholly obliterated, the contraction must at birth have been only very slight. The vessel might, indeed, be of full size, but the coats in that situation being somewhat rigid and unyielding, may not expand adequately with the progress of growth, and thus a source of obstruction may become slowly developed in after-life. Such a condition must be very favourable for the enlargement of the anastomosing vessels. When the establishment of a collateral circulation is complete, the contracted portion of the vessel, being no longer expanded by the current of blood, will undergo a tonic contraction, the stasis of blood in the contracted portion will be promoted, coagula will form, and the cavity of the vessel will gradually become obliterated. This seems to be the explanation of those cases in which the aorta is found very greatly contracted or entirely obliterated, in persons who die at advanced periods of life, and without having manifested during life any marked symptoms of aortic or cardiac disease.

Diagnosis.—It only remains to inquire what are the means by which we may hope to effect a diagnosis of this form of disease during life; and the inquiry is not without its practical use, for from the cases which have within the last few years been placed on record, it is evident that the defect is one by no means of very rare occurrence.

It will be remembered that in some of the cases no marked symptoms of disease of the vascular system had been observed during life; and in some instances in which the change had evidently long existed, it is expressly stated that the patients were quite well and capable of following their usual occupations till very shortly before death. In those cases, also, in which obvious symptoms had been produced, they

* Hein: De istis Cordis Deformationibus, &c., obs. 66. Goettingæ, 1816.

† Bull. de la Soc. Anat., année 14, p. 203. 1839.

‡ Edinburgh Monthly Journal of Medical Science, vol. xv. p. 29. 1852. See also my own work on Malformations of the Human Heart, pp. 107–109, where all these cases are mentioned.

§ The whole aorta was small, but the portion behind the left subclavian artery especially so, and the coats were there thinner than those of the artery elsewhere. The ductus arteriosus admitted a ball measuring eighteen French lines in circumference. The aortic and pulmonic and tricuspid valves were thickened and indurated, especially the former. The preparation was removed from a man, aged thirty, who died suddenly.

‖ Path. Trans., vol. i. p. 55. 1846-8. Cyanosis dependent on patent Ductus Arteriosus and Disease of the Aortic Valves, in a female, thirty-four years of age.

were often rather due to the secondary affection of the heart than to the original disease, and do not appear to have differed in any material respect from those of cardiac disease dependent on any other source of aortic obstruction. In other instances, however, there have been observed signs which pointed more decidedly to the specific character of the obstruction which existed.

1stly. It has already been named, that the vessels originating from the arch of the aorta, and their branches, were generally considerably dilated; and the transversalis colli, the posterior scapular, and the superior aortic intercostal arteries, together with the superficial branches distributed to the chest and back, were seen during life to be very obviously enlarged and contorted, and were felt to beat with undue force.

2ndly. In some instances a harsh murmur, which somewhat followed the impulse of the heart, was heard in and around the præcordial region. It was most intense at the base of the heart, along the upper part of the sternum, under the clavicles, especially on the left side, and at each side of the neck in front. It was also loudly heard at the root of the neck posteriorly, and along the course of the spine, especially in the left side; but it became less intense below the level of the spine of the scapula. A mumur was also audible in the dilated superficial arteries, and, as especially noticed by Hamernjk, in the course of the internal mammary arteries, but it could not be heard in the abdomen.

3rdly. While the arteries of the upper parts of the body beat with undue force, those of the lower extremities, as the inguinal and femoral, could barely be felt; the contrast being the more striking according to the degree of the aortic obstruction, and especially when the canal of the vessel is entirely occluded.

These signs would indicate some source of obstruction below the origin of the left subclavian artery, or at the usual point of the aortic disease; and if there be no deficiency of the resonance on percussion, no evidence of pressure excited on the recurrent laryngeal nerves, or of interference with the œsophagus, trachea, or bronchi, it would be a probable inference that such obstruction was situated in the aorta itself, and not produced by the pressure of an aneurism or other tumour. Such conclusion would be corroborated if it should appear that the obstruction had probably been of long duration, not brought on by any sudden muscular exertion or accident, but slowly progressing, and especially if it occur in persons whose age and sex would render it improbable that they could be subjects of aneurism.

Guided by observations of this description, the diagnosis has, as before stated, been effected in several instances—as approximatively by M. Mercier, and more decidedly by Oppolzer, Hamernjk, and Van Leeuwen, in cases in which the correctness of the opinions was verified by examination after death. Oppolzer and Dr. Walshe have also effected diagnoses in cases of which the result has not, so far as I am aware, been recorded. It seems, therefore, fair to suppose that, with the knowledge we now possess, a large proportion of the cases which occur will, in future, if under observation for a sufficient length of time, be detected during life.

Art. II.

On the Morbid Actions which constitute Osteitis. By Richard Barwell, F.R.C.S., Assistant-Surgeon Charing-cross Hospital.

Inflammation, in whatever structure it may occur, must be in reality the same action, only modified by certain secondary circumstances; and thus osteitis is essentially the same as inflammation of the soft parts, and like that phenomenon, leads to induration, suppuration, ulceration, or sloughing, or, indeed, to all these four terminations.

Ulceration of bone, or caries, nearly always involves a certain amount of necrosis, and if the disease be not diffuse, is accompanied by considerable deposition of new bony matter. It may affect the surface and periosteum, *caries superficialis;* or the dense substance or spongy texture, *caries centralis;* or all the whole thickness of the bone, *caries totalis.* It so rarely happens, however, that inflammation attacks the dense substance without involving the outer or inner lining membrane (whose actions are similar), that we will only consider the disease as commencing on the surface and periosteum and in the spongy texture; either may assume the diffuse form, in which the suppurative action has no distinct boundary; or the circumscribed form, in which deposition of new bone takes place around the focus of suppuration. Thus also in soft parts we may have a diffuse suppuration, or a localized one surrounded by hardened tissues which form the walls of the abscess.

When the disease attacks the periosteum and surface, the first effect visible to the eye is swelling and redness of the membrane, and slight redness also of the underlying bone; very soon the fibrous tissue is separated from the osseous surface by a soft but solid growth from this latter, and afterwards by pus. If we have an opportunity of examining the condition of parts in a not far advanced stage of the disease, we shall find the periosteum soft, thickened, and sodden; it can be very readily stripped away, and in leaving the bone, it does not merely drag after it thin fibres (capillaries), as in a healthy condition, but thick soft plugs and ridges, which as they come out of the bone, leave it marked by deep holes and grooves, giving it the appearance of being worm-eaten. Often a few thin flakes of porous softened or blackened friable bone adhere to the periosteum and come away with it. The bared surface is soft and yields to pressure with the finger, or may readily be cut with a knife. From this outer point of commencement the action spreads inwards; hyperæmia in the dense substance of bone only slightly heightens its ordinary pinkish tinge, which, however, increases as the tissue softens, as is evident if the resultant pus be wiped away. In circumscribed caries, the osseous tissue beyond the focus of inflammation becomes indurated and white, while on the surface new bone is deposited beneath the periosteum in an annular form.

When inflammation begins in the spongy ends of long bones, or in the substance of such as form the carpus or tarsus, the redness is much more marked than it ever becomes in the dense tissue; it is chiefly produced by hyperæmia of the cancellar vessels; but is much increased by effusion of a pinkish (blood-stained) serum; in spots where the hyperæmia is greatest, actual extravasations occur. Soon afterwards it is found, on sawing through the bones, that the redness is concealed by thick pus, which oozing from the cancelli, gives the section a dirty yellow appearance; a stream of water directed upon the cut surface, washing away the pus, restores the red tint, and shows the cancellous cavities to be filled with a pink membranous-looking substance—granulation tissue. The bone in the seat of suppuration soon becomes soft, and may be broken down or compressed by the finger; in parts it is quite tough and flexible, in others brittle and pulverulent; in some spots only a cretaceous material soaked in pus is left behind. In the circumscribed (the most usual) form, thickening and induration of bone will be found around the focus of suppuration; deposit of new bone even takes place on the surface; and thus while loss of substance occurs within, increase in size takes place without. Sometimes the whole tissue included within the induration disappears, and is replaced by pus; thus is formed an abscess whose cavity appears hollowed in condensed bone, and is lined by a soft membrane.

Such are, very briefly stated, the gross appearances and results of osteitis, but by merely observing and describing these phenomena we do not approach nearer to a comprehension of the actions causing them. The study of all natural processes divides itself into two parts—viz., observations of the changes produced, and investigation into the means whereby they are produced. The former of these is by far the easier, hence all natural sciences commence by simple observation and classification of such changes; subsequently, when under certain conditions a number of phenomena have been observed constantly to concur, they are ranged together under one name, which at first intended simply to denote the series of phenomena, is used at length to connote their cause. Few habits, perhaps, have been so detrimental to true knowledge as this misuse of collective terms—a misuse which, if carried throughout, would verify the Frenchman's reproach, that "*La science n'est qu'une langue bien pendue.*"

The word inflammation has been used from remote antiquity with more or less belief in its explanatory power; we can in elucidation of the term recite the various phenomena, which it is intended collectively to denote; but do we really know in what the action consists? When certain changes are referred to inflammation, are we by the application of the term much nearer to a comprehension of the morbid action?

In a paper, "On the Nutrition and Inflammation of Cartilages," which appeared in the last October number of this periodical, I showed, that most ulcerations in cartilage are produced by a series of active changes in the cells of that structure, and affirmed that these changes were essentially inflammatory; nay, I even ventured to assert that "*this increased action occurring in cartilage should serve as the type of the inflammatory process in its simplest form.*" The action of blood-vessels in inflammation was described as secondary; they are simply carriers of a greater quantity of pabulum to tissues making increased demands. But in cartilage there are no bloodvessels to obscure the primary acts of inflammation, hence in that structure we may study, under conditions as little complicated as possible, the naked form of inflammation in connective tissues. These tissues (Bindegewebe of Donders) constitute a large class — viz., areolar, fibrous (ligament, tendon, fascia), cartilaginous and osseous tissues—which has no special function, and in whose nutrition the cells play a very considerable part, and therefore are the most active agents in its diseases.*

The point which may, I believe, be best fixed upon as histologically distinguishing the connective from the special tissues (nerve, muscle, glands, &c.†), may be thus stated. In these latter (special), the material which gives to the tissue a special functional value is collected in the cells of the tissue, or at least in the tubular remains of cell walls. Connective tissues have no special function, but are enabled to perform the duty of binding parts together, resisting pressure, or giving support, by virtue of certain qualities of elasticity, toughness, or hardness, which are bestowed upon the structures, not by any peculiarity of the cell contents, but of the intercellular substance. Thus the cells of the special tissues have taken into themselves a material which enables them to perform a special function, and by so doing have lost their formative power; hence these tissues are never reproduced after injury; moreover, they possess no intercellular but only intracellular substance, therefore are only capable of self-nutrition, and not of supporting or governing a certain district around themselves. The cells of the connective tissue, on the other hand, have not fixed in themselves any special material, nor assumed any special function, but retain their common fluid contents and their common formative function, being capable of producing, under certain circumstances, new cells and new germs of cells, and thereby of repairing loss of tissue, filling up the breach with a like material. Moreover, these cells do not lie in close contact, but are separated from each other by intercellular substance; and each cell has the duty of nourishing its own district of this material. To this action I would limit the term nutritive cell-function, and would not extend it to mere power of self-support. Hence the nutritive and formative power resides in the cells of

* Prof. Virchow has carried investigations further than other authorities into what may be styled the cell-constitution of tissues, and their action under disease; he has, indeed, founded a new school of pathology under the name of Cellular Pathology. It is only after much distrust of myself, and great care in my examinations of diseased tissues, that I am led on several important points to differ from so renowned an investigator and author, but am obliged by many cogent reasons to do so. It would be out of place to describe here the many points and grounds of disagreement; it will suffice to say, on the present occasion, that he appears to have passed over the manifestly various actions which different tissues undergo during many processes—that of repair, for instance—and to have missed thereby a key to many most important morbid conditions, and he also has ascribed to the cells of tissues three powers (functional, formative, and nutritive), which, as will be immediately shown, are in their nature incompatible with each other. Let me, however, remark at the same time, that no one can have greater reverence for the many incontestable services, he has rendered to pathological research.

† Virchow divides the tissues into connective, special, and epithelial; among the last he includes glands.

connective tissues only being incompatible with the absorption of any special material.* The difference, however, between the merely nutritive and formative act is so slight, that, as in vegetable cells, a small increase of stimulus or of supply will change the common nutritive into a formative function, producing new cells and new germs of cells. This is that which we see takes place in cartilaginous inflammation.

Bone is a substance, which once was cartilage and has been changed, not by the subtraction of animal matter, but by the addition of lime. It belongs, therefore, to the connective tissues; and if there be any truth in the assertion, that the "cell action taking place in cartilage should serve for a type of the inflammtory process in its simplest form," similar action ought to take place in the cells of bone. We may therefore advantageously compare or contrast the actions, under inflammation, of bone and cartilage,—that is, those of a so-called vascular with those of a so-called non-vascular tissue. Impressed with the idea of this comparison, I examined with care every specimen I could obtain of carious bone; and as I found changes, which were difficult to elucidate unless it were possible to obtain a piece of bone from the same case at regular periods of the disease, I was led to make the experiments below detailed. It will be desirable, however, before describing them and the pathological specimens, which caused their institution, that the idea of reader and writer concerning the structure of osseous tissue should accord.

Bone is generally described as a compound of cartilage and phosphate of lime, plentifully supplied with vessels, among which a large number of branched cells are arranged in more or less definite order. Let us describe the structure in the same language differently placed, and say bone consists of a number of branched cells, whose interstices (intercellular spaces) are occupied by a compound of cartilage and phosphate of lime, and among which vesssels pass in a certain definite relation. The bone cells are placed in the lacunæ, the wall of the cell probably lines the space, and, under favourable circumstances, the nucleus is plainly visible. From the lacuna certain minute chasms in the bone run in all directions; they are called canaliculi. As far as the hard, osseous, intercellular material is concerned, they certainly are tubes; but that the space is lined by a tubular membrane is, I think, very doubtful; it appears to me rather filled with a bundle of fibrillæ, which formation would equally permit the transmission of fluid. The district of intercellular substance belonging to each cell is marked out by the limits of the canaliculi branching from that body. The cells are seen on transverse section usually to surround certain vessels in bony canals called Haversian. The whole arrangement, vessel, canal, and cells, is called an "Haversian System." Certain parts which fill up the spaces between these systems are called by Kölliker, "interstitial laminæ," but by Queckett are better termed "Haversian interspaces." In a long bone these constituents are enclosed on the periosteal and medullary surface by layers called circumferential laminæ,

* For a fuller account of this doctrine the reader is referred to my paper in Beale's Archives of Medicine, on Granulation, &c., Vol. II., Part I.

in which the cells run circularly round the bone. Among these are some very long and narrow cells,* and in a longitudinal section this same form of cell will be seen underlying the surface and running parallel to the axis of the bone. Thus there are two sets of lengthened bone-cells—one which runs parallel, one at right angles to the long axis. They are never found among the Haversian systems and interspaces. The ordinary cells appear on transverse section as narrow slits in the tissue (those of the Haversian interspaces are a little broader); on longitudinal section, they present a longer and an oval appearance; hence it is evident that they are ovals, flattened from the sides, doubtless by pressure. The lacunæ of the spongy texture, particularly where they do not immediately surround a cavity, are larger and fuller than in the hard texture. Although each cancellous cavity takes the place histologically (in regard to the position of cells and to the nutritive function) of an Haversian canal, yet many of the larger laminæ are perforated by such a channel.† The specimens of diseased bone-tissue which I examined all belonged to cases of caries and necrosis of different forms. But in these diseases we have generally three actions going on at the same time—viz., hardening, softening, and sloughing of bone-tissue; therefore I wished to find the earliest condition of inflammation of a simple nature.

On Tuesday, January 17th, 1860, I procured three adult rabbits, and broke the right tibia of each a little below the middle. The bones were, notwithstanding their small size, very strong, and broke with a snap; the animals gave hardly any sign of pain, and returned to their food as soon as released.

It was my intention originally to have one of these animals killed every second day, and to examine the condition of bone-tissue in the neighbourhood of the fracture; but engagements interfered, and the first was killed on the second, the next on the seventh, the last on the tenth day.

The fractured ends in all had ridden to a considerable extent. In the first they had pierced the gastrocnemius muscle; the soft parts around were covered with a delicate film or bloom, which on microscopic examination was found to consist of the cells, nuclei, &c., of granulations; a like tissue filled the medullary cavity. The broken

* See Tomes and De Morgan on the Development and Structure of Bone: Philosophical Transactions, 1853.

† The measurements of the lacunæ may be thus stated:

Transverse view:—	
Cells of Haversian systems; Ordinary cells of circumferential laminæ	1-20 to 1-100 of a line in length.
Cells of Haversian interspaces	Same length, but broader.
Long cells in circumferential laminæ	1-50 to 1-40 of a line in length.
Longitudinal view:—	
Oval cells of compact bone	1-100 of a line in length, by 1-400 in breadth.
Round	1-300 of a line in diameter.
Spongy Texture:—	
Oval cells	1-80 of line in length, by 1-300 breadth.
Round cells	1-250 of a line in diameter.

bones in each of the other two animals were surrounded to the extent of the riding by a bag, the outer limits of which adhered to the surrounding muscles and fasciæ, and through which two tendons passed. The outer part of this sac was a dusky pink, the inner portions, a little paler in colour, were harder and elastic. This bag seemed to arise from each fragment just beyond the spot, to which the ridden end reached; and where it was thus attached to the bone, the inner layer was to some extent ossified.* In each of these three rabbits many sections were made through different parts of the fractured bone, and from the sound tibia sections were taken as similar as possible to those removed from the broken one.

The normal bone tissue of the rabbit might be described as that of man in miniature; the arrangement of the solid tissue around a medullary cavity, of the cells on both inner and outer surface, and the circular shape of the Haversian systems, are all similar, but on a very much smaller scale; the lacunæ measuring only from the 275th to the 350th of a line, or even smaller; the canaliculi, which in the human being are sufficiently clear and marked under a power of 250 or 300 linear, scarcely show in the rabbit under the same glass, except as a fine reticulation of the intercellular substance. This appearance is very difficult to render, but the annexed figure gives a very fair view of the transverse section of the long bone of a rabbit. It is taken from the lower half of the tibia, near the external surface; but I feel that the canaliculi should have been more minutely and delicately rendered.

Fig. 1. Transverse section of normal tibia of rabbit. Mag. 360 diam.

The sections from the fragments of bone in the three rabbits were made—some transverse, some longitudinal, and at various different distances from the seat of injury—for instance, in the first, the transverse sections were made from the upper piece; one at the seat of fracture, one half an inch, a third an inch, a fourth an inch and a half beyond. The lower piece was used for longitudinal sections. Of the transverse, the two last are normal, but the first and second show in different degrees an unusual size and crowding together of the lacunæ, chiefly among those of the Haversian system, but also among those of circumferential laminæ.

* The remarkable mode in which the granulation tissue on the outside of this bag, changed gradually in its inner parts to a membraniform cartilage very like that of the mouse's ear, and the process of ossification in this latter tissue, were most evident and beautiful, but their description must be postponed to some future opportunity; the present paper being intended simply to describe the cell changes in osteitis, must not exceed its prescribed limits.

In the second and third rabbit are to be noted that the ridden portions of the broken bones were enclosed in a bag, which sprung from each piece, beyond the spot where the fellow fragment could reach; and that at the place of origin, ossification had commenced, so that each portion was at this spot surrounded by a ring of new osseous matter. Sections in any part of the ridden end showed the same changes as above in the lacunæ, but somewhat more advanced; the canaliculi also were more evident. Sections through the part surrounded by the bony ring (which was thickest at the side where it was irritated by the end of the fellow fragment) showed, that parts of the bone—those next the greatest development of new osseous tissue—were denser, whiter, and more opaque than normal; this could be seen as soon as the bone was sawn through: when the section was completed and placed under the microscope, this part of the bone was found most changed. The change consisted in enlargement of the lacunæ, chiefly in their transverse direction; that is, they became much rounder; in many instances, nearly circular, and seemed much more crowded together. This enlargement was most marked in the lacunæ of the Haversian systems. Moreover, the canaliculi were much enlarged, more pronounced, and indeed seem increased in number; the bone, instead of being merely marked by fine reticulations, as in the normal condition, was considerably engraved by very evident dark lines. Great care was of course taken to reduce the bone to the same tenuity as the normal section. In passing the object under the microscope field, the change from the less to the more altered parts was very observable. The same change had affected the canaliculi of the circumferential laminæ. The cells of this portion were less rounded than those just described, but they had increased in breadth, as also in length; so that in some, the ends of the lacunæ are united, or nearly so, forming long continuous lines underlying the bony circumference; the lengthened cells described at p. 494, had also increased considerably in breadth. It is probably owing to this circumstance that the outer layers of any thin section of bone in this condition are ex-

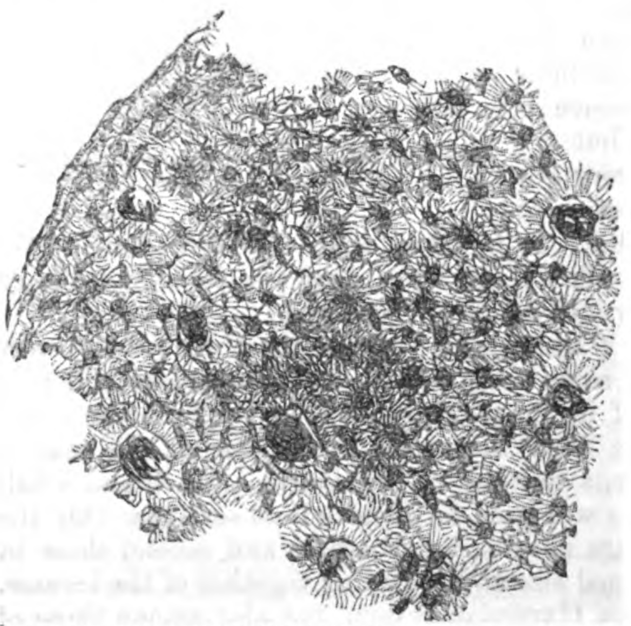


Fig. 2. Section of tibia of rabbit through a part which was inflamed. The section is taken from a corresponding spot of the other tibia of the same rabbit as Fig. 1. Mag. 360 diam.

tremely difficult to preserve: they are very apt to break away and disperse themselves into the fluid or Canada balsam, in fine fragments or even powder.

The measurements of the enlarged lacunæ are as follows: enlarged lacunæ of Haversian systems, about the 180th of a line in diameter; enlarged lacunæ of circumferential laminæ, about the 100th of a line in length.

Pathological specimens usually present to our notice two, or at least three forms of diseased action, and it is difficult to decide, without assistance from analogy, what is the early condition of inflammation; the observations upon rabbits were therefore devised to afford a certain known standard as the commencement of the inflammatory process, and have fulfilled that object. Caries generally presents two conditions: the actually suppurating bone, and the surrounding induration, whether in the dense bone tissue, or in the spongy texture. The specimens examined were very numerous, belonging to several different classes of disease.* Suppuration of bone commences like that in soft parts, by inflammation, and unless the constitution be so debilitated as to give rise to a diffuse form of the disease, the first effects of the attack will be hardening of the bone tissue. In due course, the part thus indurated will soften and suppurate, while the disease, still spreading, will produce hardening all around. Hence, if a circumscribed caries be examined, there will be found different parts in every different degree of inflammation, from the commencement in incipient induration to its termination in purulent softening.

Inflammation, producing simple induration, commences by some enlargement of the lacunæ; those of the Haversian systems, which, on transverse section, appear normally long and narrow, assume under the disease an oval shape; they remain dark, except in rare instances. In the spongy texture, in which the lacunæ are ordinarily larger and more broadly oval than in the solid substance, the increase is not so marked, but is not less real. The appearances of the sections show, therefore, an unusual crowding together of the bone cells; but the observer will be principally struck by the increased development of the canaliculi: these channels are not only more plainly marked, but are more numerous than natural; so much so that the bone, except for the lacunæ, assumes almost the appearance of dentine. The portion of tissue thus affected may be detected by the naked eye; if a section simply filed tolerably smooth, or rubbed on a stone, without being ground thin, present any white, opaque portion, the canaliculi will in those parts be certainly thus affected. It has not been thought necessary to give a cut of this appearance in solid bone tissue. Fig. 2, representing the rabbit's bone inflamed, looks very nearly like human bone in the same state under less magnifying power.

Passing onwards to the focus of the inflammation where suppura-

* Among these were some of strumous, rheumatic, and syphilitic caries, chronic rheumatic arthritis, &c.; there are remarkable differences, not so much in the process itself as in the results of different forms of inflammation, but at present we have simply to do with the inflammatory action as a whole.

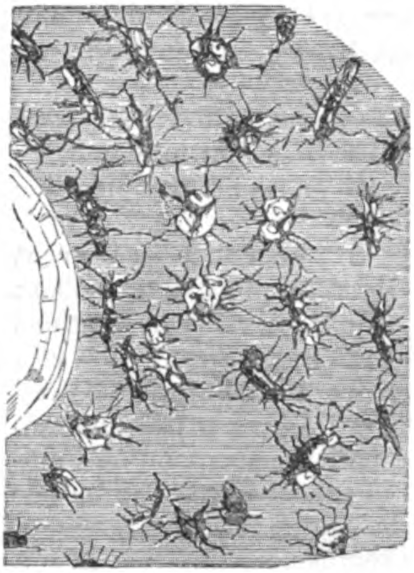


Fig. 3 represents a lamina taken out of the spongy portion at the upper end of a human tibia, normal. The light semicircular portion at the side represents the edge of a cancellus. The longest lacuna is 1-90th of a line in length. The largest oval has a longitudinal diameter of 1-150th of a line.

tion is taking place, a further series of changes becomes apparent. The lacunæ have increased still more in size and breadth, even those of the Haversian systems are very broad ovals or are rudely circular; their interior instead of remaining dark has, as it were, opened out into a light space, marked by light-coloured round spots surrounded by dark lines, or *vice versâ*, according to the focus and direction of the light; some of them are very granular, others more rare are crowded with round, cell-like bodies, forming a mulberry mass, which appears to stand out above the bone surface. The canaliculi remaining large in number, have increased in size chiefly at their commencement at the lacuna, so that they appear to open into that space by a broad mouth like an estuary. They are throughout more marked than the normal tube; they branch also, in many instances, into three or four channels, and sometimes at the spot whence these branches diverge, a considerable enlargement in the main trunk is perceptible, as though at that point a new lacuna were being formed.

During these changes in the appearance of the cells and their branches, the intercellular substance begins to suffer a peculiar transformation, which commences first in the parts next the Haversian canal, or cancellus, as the case may be; the bony substance becomes perfectly granular, that is to say, it looks as though it were composed of dark and light coloured dots placed close together. As this change spreads from the Haversian canal, or cancellus, outward, the margins of the cavity lose their distinctness of outline and become very irregular; in parts the edge is gone, the cavity is therefore on that side increased, in other parts the spotted bone tissue appears to mingle or to be continuous with some granular contents of the cavity. It is quite evident, that in these places the bone-tissue is softened; one can trace the gradual completion of the process from some spot, which is only slightly spotted, on to the part next the cavity, which is a mere pultaceous, granular mass, in which many of the dots have the appearance of nuclei. (Fig. 4.)

Another change in the cell forms part of this softening process—viz., that as the dotted or granular condition reaches a certain stage, so do the canaliculi disappear, and therefore, of course, from that side first which is turned towards the cavity (Haversian or cancellar); they vanish by simple shortening, by recession from the entirely softened bone, until they are reduced to mere little rudimentary projections on the surface of the cell. At this time the cell itself is visible as a granulated dark bag, more or less transparent and very highly refracting, which projects from the wall of the scarcely resistent bone, and is of large size; it bulges out and seems swollen, projects more and more, at last breaks away from its attachment, and lies among the softened *débris* in the cavity, still retaining its dark colour. In breaking away, however, it often leaves behind those of its canaliculi which were turned away from the cavity, which may often be seen on the edge, but which soon disappear, as softening goes on spreading outwards. Frequently several smaller cells come out of the lacuna instead of one large one. In this way a lamina between two cancellous cavities very soon disappears from softening on both sides; in this way also, circlet after circlet of cells, around an Haversian canal, cave into the cavity, and thus the system melts away and leaves around the vessel only a soft granular and cellular mass.

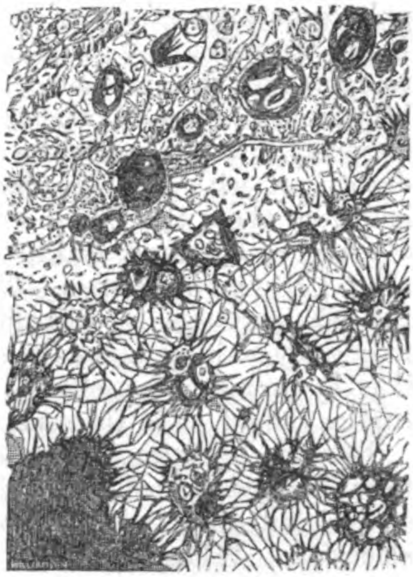


Fig. 4 represents a lamina taken out of the spongy portion of the upper end of a human tibia in a carious condition. The upper portion of the cut shows a mere pultaceous mass into which the bone has become converted, and in which dark cells from the bone are scattered. In the lacunæ many nucleated cells are frequently to be seen. One in the lower right corner is conspicuous for its size and appearance, it measures 1-85th of a line in diameter.

That portion of osseous tissue which lines within and without the shaft of a long bone, may be regarded as having the same relations, in the one case to the periosteum, in the other to the medullary membrane, as the Haversian system has to the canal, or as the laminæ bear to the cancelli. The ordinary cells of this tissue have certainly this relation, but those very long cells already described as peculiar to this situation are not so analogous. These cells appear destined to aid quickly and uniformly in the circumferential growth, and therefore also in the internal absorption of the bone; their action under inflammation is so rapid, that unless by experiment one has hardly any chance of detecting their agency, for as soon as disease commences at the outer layers of the bone, they begin to swell in thickness, loosening thin flakes of the bone even before it is softened, so that in stripping off the periosteum, however gently, such pieces will (if the attack be sufficiently recent) remain adherent to the membrane. If, however, the loosened flakes be not disturbed, they soften with great rapidity, and add their quota to the thickened state of the periosteum.

It was said, page 490, that if the periosteum be stripped off a bone inflamed at the surface, "it does not merely drag after it thin fibres (capillaries), but thick soft plugs and ridges, which, as they come out of the bone, leave it marked by deep holes and grooves, giving it the appearance of being worm-eaten." If one or more of these plugs be examined, it will be found to consist of a vessel, surrounded by a pultaceous mass of granules, among which are some scattered cells. If the worm-bitten-looking holes be studied, they will each be found to represent the absence of an Haversian system, and the parts left between them the Haversian interspaces. Some of these holes run obliquely into the substance of the bone, and from these the plug is drawn out with the periosteum, others run along the new surface, and from these the ridges come; thus it is evident that the pultaceous granular mass which surrounds the vessel is a molten and altered Haversian system. It does not always happen, however, that the whole system is thus dissolved and comes out with the vessel, frequently only its inner layers are sufficiently softened to do so, and it is by no means necessary that every Haversian canal on the new surface should be in the same state of advanced softening.

Necrosis presents to our consideration three conditions of osseous tissue—necrosis, caries, and induration; the two last have been described, but the relative positions in which the three occur must claim attention. When a portion of bone dies, ulceration must separate the dead mass. This action does not take place immediately on the surface of the necrosis, but a little beyond it. In soft parts the slough becomes separated by ulceration, which occurs on both sides of the demarcating line, at the edge of the sloughed portion, as well as at the edge of the part to be preserved—that is to say, that a certain portion of the tissues separated is not dead, since on its surface ulceration and granulation occur. Thus also in osseous tissue the ulcerating action continues in still living bone on the surface of the dead material; the inter-relation of diseased parts proceeding from the healthy to the necrosed portion may be thus stated: healthy, indurated, ulcerated, indurated-necrosed, the two last together constitute the mass separated. Hence, in all sequestra are two portions, the actually necrosed, lined by indurated but living bone. If the slough have occurred in the centre of a bone, it is surrounded by the hardened tissue; if only on the surface, with death of the periosteum, the dead portion will only be lined by indurated bone on that side which was attached. On sawing through a sequestrum, and rubbing the cut surface smooth on a file or on a stone, the distinction between these two portions will be very evident; the centre or the edge, as the case may be, will appear of a dull leaden grey, surrounded or only lined on one side by white and hard bone, the slough bearing in colour the same relation to the indurated portion, as a piece of note paper on which a drop of oil has fallen does to the clean white surface surrounding the spot. A section ground thin and placed under the microscope presents a similar difference of colour, the light coming through the actually necrosed bone receives a dusky yellow tinge, which is not imparted to it by the hard tissue. The transverse section presents lacunæ not at all enlarged, and void of canaliculi, whose traces appear as slight serrations of the cell's edge; the laminated lines of the Haversian systems are abnormally distinct, each canal being surrounded by circles, comparative to those which in smooth water surround the spot where a stone has been thrown in. These appearances are so peculiar as at once to distinguish the necrosed portion from the rest of the sequestrum, and it is well worthy of remark that at the edge of the slough Haversian systems are frequently to be seen, half of which are necrosed, the other half indurated. The longitudinal section also shows absence of canaliculi, a certain diminution in the size of the lacunæ, many of which lose their distinctness of outline and hollow appearance, looking like drops of oil; the laminated lines are now seen lengthwise, running along the vessels.

The three changes in the bone-cells which have just been described correspond, then, with the three states known as induration, caries, and necrosis. We have followed out the minute anatomy sufficiently closely; let us now consider the *rationale* of the process. It cannot fail to strike the attentive reader, that in the first of these conditions the lacunæ and canaliculi being increased in size, the actual osseous substance must be diminished, and yet the bone is condensed, a combination which at first sight seems impossible. But if the function of the bone-cells and their branches be considered, this apparent discrepancy becomes not only reconciled, but the interdependence of the two processes will be found necessary. The lacunæ and canaliculi being the nutrient portions of the bone, it follows that their assumption of more active performance would be followed by increased nutrition of the parts they supply; hence, too, increased condensation of those parts, to greater hardness of the bone. This condition has its physiological analogies, for not only is dentine more tubular than bone, but where a hard condition is necessary, there do we find a more complete tubular arrangement of elements; the hardest ivory is most closely permeated by tubes. The external shell of certain crustaceæ, as the crab and lobster, becomes tubular instead of cellular in the hard tooth-like projections on the inside of the claw. In fact, this commencement of the inflammatory process is in bone, as in other parts, increased nutrition, and in its least marked form is hardly, if at all, distinguishable from the condition of growth in the bones of very young animals; it is simply a very active condition of the cells of the structure. If the inflammation subside, the lacunæ gradually resume their narrow chink-like shape; but the great plenty of long, almost straight, and strongly-marked canaliculi remain, at least for a lengthened period; whether as the texture becomes more and more normal these tubes resume their usual appearance I am not able to say. In a section through a humerus which I took from the dissecting-room, and thus examined on account of the apparent weight and density of the bone still present, no signs of active inflammation could be discovered, but the canaliculi have this intensity of development.

If this nutritive activity of the cells, which constitutes an indurating inflammation, be increased to a formative action, so that they not only grow still further in size, but actually multiply within the lacunæ, then follow absorption and softening of the intercellular osseous substance to support this increased cell-growth, and ultimate discharge of the cells from the lacunæ into the softened mass.*

* I have deferred to the present time giving an account of Prof. Virchow's views of osteitis and his mode of investigation. He affirms that all the researches must be made upon fresh pieces of bone, that dried portions, and pieces ground thin, lose most signs of change; and he goes on to say: "I have either broken little lamellæ out of the inflamed

A necrosed portion of bone acting like a foreign body becomes enveloped in condensed osseous tissue, just as a bullet or other foreign matter in soft parts becomes enclosed in a fibrinous bag. Soon afterwards this tissue ulcerates in a line of demarcation in the midst of the induration, so that the slough, even when separated, remains covered in by some indurated bone-tissue. The caries is accompanied by the formation of granulations and of pus from the osseous tissue itself, by excessive generation of the bone-cells, in the same way as these constituents arise from inflammation of all connective tissues. When pus from bone is put under the microscope, it is seen to contain minute lime particles; moreover, my friend Mr. Tuson, Professor of Chemistry at the Charing Cross Medical School, was kind enough to test for me some such matter by mixing the pus thoroughly with distilled water and filtering the liquor, when he found in it distinct evidence of phosphoric acid and of lime, showing that the pus contains bone material in solution.

Thus, then, in osseous tissues we find, that inflammation consists of precisely similar actions to those which constitute that abnormal state in cartilages—namely, a superabundant growth of the cells of the tissue, which, destroying the intercellular substance, become converted into granulation or pus cells, or may, by becoming fatty and losing quickly their nutritive action, cause the integral death of the dependent tissue. The actions, then, of a vascular and of a non-vascular tissue under inflammation are essentially the same, and it is singular to observe, as was pointed out at p. 501, that half a district supplied by one capillary may be in an early stage of inflammation—viz., induration; while the other half shall be dead, necrosed, showing how very little in reality the vascular supply has to do with the inflammatory condition. Therefore we may return to the previously quoted assertion, "that the increased action occurring in cartilage may serve as the type of the inflammatory process in its simplest form," with greatly increased confidence, since we find that in a part containing vessels precisely similar actions constitute its inflammation.

portion, placed them as a whole under the microscope, and then quickly deprived them of their earthy matters by means of concentrated hydrochloric acid, or, what is often easier, have cut off thin slices with a sharp knife, or, lastly, have put pieces into concentrated hydrochloric acid, and then, from the softened mass, cut off little shreds with scissors, or torn them off with needles." (Ueber Parenchymatöse Entzündung: Virchow's Archiv, Band iv. Heft 3, p. 304.) After this proceeding he finds that the first change is a fatty degeneration, the second is that some of the lacunæ are slightly enlarged, a few bi-nucleated, the third is softening of the bone tissue. I have not only tried these methods of Virchow's, but have compared lamellæ taken from inflamed parts, and simply mounted with others taken from the same parts and previously ground, and have failed to see in what manner grinding and polishing, if properly done, injures the specimens; while I am quite sure that the application of an acid, particularly of a strong acid, not only totally destroys all satisfactory view of that interesting portion where the bone is softening, but also greatly alters and obscures even the hardest parts. It is however certain that the specimen should be fresh. The process of ulceration is precisely similar, whether a portion of bone be necrosed or not; the necrosis consists in fatty degeneration of the bone-cells, which does not, I believe, take place except in, and as a cause of, that malady. I have never found it in caries, except in a cell here and there lying among the most softened parts of the tissues. This degeneration of the cell causes loss of its nutrient power (limiting this term to the power of nourishing a surrounding district), according to the law that a cell, which absorbs a special substance is only capable of self-nutrition, and loses the formative power altogether, hence the canaliculi in necrosis having become useless and shrivel. I have not succeeded in accounting for the increased marking of the laminated structure.

ART. III.

A Glance at the Present State of Ethnology, with Reference to the Form of the Skull. Read at the Seventh Meeting of the Scandinavian Association of Naturalists, held at Christiania in 1856.* By ANDERS RETZIUS, Professor in the Carolinean Institution at Stockholm. Translated by WILLIAM DANIEL MOORE, M.B., M.R.I.A., Honorary Member of the Swedish Society of Physicians and of the Norwegian Medical Society.

WHEN, twelve years ago, a paper was read before this distinguished Association, "On the Form of the Skull in Different Nations," based upon the principles I had put forward two years previously at the meeting of the Scandinavian Association of Naturalists at Stockholm, the science was entirely new and untried, with a still uncertain future and with great voids to fill. Since that time, the classification of forms then assumed has gained both in stability and in extent, and it is to give a brief report of this progress that I have ventured to claim for an hour the attention of this distinguished meeting.

A. EUROPEAN FORMS OF THE SKULL.

On the occasion already alluded to, I showed that the majority of the people of Western Europe were dolichocephalic,† while the brachycephalic was the prevalent form of the skull throughout the great extent of Eastern Europe. I have since from many quarters received confirmation of this opinion.

European Dolichocephali.

Germans	Norwegians and Normans in France and England.	Orthognathic.‡
	Swedes.	
	Danes.	
	Dutchmen.	
	Flemings.	
	Burgundians.	
	Germans of the Germanic Stock.	
	Franks.	
	Anglo-Saxons.	
	Goths in Italy and Spain.	
Celts	Celtic Scotch.	
	,, Irish.	
	,, English.	
	Welsh.	
	Gauls in France, Switzerland, Germany, &c.	
	The Proper Romans.	
	The Ancient Greeks and their Descendants.	

Since I communicated the arrangement which will be found in the

* The copy of the above essay from which I make this translation has been kindly sent me by Prof. Retzius, and possesses the great advantage of recent manuscript corrections and additions by its learned author.—W. D. M.

† From δολιχός, long, and κεφαλή, head. Nations where the development of the skull is in the occipito-frontal diameter are called dolichocephalic; those where it is in the parietal diameter are called brachycephalic, from βραχύς, short, and κεφαλή. See Latham On the Natural History of the Varieties of Man, p. 4.—W. D. M.

‡ From ὀρθός, upright, and γνάθος, jaw.

Transactions of the former meeting at Christiania, I have examined a considerable number of individuals sprung from Norman families in France and England. Without exception, these individuals had preserved the same oval form of the skull which belongs to the true Normans in Norway.

I have also examined Swedish skulls in hundreds of instances, both from old graves and churchyards and in the dissecting-room, and have likewise found the form already described predominant.*

At the levelling of Riddarholm, some years ago, an entire churchyard was met with, from which skulls and remnants of skeletons were dug up, many of which were in a state of perfect preservation. All the skulls, almost without exception, bore the Germanic type. The same was the case in a burying-ground in the city, in the so-called *själagardsgatan*, where the churchyard of a monastery had existed.

Since the time already mentioned I have visited Copenhagen, have seen a number of skulls in the museums there, and have also had the opportunity of observing the form of the skulls of a great many Danes, and found that they had perfectly preserved their Germanic dolichocephalic form. The same I found to be the case in Holland and in Belgium and French Flanders; I had previously received from Professor Vrolik, in Amsterdam, several Dutch skulls of the same form, from old graves.

During a journey in Great Britain in 1855, I had again the opportunity of satisfying myself as to the general prevalence of the dolichocephalic form, both in England and Wales Proper, and in Ireland and Scotland. The majority of these dolichocephalic individuals are black-haired, and are probably Celts.

Through the kindness of the distinguished and zealous archæologist, Frederick Troyon, I have obtained for the museum in Stockholm several skulls of Burgundians, taken up by Hr. Troyon from old Burgundian burying-places in the canton of Vaud. They all present the Germanic form.

The first Roman skull I had the opportunity of seeing was sent to me by the late Dr. Prichard. This skull was taken on a field of battle (the camp of the Emperor Severus) near York, with another skull of different shape. Dr. Prichard requested my opinion as to the nationality of these two skulls, without giving the slightest hint for my guidance in the matter. I found that the first-mentioned skull possessed a quite peculiar dolichocephalic shape, not previously represented among the European skulls in the collection of the Carolinean Institution. On the other hand, I found that it answered remarkably to the descriptions and figures given by Blumenbach and Sandifort of Roman skulls. The second skull was smaller, of the very long, slender, and low kind, and was evidently that of a Celt. My opinion, therefore, was, that one was the skull of a Roman, the other of a Celt. This decision pleased Prichard greatly, because, as he said, both skulls were found on a field near York, formerly called the field of the Emperor Severus, in which place the Celts (Belgæ Britannorum) were defeated

* On the Form of the Skulls of the Northerns, by A. Retzius: Transactions of the Scandinavian Association of Naturalists at the meeting held in Stockholm in 1842.

by the Romans. The Celtic skull has, moreover, the mark of a death-stroke in the back of the neck, probably inflicted during flight, while the wound in the Roman skull is anteriorly through the orbits.*

Since that period, several authentic Roman skulls have been discovered and investigated by Drs. J. Barnard, Davis, and Thurnam. Some of these were exhibited at the meeting of the British Association for the Advancement of Science in Glasgow in 1855, and a very perfect Roman skull from a columbarium near the Via Appia at Rome has been presented by Dr. Davis to the museum of the Carolinean Institution in Stockholm. All these skulls present a remarkable similarity, both in shape and size. They are of dolichocephalic form, but unusually broad, particularly over the ears, with strongly-marked parietal protuberances and a considerable occipital protuberance, and are, on the whole, tolerably large.

I have also placed the Greeks upon the list of European dolichocephali; the reason for this I explained in the year 1847, in another place.† According to all that I could learn, the dolichocephalic form has never among the Greeks belonged to the majority of the nation, which bears the brachycephalic type. This latter belongs as well to the Greek Slavonians as to the majority of the Levantines and Pelasgi, the Albanians of the present day. In my essay already quoted, I have directed attention to the fact that among ancient statues, Apollo, Venus, and many of the noblest characters, have the dolichocephalic form; while on the contrary, others, as Jupiter and Hercules, have the brachycephalic, probably on account of difference of race in the individuals taken as models by the artist.

To the brachycephali of Europe belong:

Ugrians, (Müller, Latham.)	The Samöeids. „ Laplanders. „ Voguls. „ Ostiaks. „ Permians. „ Votiaks. „ Tcheremiss. „ Morduins. „ Tchuvatch. „ Magyars. „ Finlanders.	Finlanders. Esthonians. Livonians.	Orthognathic.
Turks.			
Slavonians . .	Bohemians (Tsheks). Wends. Slovacks. Morlacks. Croats. Servians. Poles. Russians. Modern Greeks.		Orthognathic.

* A. Retzius: Forncranier fran Britannien; Oefversigt af K. Wet. Akad. förh. 1840, No. 5.

† Ibid., Sept. 8th, 1847.

Lettish or Lithuanians.	Orthognathic.
Albanians.	
Etruscans.	
Tuscans.	
Rhetians.	
Tyrolese.	
Basques.	

Of many of the tribes here enumerated I have not myself had the opportunity of investigating the shape of the skull, but from the information I have received from various quarters, I venture to hold the opinion that it belongs to the brachycephalic class. It appears to be part of the great arrangement of the world, that the predominant tribes in Eastern Europe, among which are included, as is well known, the inhabitants of the extensive country of European Russia and Turkey, as well as of Greece and of a great part of the Austrian empire, are brachycephalic.

Several interesting skulls from the countries here enumerated have of late been obtained for the Museum in Stockholm already named. Thus, I have received from Hyrtl, the distinguished Professor of Anatomy in Vienna, the skull of a Croat from the military frontier, remarkable for its height, large size, and almost cubic shape; a Morlack's skull from Dalmatia, broad, high, and brachycephalic; several Slovackian skulls from Olmütz; two Esthonian, a Turkish, and several Finnish, from Professor Bonsdorff; and two Karelinian, from Professor Willebrand, in Helsingfors. Of Rhetians, I have had occasion to examine several living individuals; I have also examined many Basques, and have obtained valuable specimens of such skulls for the Museum, from Dr. Eugene Robert, of Paris. Sometimes I have met with brachycephalic Scots from the northern Scottish isles, and from the north of Scotland. During my last visit to Scotland, I again met with some individuals of the same type. They have a peculiar expression; not unfrequently, they have a short, rather broad face, red hair, and a somewhat freckled skin. I have subsequently heard from travellers that this type is not unusual in the Highlands, and that it is of remote antiquity. I imagine that these people are descended either from the Fins or from the Basques.*

B. Asiatic Forms of the Skull.

Asiatic Dolichocephali.

Hindus,	Orthognathic.
Arian Persians,	
Arabs,	
Jews,	
Tungusians,	Prognathic.†
Chinese,	

* Since the above was written, the author has again visited the South of Germany, Switzerland, and France, and has found that the Tyrolese are almost, without exception, brachycephalic. The same is also the case in the Italian and German cantons in Switzerland. He has also found the brachycephalic form predominant in Bavaria, Baden, and Würtemberg. He believes that these people are derived from the Etruscans.—MS. note of the author.

† From πρὸ, forwards, and γνάθος, jaw.

The area inhabited by these people is therefore limited to the southern parts of the great Asiatic continent—namely, Arabia, Persia, Hindostan, and China (in which I do not here include either Mongolia or Chinese Tartary). Both north and south of this area, these countries border on brachycephalic people, as the latter are also almost everywhere scattered among the Asiatic dolichocephalic tribes.

I have here included the Chinese as well as the Tungusians among the dolichocephali. They have by others generally been reckoned with the Mongolidæ. Many and many cranial examinations have, however, confirmed the opinion which I long since put forward, and which is quoted by Latham,* that the proper Chinese have long skulls, with prominent occipital protuberances; but with the latter, they have also considerable parietal tuberosities, in consequence of which the circumference of these skulls approaches rather to an oblong pentagon than to an oval. I have, in fact, obtained several Chinese skulls; some original specimens, some casts, from England (Dr. J. Bd. Davis); from Holland (Prof. v. d. Hoeven); from St. Petersburg (v. Baër); some from the circumnavigators of the world in the frigate *Eugenie* (Andersson, Kinberg, and Ekströmer); all have the same, as it appears to me, characteristic form. As to the Tungusians, I must acknowledge that I have only a single skull for my guidance. It is a plaster cast sent to me in exchange by Professor Purkinje, of Prague. I have every reason to believe that this cast is from the Tungusian skull described and represented by Blumenbach,† of which he says: "facie plana ad arcus zygomaticos latissima, fronte depensa, &c. olfactus officina amplissima, occiput mirum in modum retro eminens ita ut protuberantiæ occipitis externæ distantia a dentibus incisoribus superioribus 9 pollices æquaret." Blumenbach's collection of skulls now belongs to the Physiological Institute in the Göttingen Museum, and is under the care of its eminent director, Professor Rudolf Wagner. The latter has caused an able artist to take casts of several of the most remarkable skulls for presentation to other museums.

A very remarkable similarity exists between this Tungusian skull and the skull of the Esquimaux. The shape of the face is quite the same; the face is flat, of great breadth over the zygomatic tuberosities; the upper jaw is broad and prominent; the arch formed by the alveolar processes and the teeth is very wide, precisely as in the Esquimaux and Greenlanders; they also resemble one another in the capacity, oblong character, and large occipital protuberance of the head. The same characters belong also to a considerable number of the Chinese skulls in our collection, and I therefore believe that in this Tungusian skull, we find a link between the Chinese and Esquimaux form of the skull.

Asiatic Brachycephali.

Ugrians, Samöeids, Yakuts, &c.
Turks.
Circassians, and probably the majority of the numerous tribes in the Caucasus.

* The Natural History of the Varieties of Man, p. 16, "Physical Conformation." London, 1850.

† Decas Collectionis suæ Craniorum diversarum gentium, &c. De secunda, Tab. XVI.

Turkomans. Affghans. Lascars. Tartars and Mandshoo Tartars. Mongolians, both in Asiatic Russia and Mongolia. Malays.	All Prognathic.

The Indian Mongolidæ in Dr. Latham's work on "The Varieties of Man" probably belong also to the same class.

These people occupy the whole of the great Asiatic continent, with the exception only of the above-mentioned dolichocephali in India, Persia, Arabia, China, and a small part of Siberia; but, as has already been observed, there also dwell among these in many places, and in small scattered communities, the brachycephali just now enumerated. In Asia, therefore, as in Europe, the brachycephalic form of skull is the prevailing one; but with this difference—the Asiatic brachycephali are, for the most part, prognathic.

C. Australian Forms of Skull.

Australian Dolichocephali.

Austral Negroes—all prognathic.

Our knowledge of these tribes is still so imperfect, that I do not venture upon any arrangement of names, but confine myself to observing that, partly from the museum of the Carolinean Institute, partly from other collections and many published works, I have satisfied myself that dolichocephalic tribes are met with in almost all the Australian islands. On the continent of Australia Proper, or the so-called New Holland, as well as in Van Diemen's Land, all the savage tribes appear to be prognathic dolichocephali; in the other islands, brachycephali also occur (Malays, Polynesians, and Papuans: Quoy and Gaimard). In most of the islands they are black or blackish, and have therefore been called Austral negroes; and as to the form of the skull, quite resemble negroes. Many tribes have the hair frizzy, but long, as if formed into long tails; in others, the hair is frayed. Our collections contain such skulls from many of the islands of the South Sea and Pacific Ocean; they resemble one another in a remarkable manner. They are in general small, but thick; in this also resembling those of negroes. The skulls of these people are much smaller than those of the Chinese, but have the large parietal tuberosities which seldom occur in negroes; the occipital protuberance is large, and somewhat compressed at the sides. The zygomatic space is not extensive, the nose is not so flat as in the negro, the forehead is small and low. I have recently obtained, through Professor Bonsdorff, of Helsingfors, such skulls from Oahu island, in the Sandwich group. The royal Danish frigate *Galathea* brought home several such skulls from the Nicobar islands; Professor Ibsen delivered an interesting lecture on these skulls at the meeting of Scandinavian naturalists in Stockholm in 1851, and had the goodness to present a specimen to our Anatomical Museum.

Through Dr. Robert Gordon Latham our Museum also obtained an extremely valuable skull of a so-called Dyak, from Borneo. This is likewise dolichocephalic. Half of such a skull is preserved in the collection of the University in Christiania, quite similar in shape to several other skulls of this kind which I saw in London. These Dyak skulls are likewise all small, but strongly made; the parietal tuberosities are somewhat less than in the Austral negroes. All the Dyak skulls which I saw were decorated with engraven symmetrical ornaments on the forehead, vertex, and temporal regions, as far as the apex of the lambdoid suture; many parts of the figures are of a dark-brown colour; here and there are small spots with bright colours in blue and red.

Of the Dyaks, Latham says: "Before a youth can marry, he must lay at the feet of his bride elect the head of some one belonging to another tribe, killed by himself. According, then, to theory, every marriage involves a murder. I believe, however, that the practice is less general than the theory demands. Still, a morbid passion for the possession of human heads is a trait of the Dyak character; skulls are the commonest ornaments of a Dyak house, and the possession of them the best *primâ facie* evidence of manly courage;" *loc. cit.*, p. 166. From what I can glean from the statements of many, the Dyaks, as well as the majority of Australians, are black in colour. I believe that all the tribes, which are called Alforas and Haraforas, are prognathic dolichocephali, as are the majority of the generally so-called Papuans,* which, however, ought not to be confounded with the brachycephalic Papuans described by Quoy and Gaimard. Many tribes of these Austral negroes, or so-called Papuans, erect their dwellings on piles over the water. M. Troyon has shown that the ancient inhabitants of Switzerland had similar abodes, as, according to Herodotus (B. 5, ch. xvi.), the Pæonians had in Macedonia. The majority of the Austral negroes live in the interior of the islands; many tribes are mountaineers.

The Brachycephali of Australia consist of:

$$\left.\begin{array}{l}\text{Malays} \ldots\ldots\ldots\ldots \\ \text{Polynesians: Dieffenbach} \ldots\ldots \\ \text{Papuans: Quoy and Gaimard} \ldots\end{array}\right\}\text{All Prognathic.}$$

nations which, in my opinion deserve to be designated by Latham's term, Oceanic Mongolidæ. The well-known Malays, with their yellow skin, black, strong, shining hair, and prominent jaws, belong likewise to the peninsula of Malacca, and are otherwise so well known as the most intelligent and, in their way, cultivated among the genuine inhabitants of the South Sea, that they need not in this brief notice be further mentioned.

Their skulls are seldom wanting in any ethnographical collection. Among the Polynesians I reckon the more bronze-coloured or brownish inhabitants of the Tonga Islands, New Zealand, Otaheite, the Sandwich Islands, and a number of other smaller groups in the Pacific Ocean, belonging to the Micronesian Archipelago. The skulls of the Poly-

* The Ethnological Library, conducted by Edwin Norris, vol. i.

nesians have for the most part still flatter necks than those of the Malays; their jaws and teeth are not so prominent; the skulls themselves are in general larger than those of the Malays Proper. The Polynesians have usually a larger, more beautiful, and more muscular frame of body, and are of better character and more amiable disposition than the Malays. In the ethnological cranial collection of the Royal Carolinean Institution are to be found skulls of Sandwich Islanders and of New Zealanders, which in size, and especially in height, are of the first class.*

Papous: Quoy, Gaimard (Mop-headed Papuas: Dampier).—Dampier, Forrest, and many older travellers mention a peculiar dark-brown coloured people on the shores of the islands near the most northern coast of New Guinea, which people is in many respects distinguished from the other South Sea negroes; and among other points, by their bushy, black and, as it were, frizzled hair. Quoy and Gaimard, who accompanied M. de Freycinet with the corvettes *Uranie* and *Physicienne*, have made us better acquainted with this people, and particularly with their cranial formation.†

The most important point is, as it appears to me, that their skulls are quite unlike those of the Austral Negroes. While the skulls of the latter, as has been mentioned above, are low, narrow, oblong oval, with a prominent occipital protuberance, the skulls of these Papuans are, according to Quoy and Gaimard, high, short, broad, with flatter occiputs. Quoy and Gaimard say of them:

"The Papuan's head exhibits a flattening, both anteriorly and posteriorly, with a strong development of part of the face (the jaws). The skull is of considerable height; the parietal tuberosities are prominent; the temples are very convex; the anterior part of the temples, through which the coronal suture is continued below the linea semicircularis temporum, presents a peculiar and considerable elevation.‡ The nasal bones stand almost perpendicular, as if compressed backwards. The nasal or frontal processes of the superior maxillary bone are broad, and, in consequence of the nature of the nasal bones, pass farther forwards. The superior maxillary bone is much larger than in Europeans, in consequence of the large alveolar process, by reason of which the face of these islanders is of considerable breadth. The anterior nasal opening is inferiorly very wide, even wider than in Negroes. The jaw-bone is at the same time very prominent, and its zygomatic process is larger and more prominent than in the Negroes. The alveolar process is very thick at the sides where the molar teeth are situated. The arch of the palate is broader than it is long; the foramen incisivum is large."

In the Museum of the Carolinean Institute we have four specimens of brachycephalic Papuans; for three I am indebted to the kindness of Dr. Wise, of Edinburgh, who himself brought them over to Europe; the fourth is a plaster cast of one of the specimens brought

* A. Retzius: Cranier fran Sandwichsöarne, &c.; Oefversigt af K. Wet. Acad. förhandl. 1845.

† Observations sur la Constitution Physique des Papous, qui habitent les îles Rawak et Vaïgiou; lues à l'Academie des Sciences de l'Institut, le 5 Mai, 1823: Annales des Sc. Nat., tom. vii

‡ This peculiar elevation I have also found to occur generally in skulls of Malays and Polynesians.

home and delineated by Quoy and Gaimard. All these four skulls exhibit a striking agreement with one another, and with Quoy's and Gaimard's description, which for this reason I feel bound to quote here, the more so as it has also been made use of by other writers, although in one or two places it has been misunderstood.

For my own part, I would briefly say, in reference to these skulls, that they very much resemble those of the Polynesians above-mentioned, and are distinguished from them by the lowness of the dorsum of the nose, the width of the zygomatic arches, the broad nasal opening, and the wide alveolar arch.

Quoy and Gaimard describe these Papuans only from the two islands, Waigiú and Rawak. They state that the inhabitants of these and the adjoining islands call themselves Papua, and distinguish themselves most decidedly from the black inhabitants of New Guinea, who very closely resemble the Negroes of Eastern Africa. They say in one place, that these Papuas live on the coasts, feeding chiefly on fish and shell-fish, and erect their dwellings on poles out of the water. In another place the same writers say:—"The Papuas, which live in the mountain on the island Waigiú, call themselves Alifurus, quoted by other travellers as Alfoirs, Alfurs, Alfurus, Alforeses, and Haraforas. But it would appear as if they knew them only by name. We have thus no proof that they are of the same tribe." In the Museum of the Carolinean Institute are some skulls from the islands in these regions, presented by Dr. Wise, with the inscription "Mountaineers;" and these have the above-mentioned dolichocephalic negro form, are narrow, low, and oblong, with a prominent occipital protuberance.

George Windsor Earl, who has published an interesting work called 'The Native Races of the Indian Archipelago-Papuans,' London, 1853,* quotes an interesting description of probably the same species of Papuans by Lieutenant Bruijn Kops, of the Dutch Royal Navy, drawn up on the expedition sent by the Dutch Government, in 1850, from the Moluccas to the north coast of New Guinea, under the command of Herr van den Dungen Gronovius and Lieutenant Brutel de la Rivière, in the war schooner *Circe*, with some smaller vessels of war belonging to the Dutch vassal, the Sultan of Tidore.† This expedition landed at Dory, on the northern coast; the author calls the inhabitants of the vicinity Dory Papuans. Bruijn Kops describes them as small, of $5\frac{1}{4}$ feet in height (sometimes $5\frac{1}{2}$ feet), of a dark brown colour, occasionally almost black; the hair black, crisp, often very long, sometimes as if it were clipped. He further describes their hair, dress, and features, but so imperfectly that it is impossible to decide with any certainty whether these Papuans belong to the brachycephali just mentioned. I regret to say that I am acquainted with the Dutch work only through Earl's book. Earl gives a drawing representing a Dory-Papuan with his dog in a boat, hunting a boar; he also gives a drawing of the house of a Dory-Papuan, built on piles

* The Ethnological Library, conducted by Edward Norris, vol. i.

† Natuurkundige Tijdschrift voor Nederlandsch Indie for 1851.

in the water. Both these drawings are probably borrowed from Bruijn Kops' account. On the Papuan in the boat the turban-like hair is seen standing out, whence these Papuans have got the name of mop-headed Papuans. I suspect that Prichard's figure of such a Papuan is also borrowed from Bruijn Kops.

Bruijn Kops states that the people of New Guinea divide themselves into Papuans and Alforas, of whom the former inhabit the coasts, the latter the mountains and inland district. But it would appear that he has not more closely investigated the ethnological condition of these people, for which reason we can only receive it as probable that they constitute dissimilar tribes. Lieutenant Bruijn Kops praises the Papuans much, as in reality a good people. Theft is with them a grave and very rare offence. They were for several days on board or alongside the ships of the expedition, without anything having been lost from the vessels. They cultivate respect for age, love of children, and fidelity in the marriage state. Chastity is held in great esteem, and is seldom violated. A man is not permitted to have more than one wife, and to her he is bound for life. Concubinage is not permitted. They are particularly fond of strong drinks, but prepare none themselves, so far as Hr. Bruijn Kops could ascertain. But to steal children and to traffic in them is not disgraceful; the captives so taken are treated well, and are restored for ransom. The slave-trade is general, but the slaves are treated well. The same officer describes the following as their mode of punishing offences. An incendiary is condemned, with his whole family, to be the slaves of the proprietor of the burned house. A man who intentionally wounds another must give a slave as compensation. A thief is obliged to restore what he has stolen, with some addition. If injury is done to a garden or plantation, it must be made good. An adulterer is persecuted to death, or until he has satisfied the offended party by a heavy fine. A man who violates a girl is bound to marry her, and to pay the usual dowry of ten slaves to her parents. In case of illicit intercourse, the woman is not punished, and no infamy attaches to her if unmarried. A slave is the standard of value.

The majority of the Dory Papuans are Pagans, a smaller number are Mahommedans, under priests from Ceram and Tidore. The Pagan's idol, "Karwar," is rudely carved in wood, about eighteen inches high, ill shaped, with a very large head, a long pointed nose, and wide mouth well furnished with teeth. The body is usually clothed with a piece of calico, and the head is covered with a handkerchief. Every household has its image. The image must be present on all important occasions, and is consulted as an oracle. They have also "fetishes," most frequently carved images of serpents and lizards, suspended from the roof, and carved on the door-posts. They have a kind of priests, who are at the same time their physicians and fortune-tellers. Their houses are built out in the sea on piles; the outer walls are of planks. According to the drawing given by Earl, they resemble our larger marine stores, with loophole windows. In the middle is a passage, on each side of which are the apartments. The partition-walls consist of mats, the floor of rough spars laid close together.

These Papuans work in iron and other metals, and practise a limited amount of agriculture, or rather herb-gardening; but no mention is made of rearing domestic animals. Hunting and fishing are the chief occupations of the men; the women manage the household affairs. Both in hunting and in war they use bows and arrows; they do not make use of poisoned arrows. Fish, too, are shot with arrows, speared with lances and lines, and taken in traps.

As the Papuans spend so large a portion of their time on the sea, the canoe constitutes an important part of their property. They have small canoes for children, larger ones for their own daily use, for two rowers, and still larger canoes for twenty rowers. Every such vessel is made of the trunk of a single tree; the large canoes have a mast, with sails of mats. With these imperfect vessels they cannot, however, undertake long voyages, on which account the trade in these islands is in the hands of foreigners, and particularly of the Chinese. In the year 1852, the Dutch Government founded an establishment at Port Humboldt, on the northern coast of New Guinea, whence we may hope to obtain more accurate knowledge of the inhabitants of the country.

I have spoken thus fully of these Papuans on the north coast of New Guinea, because our knowledge of them is still involved in so much obscurity. We see, however, that Herr Bruijn Kops considers them to be quite a distinct race from the Alforas. Although the names Papuan and Alfora, or Alfura, are probably employed without any strict ethnographic precision, it seeems to be generally understood that the term Papuans is applied to the inhabitants of the coast, and the word Alforas to those of the inland and mountain districts. The name Papuan is said to be derived from the Malay denomination for frizzled or woolly hair, "rambut pua pua," whence pua-pua, or papua, has been applied to these inhabitants of the coast with woolly and frizzly hair. Alforas is a Portuguese word, properly signifying emancipated slaves. The Portuguese employed this term, for want of any other, for the free inhabitants in the country parts of the Molucca islands, to distinguish them from those living in the towns. However, the above terms are now applied, as has already been mentioned, to the inhabitants of the coast and of the interior, who, as we have seen, are considered to be of totally different races. I would here quote an important expression of Prichard respecting the Alforas of these regions:

"What shall we do with the Alforian race, which has been described as a peculiar and distinct people, with a peculiar type and peculiar form of the skull? It will still always remain one of the most remarkable varieties of mankind. To the same category we must refer the mountaineers from Arsaka in New Guinea, whom Lesson has seen and, as it would appear, accurately described, as well as the other natives of the great continent of Australasia."

Latham has in his very learned work, already quoted (p. 213), in his section on "The Papua branch of the Kelænonesian Stock, New

Guinea," adopted two varieties, and given remarkably good profile figures of the forms of their skulls from the 'Voyage sur *l'Uranie* et *la Physicienne,*' one of which is negro-like, dolichocephalic, the other brachycephalic, as it occurs in the above-mentioned brachycephalic Papuans. Do we not, again, see in these figures, in the dolichocephalic skull that of an Alfora, in the brachycephalic that of a Papuan? The author adds, moreover, that the hair of the dolichocephalic is elaborately frizzled, and that of the brachycephalic simply tied up.

With respect to the place of the brachycephalic Papuans, which is, properly speaking, the object of the present inquiry, I would, lastly, venture to put forward the opinion, that they are most nearly allied to the brown Polynesians, being either their elder stock or their descendants, who, through peculiarity of mode of life and climate, have acquired a peculiar condition. Earl wholly rejects the opinion that they are hybrids, and, it would appear, upon very good grounds.*

* Long after the publication of this little work, Prof. Baër, of St. Petersburg, in an essay published this year (Ueber Papuas und Alfuren), has endeavoured to show that Quoy's and Gaimard's Papuan skulls belonged to Malays killed by the Austral negroes, and that no brachycephalic Papuans exist. He therefore restricts the name Papuan, as do the English ethnologists, to dark-coloured Austral dolichocephali.—MS. note by the Author.

(*To be continued.*)

PART FOURTH.

Chronicle of Medical Science.

HALF-YEARLY REPORT ON MICROLOGY.

By John W. Ogle, M.D., F.R.C.P.
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Part I.—Physiological Micrology.

EPITHELIAL SYSTEM.

On the Epithelium of the Urinary Passages.—Burckhardt,* after minutely describing his method of preparing the parts for microscopical examination, considers first the normal epithelium, then the physiological changes which it undergoes, and finally, certain of its pathological states. He delineates three distinct cell-layers existing continuously from the opening of the uriniferous tubes of the kidney, along the urinary mucous surface as far as the outlet of the urethra; the only modification which exists in the character of the cells being at the commencement and termination of this tract. The "*most superficial*" of these three layers of cells consists of round, oblong, or irregularly-formed, flattened bodies. The undermost cells, however, of this layer are much thicker, and differ from the others by reason of their possessing one or more recesses for the reception of the extremities of adjoining cells; the cells, when seen laterally, showing elongated projections from their under surface, overhanging, as it were, these recesses. Of the cells of this upper layer, many have two or more vesicular granular nuclei, the majority only one; they also contain more or less finely granular material.

The "*second*" layer of cells consists of forms of very great variety, and is clearly separated both from the above layer and the one situated beneath it. Its cells, as contradistinguished from those of the first layer, are placed perpendicularly to the mucous membrane; the rounded cells being small, and containing comparatively large nuclei, and being situated at the deepest parts of the layer, whilst those which are oval are larger, many of them being elongated or caudate, the elongations being directed downwards, and their other extremities forming a continuous level line. In the caudate cells the nucleus is usually single, but at times there are more; and when this is so, they occupy the prolongation, the cell wall at their situation often bulging out. The contents of the nuclei may be granular or a bright refracting substance. Very often the termination of the elongated part is rounded or blunted, and may contain a clear granule; it may also be bifurcated or otherwise formed. Many of these elongations are of extreme length, as may be seen in the young and very old. The "*third*" layer of cells consists of an upper and lower portion. In the upper one the cells are rounded and oval, and between them a certain amount of delicate fibre-tissue exists: very often, too, the long projections of cells belonging to the second layer are to be seen fixed among them. In the deeper parts of this layer the connective-tissue is seen to be much more abun-

* Virchow's Archiv, Band xvii. Hefte 1 and 2, p. 94.

dant, the cells being elongated. Elastic fibres are also visible, and the connective-tissue cells are long and spindle-shaped. In this the third layer, a fine capillary network is seen passing up from the small vessels of the subjacent connective-tissue.*

NERVOUS SYSTEM.

On the Structure of Nerve-fibres.—Messrs. Lister and Turner† communicate the results of their observations made with the view of explaining certain appearances presented by nerve-fibres, as seen in sections of the spinal cord which have been hardened in dilute chromic acid, then immersed in an ammoniacal solution of carmine, and subsequently soaked in alcohol previous to being rendered clear and transparent by turpentine,—the method of treatment used by Mr. Lockhart Clarke in his examination of nervous structures. Transverse section of the longitudinal fibres of the cord thus prepared, shows itself as a carmine-coloured *point*, surrounded by a perfectly pellucid and colourless *ring*. The problem was to determine whether the transparent ring was a mere space, owing to shrinking of the object during preparation, or the white substance of Schwann (the medullary sheath) rendered transparent by the turpentine, the axial cylinder alone, in that case, having received the carmine colour. The authors thought that the question might be solved by preparing in a similar way some nerve, the dimensions of whose fibre could easily be ascertained; and for this purpose they made their comparative observations on the sciatic nerve and spinal cord of a cat. Sections of the hardened nerve were soaked in the carmine solution, and then dried and examined without the application of turpentine. Viewed by transmitted light, it appeared as an opaque, confused mass; but by reflected light, each nerve-fibre presented a carmine spot surrounded by a yellowish-white, somewhat granular ring, evidently composed of some solid material, but plainly corresponding to the pellucid rings in the fore-mentioned preparation treated with turpentine. The *dry* specimens of the cord gave no satisfactory results, but one specimen was found still remaining moist, and this was examined by transmitted light. In this section, carmine points were seen in the columnar regions, as in Mr. Clarke's preparations, surrounded by rings; but the latter, instead of being transparent like mere spaces, were dead white; the carmine points, on the other hand, appeared in the thinnest parts of the section as illuminated spots amid the general opacity. This was the case with all nerve-fibres which were large enough to be distinguished. It was obvious that in the cord, as in the sciatic nerve, the carmine central part of each fibre was the axial cylinder, and the opaque circumferential portion the medullary sheath; and therefore, that the pellucid rings in preparations treated with turpentine consisted of the white substance rendered transparent by that re-agent. Moreover, to confirm this opinion, the following additional observations were made:—The hardened sciatic nerve, untinted by carmine, was examined, and transverse thin sections showed the nerve-fibres, by transmitted light, as brownish rings with central transparent, colourless spots, whilst by reflecting light the central parts were seen black. In fact, under a low power, the axial cylinders had in these specimens as much the appearance of mere spaces as the medullary sheaths had in preparations of the cord treated with turpentine. But on examination by a higher power, a granular appearance was seen in the central pellucid part, showing that it was really solid: on afterwards treating similar sections with carmine, this part alone became coloured. The high power also brought out an appearance of irregular concentric lines in the brown medullary sheath.

* See also Report on Pathological Micrology, p. 521.

† Journal of Microscopical Science, Oct. 1859.

These facts are considered as showing the essential difference, chemically as well as morphologically, between the axial cylinder and the medullary sheath; the former being totally unaffected by chromic acid, though the latter is made by it brown and opaque, and concentrically striated; while, on the other hand, the axial cylinder, after subjection to chromic acid, imbibes the carmine easily, although the medullary sheath is quite untinged by it.

As nerve-fibre seems to be the only tissue which, under the use of chromic acid and carmine, exhibits fibre, with a central carmine axis and an uncoloured peripheral sheath, the author looks upon these agents as likely to aid materially in discriminating nerve-fibres among other structures.

On the Papillæ, &c., of the Frog's Tongue.—In a communication of some length, Hoyer* has some highly interesting observations on the various elements of this organ. The main results arrived at were as follows:—That two kinds of papillæ exist at the upper surface of the frog's tongue, one kind broad and containing nerves, the other narrow and destitute of nerves. Of these the first are the true organs of taste, containing a thin nerve-stem (consisting of single-centred fibres), which terminates inside the substratum beneath the point where the epithelium is attached at the end of the papillæ. The epithelium over the termination of the nerve is differently constituted to that on the lateral surfaces of the broad and on all the narrow papillæ, being narrow, elongated, cylindrical cells without cilia. Around these cells exist the ordinary epithelium of the upper surface of the tongue, which, however, possesses much longer cilia than that of the narrow papillæ. Between the areolar-tissue corpuscles and the epithelium of the narrow papillæ an intimate connexion can be seen to exist. The muscular fibres which approach the upper surface of the tongue divide plentifully, diminish in size greatly, and finally form very fine fibrils containing nucleus-like formations. These are, however, not identical with areolar-tissue corpuscles, but belong to the contractile tissues.

Connective-tissue of the Spinal Cord.—Lockhart Clarke† has found that in the calf and other young animals the connective-tissue between primitive nerve-fibres in the white columns of the spinal cord abound both in nuclei and small nucleated cells, of which many are quite equal in size, and similar in character, to numbers of those found in the grey substance, especially the *substantia gelatinosa;* but on account of their position, some are more diversified in shape. They are more or less round, oval, pyriform, club-shaped, angular, fusiform, or crescentic, and lie in the interspaces of the nerve-fibres, sending out fine processes in different directions to join the network of connective-tissue. Frequently a crescentic cell half encircles a primitive nerve-fibre, and forms a swelling or knot around one side of its sheath. There is no difference in structure between these nucleated cells and the smallest of those of the grey substance, from the verge of which they form an uninterrupted layer as far as the surface of the cord. The free nuclei are more or less round and oval, and sometimes partially or wholly surrounded by a kind of flocculent or finely filamentous mass, while in other instances they are attached to the tissue in which they lie by a few fine fibres only. There is reason to believe that this substance surrounding the nucleus is the remains of the cell in process of development into connective-tissue, and the sheath of the nerve-fibre; for as the animal approaches the adult state the cells disappear, while their nuclei remain embedded in the network of connective-tissue. Nuclei of the same character are also found scattered through both the white and grey substance of the cerebrum and cerebellum.

* Archiv f. Anat. Physiol., &c., by Reichert and Dubois-Reymond, Heft 4, p. 481.

† Philosophical Transactions, part 1, 1858, 1859.

Epithelium of the Spinal Cord.—Lockhart Clarke has also found* that in the ox the epithelium lining the central canal consists not only of columnar or cylindrical structures, but also of more or less fusiform structures. In the columnar variety the nucleus is large, sometimes spheroïdal, but generally oval, and contains apparently from two to five large brilliant globular granules or nucleoli, powerfully refracting light. Between this and the verge of the canal the free ciliated end of the cell is broad and short, like the end of a cylindrical brush, and appears to be made up of granular fibres; but in the fusiform epithelium the corresponding free portion consists only of a long and narrow kind of stalk extending from the tapering end of the cell, which latter in this case is similar to what forms the nucleus in the columnar variety, and contains several brilliant granules, which are often arranged in a row. Between these two kinds of epithelium there are grades of transition. All are packed in close apposition, so that the convexity of each is applied to the concavity of the surrounding ones, and by reason of its peculiar shape, it exactly occupies the intervening space.

In the human spinal cord the canal is often completely filled up by what would seem to be the *débris* of the epithelium, for nothing can be seen but a confused heap of nuclei; but sometimes in the midst of this heap there remains a small opening and occasionally two openings, each lined with the usual regular layer of epithelium. The cilia of the epithelium of the cord are much coarser and less numerous than those in the trachea and larynx, and are not easily seen except in the sacral region.

This observer has reason to believe that the epithelial bodies are allied to the connective tissue, for their outer or peripheral ends are connected with nuclei which appear to belong to that tissue, and in the tortoise and some other animals the nuclei found in the white columns are in all respects identical with the nuclei of the epithelium which lines the central canal. He considers that if the processes of the epithelial bodies were directly continuous with, and formed elements of, nerve-cells and nerve-fibres, as maintained by Stilling, we might reasonably expect to find the number of the former always in proportion to that of the two latter; but the very reverse is the case, for in the "filum terminale" of the cord, where both nerve-cells and nerve-fibres have entirely disappeared, the canal is much larger and the epithelium much more numerous than in any other region.

On the Structure of the Double-contoured Nerve-fibres.—The so-called elementary tubules or fibres of which, according to Stilling, the white substance of Schwann normally consists, are considered by Lockhart Clarke† to be nothing else than corrugations, ridges, or folds, produced by the action of chromic acid. His observations also go to show that similar appearances may be caused by acetic acid, and even by mere mechanical disturbance or manipulation.

On the Structure of the Cerebro-spinal Capillaries. By C. Robin.‡—The author considers at length two anatomical conditions bearing on the phenomena of the cerebral circulation. These are, firstly, the disposition of the fibre-cells around the capillaries; and secondly, the existence of a special external enveloping sheath which they possess.

The capillaries themselves he divides into three kinds, according to their calibre. The smallest kind varies in diameter, from that of a blood globule downwards, is composed of a single homogeneous unfibrillated tunic, and is provided with longitudinally-placed nuclei. A larger variety possesses double

* Philosophical Transactions, part 1, 1859.
† Journal of Microscopical Science, Jan. 1860.
‡ Brown-Séquard's Journal de la Physiologie, p. 537, Oct. 1859.

walls, of which the inner one contains nuclei arranged longitudinally, whilst the outer one is entirely formed of fibre-cells containing nuclei with a transverse axis. The third and largest kind of capillary consists of three tunics—viz., of the two entering into the composition of the second variety of capillary, and of a third or adventitious one formed of longitudinal, parallel, and undulating fibrils.

As respects the muscular or contractile nucleated fibres which abound as well in the capillaries of the pia mater, retina, and ciliary processes of the iris, as in those of the substance of the brain and spinal cord, they are so intimately adherent that the line of junction between them is generally undiscernible. They may, however, be well studied after macerating a portion of the tissue containing them for a few days in very dilute nitric acid (10 per cent.), by which, although the nuclei are rendered less visible, the lines of juxtaposition of the fibre-cells become more visible. By this process they very frequently become quite isolated, falling off into the surrounding liquid, or are seen only partially removed, so as still to adhere slightly in a ring-like form not entirely surrounding the capillaries. In the third class, or largest-sized capillaries, the addition of the acid will be found to have destroyed the striated appearance of the outermost membrane, and to have made it granular, but at the same time to have made the transversely-striped character of the fibre-cell tunic very apparent, and to have rendered the cells less regular and of a yellowish hue. When the fibre-cells have been isolated, the nuclei and the substance of the fibres are seen to have become homogeneous.

Passing on to the consideration of the investing tunic or sheath of the capillaries, Robin states that around a certain number of capillaries of the brain, spinal cord, ependyma, and pia mater, such a covering exists at some distance from the proper walls of the capillary, and that the space between the vessel and the tunic is occupied by a colourless liquid mixed with molecular granulations or small free spherical nuclei, about $\frac{1}{5000}$th of a millimetre in size, and varying in number in different parts, in some places being so abundant as quite to obscure the nuclei of the capillary. Almost always in persons above forty or forty-five years of age the space between the vessels and this outer sheath contains much fatty granular matter, and frequently large grains of amorphous hæmatosine, isolated or conjoined. This special sheath could not be traced in capillaries beyond a third of a millimetre in diameter; but when in the smallest vessels it is found to commence, it is seen to be exquisitely delicate, and to be adherent to the capillary walls. The exact ending of the sheath towards the largest-sized vessels could not be made out. This sheath is insoluble by acetic acid, devoid of nuclei or granulation, and quite transparent, being generally undulating at its margins. It follows the capillaries in all their anastomoses, and at a bifurcation is generally dilated. The spherical nuclei beneath the sheath range about $\frac{1}{5000}$th of a millimetre in size, are finely granular, and insoluble in acetic acid. They bear a certain analogy to the leucocytes of lymphatics, and from their position, &c., particularly to those of the lymphatics which are found surrounding a part of the circumference of arteries in reptiles and Batrachiæ. Occasionally fatty granulations are seen floating between the above-named nuclei.

ORGANS OF RESPIRATION.

On the Distribution of the Ultimate Air-tubes and Bloodvessels, &c., of the Human Lungs.—Mr. Waters has made a communication to the Royal Society (May 26th, 1859), and also published a separate essay on this subject.*

* The summary of Dr. Waters' inquiries has been omitted in this place because we hope in our next number to present our readers with an analysis of his work On the Human Lung.—ED.

GLANDULAR SYSTEM.

The Follicular Glands of the Tongue—the Tonsils.—Eckard, of Berlin,* premising that the lymphatic glands now to be recognised as being essentially identical in structure with Peyer's patches of the intestines, describes sections of the follicular glands at the root of the tongue (after being hardened in spirit and well washed) as showing clearly-defined bright spots, of the size of small pins' heads, surrounded by an investment of areolar tissue containing a fine network of elastic fibre, in whose meshes run numerous capillaries. Some of the cavities which have not been thoroughly washed out are seen still to contain lymph-cells, easily to be recognised from the pavement epithelium which exists about the orifice of the glands. In fact, the structure is that of lymphatic glands. The tonsils also are to be looked upon (as described by Kölliker) as a union of follicular glands.

The Minute Structure of the Hepatic Lobules.—Dr. Schmidt, of Philadelphia, has a paper on this subject,† treating of the lobules with special reference to the relationship between the capillary bloodvessels, the hepatic cells, and the canals which carry off the secretion of the latter. In carrying out his observations, the liver of the sheep and the hog were chiefly made use of, especially that of the sheep, when injections were resorted to, as in it the portal vein and hepatic duct, owing to their branching out at acute angles, offered less resistance to the injection. He finds that two capillary networks, each independent of the other, exist in the lobule of the liver; the one commencing at the periphery of the lobule, from the smallest branches of the portal vein and hepatic artery, and ending in the centre in those of the hepatic vein, is destined for the circulation of the blood brought there by the portal vein and hepatic artery; the other commencing independently in the centre of the lobule near the intra-lobular vein (branch of the hepatic vein), and ending in the smallest branches of the hepatic duct, is most probably destined to carry off the secretion of the cells. The cells lie within the meshes of these two networks, but seem to be especially held in their position by their adhesion to the network destined for the secretion. The fine biliary vessels he looks upon as biliary capillaries, but as distinguishing them from the capillaries of the blood, he calls them for the present *biliary tubules*. These he says can only be recognised in specimens in which only the capillaries have been injected. The elements in the lobule of the liver which were formerly thought to be connective tissue, and the existence of which is denied by later observers, Schmidt looks upon as biliary tubules.

The author concludes his paper by a description of certain instruments which he has invented for the purpose of carrying on microscopical investigations most successfully—especially the microscope needle-holder and the microscope dissector, an apparatus worked by screw movement, by means of which, exquisitely fine separation of tissues under examination is effected, and observation of the changes going on during their separation is permitted. Schmidt also describes a new instrument for making microscopic sections of soft tissues.

Part II.—Pathological Micrology.

TUMOURS, MORBID DEPOSITS, ETC.

Tubercle of Bone.—In a thesis for the degree in medicine, of considerable length, Dr. G. Echeverria has brought forward several interesting points illustrative of the pathology of tubercle in the vertebræ. In consideration of the subject he has, in addition to much matter on tubercle in general, separate

* Virchow's Archiv, Band xvii. Hefte 1, 2, p. 171.
† Hay's American Journal, p. 18, Jan. 1859, accompanied by illustrations.

chapters devoted to the nature of this affection of the vertebræ, the tubercular infiltration itself, its origin and progress, the character of the pus, and its relations with the structural alteration, destruction of the vertebræ, tubercular abscess, concomitant lesions, and cicatrization of affected parts. As regards the microscopical investigation of tubercular material of the vertebræ, he describes it as consisting of a very dense fluid, in which exists a large quantity of amorphous material, easily separating on the addition of water, and becoming transparent under the action of acetic acid. This amorphous matter contains a large number of yellowish granulations, some fatty, soft, delicate, and refracting, from 0·001 to 0·002 of a millimetre in size, and soluble in ether; others opaque, firm, disappearing on the addition of hydrochloric and acetic acids with the escape of gas, and from 0·001 to 0·009 of a millimetre in size. In the midst of this amorphous material, irregular medulla-cells and myeloplates exist, of which the latter may be absorbed. Scarcely any adipose cells are met with. The nuclear medulla-cells are less abundant than the other variety, the cellular form.

The author here takes occasion to show the distinction between leucocytes and the nuclei of medullary cells, the first being from 0·007 to 0·011 of a millimetre in diameter, swelling out on the addition of water, and becoming paler on the addition of acetic acid, whilst the latter are about 0·006 of a millimetre in size, and are unaffected by water and acetic acid.

But besides the above-mentioned elements of tubercle in bone, leucocytes of pus exist, covered with fine granulations like the amorphous material around. No trace of any membrane could be found surrounding the pathological material in the centre of the bone. As regards the medullary structure in the immediate neighbourhood, the capillaries are found to disappear. The red colour which gives the appearance of undue vascularity, depends on the increase of medulla-cells, which arises when the inflammation has reached the medulla.

From the entire thesis the following conclusions are to be drawn:

1st. That the tuberculization of vertebræ, like that of other bones, is a totally different alteration from that of other organs.

2nd. That in bone, tuberculous infiltration never exists, that which is so called being of a nature differing essentially from tubercle of the encysted form, which is the only form in which it is found in bone.

3rd. The disappearance of the vertebræ takes place by resorption and invasion; occasionally a separation into fragments of the lamellæ of spongy tissue exists, in which case the remainder depends upon the alteration alluded to, mechanical pressure having nothing to do with it.

4th. The destruction of the vertebræ bears no relation to the abscesses found, which are more frequent when no gibbosity exists.

5th. Deformity of the spinal canal generally occurs without obstruction. Thickening of the dura mater opposite the bend of the canal is the commonest cause of paralysis; sequestra and products of alteration being less common causes.

6th. The healing of tubercle of vertebræ occurs by means of a solid callus. The external callus is not worthy of the name, as it is only a consolidation from stalactites and bony projections.

EPITHELIAL SYSTEM.

On the Intimate Changes in Mucous Membrane effected during Inflammatory Action.—Burchardt, in a paper on the epithelium of the urinary passages,* describes minutely the changes brought about in mucous membranes, the seat of inflammation. He states that during the stage of commencing hyperhæmia,

* See our Report on Physiological Micrology, p. 515.

no peculiar microscopic alterations are to be seen. The capillary network on the surface of the cicatrix is very highly injected, and the cells of the matrix become more clearly distinguished from the tissue itself. Subsequently, exudation of fluid from the vessels occurs into the deeper parts of the second layer of cells entering into the superficial formation of the mucous membrane, and owing to nutritive and mechanical changes, the epithelial cells begin to lose their connexion. Their blood becomes poured from the vessels. Of the cells of the upper layer which have become loosened, many are seen to contain numerous bright granules, and many have no nuclei; so also is it with the cells of the second layer, especially the caudate cells. The deeper sets of this layer of cells—that nearest the matrix—are still in part undisturbed. The small round cells, owing to nutritive excitement, are seen in a state of active growth, especially in length, many of them having double nuclei; but chiefly are the cells of the third layer or matrix in an active condition, increasing by division, and becoming much larger, the matrix consequently becoming much thicker. If the inflammatory process be long continued, the connective tissue becomes increased, and then the matrix is elevated in an undulatory manner, just as in catarrhal changes of the laryngeal mucous membrane, papillary elevations are produced by the formation of new connective tissue.

So far the inflammatory process has produced no morphological changes in the various elements of the mucous membrane. It is otherwise when pus becomes formed. It then becomes greyish, thickened, and softened, and the upper layer of epithelium no longer exists; but how far this and the second layer contribute to form the pus, it is difficult to determine. The formation of pus-cells, however, is determined by Burckhardt to arise from the division of the nuclei in the cells of the matrix, which undergo great increase in numbers, and thus he establishes a relationship between pus cells proceeding from a mucous membrane, the epithelial cells of the same, and the so-called mucous cells, inasmuch as all arise from the round cells of the matrix. The author concludes by relating two cases in which degeneration of the urinary mucous membrane arose from thrombus of the subjacent bloodvessels.

Microscopical Appearances of Human "Horns."—In a paper by Mr. Edwards, of Edinburgh,* the details of an examination of several cases of horny outgrowths are related. In two cases particularly the minute characters of the substance are given in the words of Mr. Salter, of Guy's Hospital, whose observations on their structure coincided with those of Mr. Edwards.

The so-called horn is described as a prolonged growth of the cutaneous papillæ, each one containing a bloodvessel or vessels, and being covered by epidermis uniting the whole into a mass. Thin sections were with difficulty made, and showed only a granular texture pierced with small orifices, and when dry, numerous crescentic cracks. The mass was in no way calcified. On examination of thicker layers with a power of about forty diameters, the above-named small orifices were seen to be sections of cylindrical bloodvessels, many tinged red with contained blood. A clear amber-coloured circular area surrounded each of the vessels which were separated by the general granular structure of the mass, incapable in the compact part of the horn of being reduced to its ultimate original elements. Vertical sections of the edge show best the character of the growth. In these the vessels are easily seen occupying the axis of the papillæ, which are indicated by the clear cylindrical area surrounding the vessels, the limit of the clear cylinder appearing to be the basement membrane of the papillæ, and presenting on oblique section a somewhat jagged outline. The central parts of the horn are more compact and less vascular than the outside. Mr. Salter likens this growth of horn (excepting the fusion into a common mass) to the disease of the hair termed "plica polonica."

* Edinburgh Medical Journal, p. 420, Nov. 1859.

NERVOUS SYSTEM.

New Formation of Nerve in Neuroma.—Dr. Weismann, of Frankfort, describes a case of this kind.* After alluding to the observations of several writers on the same subject, he notices the description first made by Wedl,† of the new formation of nerve in the enlarged terminations of nerves in the stumps of amputation; then the description of the tumour of the median nerve by Führer,‡ which consisted chiefly of nerve fibres, and which had a similar structure to that of the cases described by Wedl, but showed divisions of the nerve bundles and apparently of primitive fibres; and subsequently that by Virchow,§ of the swelling upon the nerves of an amputated arm, which were composed of freely crossing and dividing bundles of nerve fibres containing medulla. In these cases the author notices the defect of any explanation of the manner in which the formation of new nerve fibre comes to pass, an explanation which his own case affords. This case, in fact, occurred in his own person, and therefore its causes were closely watched. In July, 1859, he hurt his left hand with some glass, between the thumb and first finger. The wound was irregular, attended by moderate bleeding, and very painful to touch, or on movement. The skin of the last phalanx of the thumb, on the ulnar side, as far as the middle line of the volar side, was destitute of sensibility. The wound healed slowly and incompletely, a small opening remaining through which the central stump of the nerve projected; the latter had to be deeply divided before the wound healed. In the course of a year and a-half the cicatrix continued more and more exquisitely painful, and beneath the cicatrix a small tumour formed, which, however, could be squeezed by the two fingers without any particular pain. The tumour was removed, and was found to be about seven millimetres long, three broad, and three and a-half in thickness, not differing from the nerve itself in colour, but much firmer in consistence. A sectional surface was smooth, whitish, and showed slight streaks, arranged concentrically around greyish central points. Close by the tumour, and surrounded by thickened neurilemma, was found a small splinter of glass, 4·5 millimetres long and 0·5 millimetre thick, lying parallel to the nerve. On microscopical examination, the entire tumour was found to consist of nerve elements of medulla-holding primitive tubules, which, grouped into bundles with independent investments, showed a thick texture of nerve fascicles crossing in all directions and copiously bifurcating. The spaces between the bundles were of various size, and filled with tough areolated connective-tissue, with nucleus-fibres and occasional elastic fibres. Frequently, however, the nerve bundles were in close connexion. These bundles were of various thickness, and corresponding with them in size were the fibres which were enclosed, fine primitive fibres existing in the fine bundles, and thicker ones in the coarser bundles; but all, even the thickest primitive tubules of these bundles, had a less diameter than the primitive fibres of nerves, and the bundles themselves were much smaller than secondary bundles of the nerves. The nerve bundles in the tumour crossed each other in every possible way, and were met with in a section lying in every possible direction: they repeatedly bifurcated. Each fascicle had its special sheath-like investment, which swelled up in acetic acid and became transparent—small, longish, scanty, elongated nuclei coming into view. The sheath was seen to be of a circular form on transverse section, bifurcating with the bundles. Nitric acid and potash did not dissolve it or colour it yellow. The nerve fibres themselves varied greatly in thickness, and on the addition of acetic acid the medulla became coagulated, forming varicosities. On being treated with fuming nitric acid and potash, they became strongly coloured yellow, the

* Henle und Pfeuffer's Zeitsch. für rationale Medicin, Band vii. Heft 2, p. 209. 1859.

† Zeitschrift der Wiener Aerzte, Band xi. p. 1. 1855.

‡ Archiv f. Physiol. Heilkunde, s. 248. 1856. § Archiv, Band xiii. p. 256.

investment of the fascicle becoming pale, and the medulla escaping in fine colourless drops. Only once could any bifurcation of a primitive fibre be substantiated.

The following was the relation of the nerve to the tumour. Its secondary bundles entered, in some places singly, in other places in numbers, and penetrated, without alteration in diameter, to beyond the middle of the tumour, where they divided, chiefly dichotomously, so that soon they could not be distinguished from the slender nerve bundles forming the mass of the neuroma. The primitive nerve fibres did not bifurcate, however, and the primitive tubules possessed, as far as they could be traced, the same diameter throughout as they had before their entrance into the tumour. The natural neurilemma accompanied some of these nerve-branchlets, and gradually got thinner, until at last only the thin nucleated investment (the perineurium of Robin) above described remained. The uniting medium between the peripheric end of the tumour and the cicatrix did not consist of the regular bundles of nerve, but of the same texture of nerve-fascicles which formed the tumour. Thus, the author says, there can be no doubt, from the bulk and arrangement of the nerve-bundles, that a new formation of nerve-fibres existed. In some cases, the nerve-fibres contained in the bundles were so fine as not to be measurable and hardly to be recognised as nervous elements. According to the accompanying drawings, secondary or lateral nerve-bundles were seen joining the main ones on both sides, but at times on one side, only the investing membrane is continued on to the main bundles, the nerve-fibres within being but barely indicated, and therefore scarcely to be looked upon as enjoying the function of conduction. This latter fact is seized by the author as a probable cause of the want of pain in the tumour itself, whilst the cicatrix above it was very sensitive. In the finest and youngest bundles the fine fibres were often seen either not passing into the main bundle at all or only very partially, and to a short extent, the investment nevertheless clearly being continued onwards. True medulla-holding nerve-fibres were always seen to pass onwards into the main bundle. The author concludes by considering the probable kind and method of new nerve-formation in the case. In doing so, he lays great stress upon the structure of the finest fasciculi and their relation to the coarser ones. On the addition of acetic acid, a finely-contoured investment is seen, possessing, at variable distances, small, sharply-edged, nucleus-like structures, and containing from one to four very fine fibres, swelling out here and there into spindle-shaped knots. At times the peripheric end, as well as the origin of those young bundles from the coarser ones, might be viewed. As a rule, both the contours of the investment could not be seen at the same time, and in the middle of its course it was found to enclose more fibres (from two to four) than at the end. Frequently traces of caudate nuclei were met with, and a drawing is given of the out-bulging of the investing membrane of one of the coarse fascicles to form a lateral branch of small diameter, in which existed only a single line of rather closely-arranged elongated nuclei, which entirely terminated prior to its entrance into the chief bundle. The finest fibres were not confined to the fine bundles, but also existed in the coarser ones along with the medulla-holding primitive fibres. Transverse sections of the finest fascicles were circular, with chiefly a double-contoured investment. From the above observations Weismann concludes that the new formation of nerve-fibres did not arise from the division of the original ones, although it may be that such divisions may be frequently seen in other specimens of the same species of tumour. He never had observed the nerve-fibres of the main bundle to divide with the main bundle and send a twig into the lateral ones. On the contrary, the out-budding proceeded outwards from the perineurium of the primary bundle, whilst the young nerve-fibres, having been formed from spindle-shaped cells in the side bundles, afterwards joined the main ones. Of course the new fibres would not all be continued to the brain, but only those few which had been formed as prolongations of those originally existing, probably by their division.

GLANDULAR SYSTEM.

Cirrhosis of the Liver.—A specimen of this form of disease has been carefully described by Dr. Lionel Beale,* and numerous drawings given of the morbid appearances met with. He finds that the fibrous-looking material between the lobules, usually termed "fibrous tissue," is really not of this nature. He has succeeded in discovering in it numerous capillary vessels and the remains of biliary ducts and secretory structure. He finds that the shrinking of the lobules commences at the circumference, and gradually proceeds towards the centre. This is caused by the cells at the margin of the lobules being first altered by the action of the portal blood charged with deleterious substances; consequently their secretory power is impaired. Similar change affects other cells, and slowly progresses towards the interior of the lobules. The capillaries gradually shrink, and many ultimately become quite impervious. Dr. Beale is led to conclude, from a careful examination of the parts, that "the morbid changes in cirrhosis are not dependent upon inflammation, neither is there any evidence whatever of the presence of any tissue which by its contraction would lead to the alterations in the substance of the gland which have been demonstrated. The first morbid change in cirrhosis affects the cell, and the subsequent alterations result from this, according to well-established physiological laws." With the changes in true cirrhosis Dr. Beale contrasts those occurring in a liver presenting many of the characters of cirrhosis, resulting from obstruction of the common duct. These observations are also very fully illustrated, and the conclusions are summed up as follows:—1st. The cirrhous liver was smaller than the healthy organ; the obstructed organ rather larger. 2ndly. In the first, the diminution of size of the lobules was associated with loss of tissue of the whole organ. In the second, although the lobules were very small, the entire organ was even a little larger than natural. 3rdly. In the cirrhosed liver the structure of the lobule was completely disorganized and in part absorbed. In the obstructed liver, as the lobules diminished in size, the interlobular spaces increased in diameter. What the lobule lost, the space gained. In the first there is an absolute wasting—a loss of material; in the second there is a change in the character of the tissues, without alteration in the bulk of the organ. 4thly. The capillaries of the cirrhosed lobule are shrivelled and wasted, so as not to permit the passage of blood through them, while the circulation in the capillaries of the obstructed lobule was quite free. 5thly. The secreting structure of the cirrhosed liver wastes in consequence of receiving an improper pabulum, but the immediate cause of the changes in the obstructed liver is a physical impediment to the escape of the glandular secretion. 6thly. In this case of obstruction it would seem that the organ was prevented from performing its function to the proper extent. In cirrhosis, the liver undergoes a gradual process of decay.

THE STOMACH.

On Cylindrical Epithelial Cancroid of the Stomach, and its Relation to the Flattened Epithelial Cancroid of the Skin.—Förster,† after noticing instances of this species of growth related by authors, gives five examples of his own. Describing generally the character of such a growth, he remarks of it as follows: For the most part existing in a ring-like form over a limited part of the

* Archives of Medicine, No. 2, p. 118.

† Virchow's Archiv, Aug. 1858, p. 91.

pylorus, it is elevated sometimes above the mucous membrane, and often has a lobulated or villous surface, being soft, of a greyish-red colour, and full of white juice. It is not necessarily limited to the mucous or sub-mucous tissue. The juice only contains cylindrical epithelium of the ordinary characters, often in rows or gland-like groups. On drying or hardening and making fine sections of it, acinose bodies of various sizes and forms, composed of cylindrical-epithelium, and imbedded in a delicate areolar stroma full of capillaries, are seen. The cells of these acini exist, as in several glands, perpendicular to the periphery in single or multiple layers, with their outer extremities so closely in a line that it looks as if the acinus was composed of a fine membrane. The inner end of the epithelial cells are either closely approximated or permit a free space around, as seen on transverse section, in which fine albumen-like molecular masses or irregularly separated cells exist. More compound forms also exist, from which secondary out-bulgings proceed; but the type of the cell-arrangement remains the same. The surface is often found in a state of degeneration, but when entire, the normal epithelium of the stomach is seen to exist, and often numerous long papillæ or villi covered with epithelium, with a vascular loop are seen. It is impossible to demonstrate a union between these cylindrical-epithelium bodies and the normal glands of the mucous membrane, from which they differ by their irregular form and arrangement, and especially in the want of a proper membrane. Like the succulent medullary cancer in appearance, this growth also resembles it in most cases by involving secondarily the lymphatic glands and other parts. After describing the growths in question, the author quotes most of the cases reported by others, such as those of Reinhardt,* called by that author hypertrophy of the gland follicles; those of Bidder,† called by him epithelial cancer; and those of Virchow,‡ called by him cancroid. He then narrates at length cases examined by himself, which need not here be detailed. He styles them cancroid, and says that all the growths hitherto passing under that name are like to those which he describes, except that in them the flattened form of cell prevails. He shows that, however variable are the outward forms of cancroid, one thing is constant, and that is, the typical arrangement of the epithelium in the network stroma as acinose bodies. As a rule, this growth is connected with areolar tissue, and only occasionally with cuticular glands, contrary to what he used to think. This he supposes from seeing in the neighbourhood areolar-tissue cells much enlarged, filled with nuclei, and having a tolerably regular arrangement, and also free nuclei whose investing membrane had been destroyed. The smallest acini appear to arise from the formation of cells around the nuclei. The author then compares ordinary cancroid and the cylindrical-epithelium growth described. In both there is a fibrous stroma, in whose spaces typically-formed acinose bodies exist, made up of epithelium, in the one case flattened, but in the other cylindrical. In both the scaffolding is papillary, and the cell-formation may in both approximate to cancer. He shows the relation between these tumours and the glandular polypi and villous tumours of the rectum and stomach. The latter consist of acinose bodies, and are without any proper membrane; but they are simply repetitions of cylindrical forms of intestinal glands. Sections of the villous tumours are white and juicy, and show cylindrical epithelium.

* Annalen der Charité, Band ii., 1851. † Müller's Archiv, 1852, Heft 2, s. 178.
‡ Gazette Médicale de Paris, April, 1855.

HALF-YEARLY REPORT ON FORENSIC MEDICINE, TOXICOLOGY, AND HYGIENE.

By Benjamin W. Richardson, M.A., M.D., M.R.C.P.
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I. Toxicology.

Antimony and its detection.—M. Lassaigne, in commenting on a case in which it was necessary to discover whether antimony were or were not present in the tissues of a person supposed to have been poisoned by successive doses of emetics, makes some objections to the use of Marsh's apparatus. He argues that, after complete oxidation of tissue, the antimony passed over from the Marsh bottle in the form of antimoniuretted hydrogen, only represents a part of the absolute amount contained. In this respect, Marsh's process contrasts very unfavourably as compared with its application to the detection of arsenic. The following is a description of the more simple plan for the qualitative detection of antimony in an organ, as the liver.

MM. Lassaigne and Lorain burned fifty *grammes* of the liver in a new porcelain crucible. The carbon proceeding from the burning was calcined by maintaining during several hours a cherry-red heat. The ash collected, reduced to a fine powder, was successively treated with weak nitric acid and pure hydrochloric acid. The solution, to which was added a solution of weak tartaric acid, was filtered and brought into contact with three times its volume of hydrosulphuric acid. This reagent immediately threw down a yellowish orange flaky precipitate of hydrated sulphide of antimony, the volume and colour of which, compared with a precipitate formed in a standard solution of tartar emetic, permitted the operators to estimate the proportions of the salt contained in the hepatic tissue submitted to observation.—*Ann. d'Hygiène publique*, tome xi. pp. 192–7.—1859.

Detection of Arsenic, Antimony, Copper, and Bismuth, by Electrolysis.—At the last meeting of the Chemical Society, Professor Bloxam, of King's College, gave the results of a very successful investigation of this subject, and showed that, by proper refinement in the method of operating, the process of electrolysis may become a certain and delicate means of detecting one or all of the metallic poisons, at least, with but few exceptions. In an examination for arsenic by this method, the metal is obtained in the form of arseniuretted hydrogen; the process is therefore very similar to that of Marsh, over which, indeed, it does not present any advantage in point of delicacy. Marsh's process, however, although it is capable of doing all that can be done by electrolysis with even greater delicacy, is open to several well-known objections, which have stood in the way of its practical adoption by toxicologists. The process of electrolysis does not involve the use of zinc, which is so difficult to obtain pure. It forms a general method for the detection of several metallic poisons at once, and the material tested is not destroyed or inconveniently contaminated, but may be used for another operation. When the arsenic, on the other hand, is present in a state of arsenic acid, it cannot, according to Prof. Bloxam's experiments, be detected with certainty by electrolysis. It is consequently necessary to reduce the arsenic acid by means of sulphurous acid. This is an objection which does not apply to Marsh's process. In his earlier experiments, Mr. Bloxam made use of a U tube containing dilute sulphuric acid; the substance to be tested was introduced into one of the limbs, and a cork with a bent tube fitted to its mouth; two platinum plates, leading from the poles of a battery containing five cells of Groves, were introduced into the two limbs, and the liberated hydrogen passed through the bent tube, which was heated by a lamp, when the arsenic, if present, was deposited.

The form of apparatus ultimately adopted as being the most convenient, consists of a two or three-ounce bottle, the bottom of which has been cut off, and replaced by a piece of vegetable parchment, bound on with platinum wire. To the mouth of the bottle is fitted a cork with a bent tube and a piece of platinum wire, which passes through the cork, and turns up beneath in the form of a hook. A slip of platinum then hooks in the end of the wire, and passes nearly to the bottom of the bottle; it forms the negative pole of the arrangement. The bottle stands in an ordinary test-glass, and the positive pole, also of platinum, stands in the glass. Dilute sulphuric acid is put into the bottle, and also the glass, so as to stand to the same height in both vessels. The substance to be tested is introduced into the bottle, the cork adjusted, and the wires connected by five cells of Groves' battery; the heat of a spirit lamp is applied to the bent tube, and in the course of a quarter of an hour a distinct mirror is obtained, if arsenic is present. Standard solutions, containing respectively a tenth, a hundredth, and a thousandth of a grain of arsenious acid, were prepared and examined by this process, and in every case a successful result was obtained. These solutions were then mixed with organic substances, such as the ordinary articles of food—meat, eggs, milk, &c.—and the resulting matter examined.

It was got into solution by means of chlorate of potash and hydrochloric acid, and the resulting fluid evaporated down by means of a water-bath to a thick, syrupy liquid. The arsenic was thus obtained in the state of arsenic acid, which does not give a certain result by the electrolytic process. Some sulphurous acid was therefore added, and the mixture introduced into the bottle, after expelling the excess of sulphurous acid by evaporation; a drachm of alcohol was then poured over the surface, and the process put into operation. The author prefers to add this drachm of alcohol in every case, inasmuch as it not only allays the frothing, but also affords an additional indication of the presence of arsenic; for when these two substances are present—the alcohol and the arsenic—the gas which escapes at the open end of the test tube possesses a very peculiar odour, resembling alkarsin. If a little sulphurous acid be present, it also furnishes an additional character indicative of arsenic; namely, a slight yellow deposit, consisting of sulphide of arsenic, close to the borders of the metallic mirror. In all these experiments, of which a great number were made, the thousandth of a grain of arsenious acid was readily detected.

The other metals which may be detected by this process are mercury, antimony, copper, and bismuth; lead is precluded by the sulphuric acid which is present. These are all precipitated in the metallic form upon the slip of platinum, and even in the case of antimony a mere trace of antimoniuretted hydrogen is formed, the metal being all deposited upon the negative pole. The mode of proceeding in these cases is precisely similar to that adopted for arsenic; when the operation is concluded the slip of platinum is detached, washed, and the deposit dissolved off in the usual manner. Thus, where an organic mixture has to be examined for arsenic, mercury, copper, antimony, and bismuth, it is prepared in the manner just described for arsenic, and the resulting liquid introduced into the bottle, the drachm of alcohol poured over the surface of the contents, the cork adjusted, and the battery connected. The heat of a spirit lamp is applied to the bent tube, and the operation continued for about a quarter of an hour or twenty minutes, when, if arsenic is present, a metallic deposit, accompanied by some crystals of arsenious acid, will be formed in the tube, and the escaping gas will have the alkarsin-like odour. The piece of platinum in the bottle is next removed, washed, and boiled in yellow sulphide of ammonium. Antimony would be dissolved and might be obtained as sulphide by evaporating this solution to dryness. The other metals would still remain in the plate; it is next boiled in nitric acid containing a trace of hydrochloric acid, the solution evaporated to a small bulk, and an excess of ammonia added. Oxide of bismuth would be precipitated, together with whatever traces of platinum had been dissolved. The precipitate may be dissolved in hydrochloric acid, and tested by pouring into water, &c. The ammoniacal filtrate would contain the copper, indicated by its blue colour, and the mercury. By boiling with hydrochloric acid and a slip of copper, the latter would be separated in the metallic form.—*Pharmaceutical Journal*, January, 1860.

Poisoning by Arsenic and Strychnine.—Dr. Abegg was summoned at midnight on the 8th of January to a patient who was taken ill at eight o'clock. The patient exhibited all the signs of strychnine poisoning, but none of those common to arsenic. On inquiry, it was found that he, the patient, had first taken a dose of arsenic, and then a dose of strychnine in order to secure a more rapid and unfailing result. An emetic of ipecacuan was given, and also citric acid, and hydrated peroxide of iron with calcined magnesia. After the emetic, the frequency and intensity of the spasmodic attacks seemed rather to increase. The last fatal attack occurred about two o'clock. At the post-mortem examination, which was postponed until the 15th of January, there were found external signs of commencing putrefaction. The muscles, which had stiffened during life, retained their rigidity. This rigidity, indeed, lasted eight days after death. The lungs were infiltrated in patches; the heart was full of thin fluid blood. The brain contained fluid blood. The mucous membrane of the larynx was loosened and reddened, as in death from asphyxia. In the lower part of the stomach and in the pylorus there were dotted extravasations, but no further indications of an irritant poison. In the stomach and small intestines there was found arsenic in large quantity; in the softened food there was also much arsenic and decidedly strychnine; in the urine no trace of poison was found.

The rapid absorption, and therefore suddenly fatal effects of the strychnine, shows why the arsenic discovered after death in great quantity could not develop its peculiar effects during life. The patient whilst alive exhibited the pure form of poisoning by strychnine, whilst after death so little strychnine was found in proportion to the arsenic, that it might easily have been overlooked in a less accurate chemical scrutiny.—*Julius Clarus, in Schmidt's Jahrbücher*, Band cii., 1859—From *Günsb. Zeitschrift*, 1858.

[We opine that more value attaches to this case than is noticed by the learned commentator quoted above. The case would seem to show that strychnine is able to prevent the absorption of arsenic, inasmuch as there was sufficient time, from six to seven hours, for ensuring such absorption under ordinary circumstances. It has been proved that the contraction produced by strychnine extends to the arterial trunks; may it not extend also to the capillary circulation, rendering absorption a mechanical impossibility?—B. W. R.]

Poisoning by Lead contained in Snuff.—Dr. Wicke makes some remarks in reference to a case of this kind, recorded by Dr. Alfter. The symptoms were violent attacks of colic, which first awakened suspicion that the patient was being poisoned by lead. No source could be found for such poison except in the large quantities of snuff which he took. Herr Höckel examined the snuff, and found it to contain two and a half per cent. of metallic lead. Dr. Wicke is of opinion that lead finds its way into snuff from the lead wrapping in which snuff is packed. The packets being exposed often to damp and to the atmosphere, the lead becomes changed into carbonate, and the snuff is charged with the poison. He thinks that this view is corroborated by the fact that in examining packets of snuff the lead changes in amount as one approaches the centre, and that at the corners of the packets, where contact with the wrapping is most perfect, the crust of carbonate of lead is most marked. The absorption of the lead by snuff-takers is mainly by the stomach. The snuff is conveyed to the palate in small quantities, and thence is conveyed downwards to the stomach.—*Zeitschrift für Rationelle Medicin*, Band vii. Heft 1, pp. 158, 159.

Toxicological Properties of Tanghinia Venenifera.—In a paper on this poison, Professor Pelikan states:—This tree grows in Madagascar, and belongs to the family of apogineæ (to which also vinca and nerium oleander belong). It contains a milky juice; its most poisonous part is the fruit, a stone berry, similar to a lemon, with a stone resembling that of a peach, which is the principal seat of the poison. Prof. Pelikan had an alcoholic extract prepared from the leaves and stalks of the plant, and, aided by Prof. Kölliker, experimented with it on frogs. The experiments proved that it does not belong to tetanic poisons. Its effect is particularly directed upon the heart, the action of which it paralyses, leaving the ventricles in a bloodless condition. This effect is a direct one, and not brought about merely by the medulla oblongata and the spinal marrow. Secondarily, it paralyses the motor nerves in the direction from the centre towards the periphery; tertiarily, it paralyses the muscles of voluntary motion. The tanghinia is thus to be considered a specific poison for the heart and muscles; it paralyses the muscles less rapidly than upas, veratrine, and sulphocyanide of potassium, but in regard to its paralysing action upon the heart, surpasses considerably the two other poisons, veratrine and the sulphocyanide.—*Verhandlungen der Phys. Med. Gesellschaft in Würzburg*, Band ix. p. 1; and *North American Med.-Chir. Rev.*, Jan. 1860.

Poisoning with Kunaree.—Assistant-Surgeon F. Broughton, Civil Surgeon, Kholapore, reports that a case of poisoning occurred at the above place with this vegetable. The kunaree (nerium odorum, or oleander) is well known, and extensively resorted to in this part of India as a poison; the expressed juice from the red variety being considered the strongest and most fatal. Large doses are so generally followed by death, that this is the first occasion in which he has been able to mark the effects of a known quantity.

Kardaree bin Dewba, a woodcutter, aged thirty-five, of slight and delicate appearance, was brought to the Civil Hospital at Kholapore, on the morning of the 9th of August, 1858, in a state of insensibility. It appeared from the evidence of his wife, who accompanied him, that a quarrel had arisen between them in reference to the *res angusta domi*, and that he had swallowed a cup of kunaree. From subsequent investigation, it was ascertained that the cup contained a little more than an ounce of expressed juice of oleander, and that at the time of drinking the poison he was standing five yards from his door, towards which he walked immediately, and fell senseless at the threshold.

On admission, his face and eyes were flushed, head hot and perspiring, with stertorous breathing and foaming at the mouth. This was accompanied by violent spasmodic contractions of the muscles of the entire body, but more remarkably so in the superior than inferior extremities, and also more developed on the left than the right side. The effect of this was remarkable. During the intervals of spasm, the patient lay evenly upon his back, and when action commenced, the superior contraction of the left side threw him over on his right, in which position he remained during the paroxysm, after the subsidence of which he fell back into the natural position of exhaustion. Emetics of antimony having failed, sulphate of zinc produced the ejection of large quantities of greenish matter.

Insensibility remaining, with quick pulse and hot skin, leeches were applied to the temples, and sulphate of magnesia given as an aperient. The bowels were moved, although involuntarily, and the evacuations were watery, greenish in colour, containing but little fæculent matter. The spasms returned at intervals of an hour, and were apparently produced by any attempts to move or rouse him. Towards evening the spasms decreased, the face became pale, the pulse sank to a thread, the eyes sank into their sockets, and the extremities rapidly became cold. Frictions, mustard poultices, with hot bottles, ammonia and camphor, restored the circulation, but insensibility continued, and the bowels were moved involuntarily. In this condition he remained the whole of the next day; the spasms were less violent and diminished in frequency. He swallowed the ammonia, camphor, and magnesia, which were continued, but the urine and the evacuations still passed involuntarily. On the evening of the 10th reaction was established, the skin became hot, and the pulse increased; there were no spasms, but insensibility remained as complete as before. A full dose of castor oil was given, and the bowels acted freely, after which he seemed to be in a quiet sleep.

He awoke on the morning of the 11th restored to speech and reason. Weakness only remained as the natural consequence of so violent a seizure. He entirely recovered, and was not, Mr. Broughton fears, particularly grateful for his recovery, as he anticipated a recurrence of domestic trouble. He assured Mr. Broughton, however, that he recollected nothing from the moment he swallowed the draught, and could form no opinion of the time which elapsed since the suicidal attempt and his recovery.—*Transactions of the Medical and Physical Society of Bombay*, 1859.

Further Experiments with Bibron's Antidote.—Dr. D. O. C. Heery relates ('Atalanta Medical and Surgical Journal,' Aug. 1859) the following case of rattlesnake bite in which he employed Bibron's antidote with success:—"In travelling through south-western Georgia in April last, I happened to be at the house of Colonel B. Shortly after my arrival, he informed me that one of his most valuable negroes had just been bitten by a large rattlesnake, while returning from the field. The negro was bittten in the ankle of the left leg. The snake inflicted a very deep wound, and within five minutes after the bite, before much pain or swelling had ensued, I administered one dose of Bibron's antidote in two tablespoonfuls of brandy, and the symptoms almost immediately disappeared. One hour after the bite pain and swelling returned, attended with considerable throbbing. I repeated the antidote, and in less than fifteen minutes the ankle had regained its natural appearance, all pain and swelling having vanished. Before returning, I repeated the dose a third time. In the morning he was perfectly well, and resumed his duties in the field."—*American Journal of the Medical Sciences*, Oct. 1859.

Detection of Hydrocyanic Acid in a Body three weeks after Death.—A young man poisoned himself with twenty-five grammes of medicinal prussic acid twelve times diluted. Dr. Brame, who was called upon three weeks after death to prove the presence of the poison, reports that the contents of the stomach, which did not smell of prussic acid, exhibited a copious flaky, yellowish precipitate with pure nitrate of silver, which, when washed and dried by heating for several minutes in a vacuum, became of a grey colour. The precipitate was soluble in ammonia and in cyanide of potassium. Decomposed in heat with caustic alkali it formed cyanide of potassium, from which were obtained prussic acid and Prussian blue. Diluted with water the precipitate yielded, on passing through the solution sulphuretted hydrogen, a clear solution of prussic acid; it also formed prussic acid on being treated with hydrochloric acid and heat in a glass tube. The quantity of cyanide of silver was about 0·60 grammes, equalling 0·12 of prussic acid. The case is remarkable in that the contents of the stomach, while they may not smell of prussic acid, may contain considerable quantities of the acid in a free state. The whole account leaves behind much to be wished.—*Julius Clarus in Schmidt's Jahrbücher*, Band cii., 1859; and *Journal de Toulouse*, Jan. 1859.

Poisoning by Nitrobenzin.—Caspar, in his 'Vierteljahrschrift' (Band xvi., Heft 1), describes the properties of nitrobenzin ($C_{12}H_5NO_4$). It is a gold-coloured clear fluid, of a pleasant, sweet taste, and a penetrating smell of bitter almonds. It can be obtained at a small cost. In an experiment with this poison, he found that a rabbit was killed in a minute and a-half by one ounce of the substance. The pupils dilated, and convulsive movements occurred in the lower extremities. Under the influence of the same dose, a middle-sized dog fell into a stupor after some hours; his breathing was heavy; his skin cool. He was killed by cutting his throat. In both cases the blood, as well as all the organs, smelt very strongly of bitter almonds, and retained this smell for many days. The conclusions arrived at by Caspar, are—1. That nitrobenzin might have a fatal effect upon men, although the relative (poisonous) dose is not as yet determined. 2. That the strong smell of bitter almonds in a body and its contents can no longer be ascribed exclusively to poisoning by prussic acid, as was formerly supposed. On the post-mortem appearances in these experiments, Caspar places no value, but infers that one circumstance—viz., that the specific smell of nitrobenzin is evident for fourteen days after the poison is administered, offers a realizable diagnosis. In doubtful cases, therefore, the body, when opened, should be left for several days, in order to ascertain whether or not the smell of almonds remains perceptible.—*Maschka, in Vierteljahrschrift für die prakt. Heilkunde*, xiii., 1859.

II. Obstetric Jurisprudence.

Dr. Storer, in a most interesting and important paper, thus reports on laws relating to abortion as a criminal procedure:—

It would seem that little doubt could be entertained of the inefficacy of our present statutes against abortion. There are few of the States whose laws on this point are so wisely and completely drawn as in Massachusetts; yet, as they there stand, they cannot, as such, be enforced. In that Commonwealth, according to the reports of the Attorney-General, during the eight years, from 1849 to 1857, omitting 1853, as there seems to have been no report rendered for this year, there were, as we have seen, thirty-two trials for abortion, and not a single conviction!

A committee of the State Medical Society of Massachusetts, to whom the propriety of a professional appeal to the Legislature for more protective statutes had been referred by the District Society of Boston, having reported against each action, on the ground "that the laws of the Commonwealth are already sufficiently stringent, provided that they are executed," it becomes the more necessary for us to strike at the root of the whole matter, and to show, if possible, why conviction, unless in case of the death of the mother, cannot at present be obtained.

That the prevalence of abortion is in a great measure owing to ignorance of guilt on the part of the community at large, we have shown. We now assert that its futile prohibition by the law, its toleration, are plainly in consequence of similar ignorance on the part of legislators and of officers of justice.

In the following States, Rhode Island, New Jersey, Pennsylvania, Delaware, Maryland, North Carolina, South Carolina, Georgia, Florida, Kentucky, Tennessee, Iowa, and the District of Columbia, there appear to exist no statutes against abortion, and the crime can only be reached by common law, and by the rulings of the courts.

In the case of the District of Columbia, all rulings are based on the common law, the old English statutes from Elizabeth to George II., and the old colonial statutes of the Province of Maryland down to 1800, the period of cession to the United States of that portion of Maryland lying on the north side of the Potomac, now included in the Federal district.

The later English statutes, even those of George IV., being enacted subsequent to the separation from the mother country, are not recognised in the District. In the State Courts, so far as the rules and principles of the common law are applicable to the administration of criminal law, and have not been altered or modified by acts of the Colonial or Provisional Government, or by the State Legislature, they have the same force and effect as laws formerly enacted. In the States referred to, therefore, as having no special statutes of their own, the later English statutes, though not of absolute force, are to a certain extent undoubtedly acknowledged.

By the common law of England, as stated by Blackstone—"If a woman is quick with child, and by a potion or otherwise killeth it in her womb, or if any one beat her, whereby the child dieth in her body, and she is delivered of a dead child, this, though not murder, was by the ancient law homicide, or manslaughter. But the modern law doth not look upon this offence in quite so atrocious a light, but merely as a heinous misdemeanour." "But if the child be born alive, and afterwards die in consequence of the potion or beating, it will be murder."

By this law it is necessary to furnish proof not merely of pregnancy, but of quickening; no one besides the mother can be reached, save in the rare instance of beating; it is often impossible to prove that a child, born living, has died in consequence of the means used upon itself or its mother before the birth.

The Ellenborough Act passed in 1803 runs thus—"If any person shall wilfully and maliciously administer to, or take any medicine, drug, or other substance or thing whatsoever, or use, or cause to be used or employed, any instrument, &c., with intent to procure the miscarriage of any woman, not being, or not being proved to be, quick with child at the time of committing such thing, or using such means, then, and in every such case, the persons so offending, their counsellors, aiders and abettors, shall be and are declared guilty of felony, and shall be liable," &c. &c., if before quickening, to fine, the pillory, stripes, or transportation; if after quickening, to death; but in the clause providing for the latter case, mention of instruments was omitted. Punishment was thus extended to the crime prior to quickening; and, besides the actual perpetrator, to those counselling and assisting therein. For its commission after quickening, capital punishment was restored; but by a strange oversight, no penalty was provided for the cases occurring at this period, where the abortion was induced by instrumental or other mechanical violence.

To remedy this defect, another Act—that of Lord Lansdowne—was substituted in 1828. "If any person, with intent to procure the miscarriage of any woman then being quick with child, unlawfully and maliciously shall administer to her, or cause to be taken by her, any poison or other noxious thing, or shall use any instrument, or other means whatever, with the like intent; every such offender, and every person counselling, aiding, or abetting such offender, shall be guilty of felony, and being convicted thereof, shall suffer death as a felon." If the woman were "not, or not proved to be, then quick with child," the offence was still felony, and punished by transportation and imprisonment and stripes. By this enactment, the mistaken belief in a difference of guilt, according to the period of fœtal life, was still retained. For which reason, and in the false hope that by again abolishing capital punishment, juries would more frequently decide in accordance with the fact, the English law has later, and during the present reign, still further been modified and rendered more just, simple, and comprehensive.

"Whosoever, with the intent to procure the miscarriage of any woman, shall unlawfully administer to her, or cause to be taken by her, any poison or other noxious thing, or shall unlawfully use any instrument, or other means whatever, with the like intent, shall be guilty of felony, and being convicted thereof, shall be liable," &c. &c., to transportation or imprisonment. By the present English statute, the woman herself can hardly yet be reached; many convictions must be lost from failure to prove the agent either a poison or noxious thing; in other respects, however, it is well drawn. Attempts at crime are covered, and proof is not required of pregnancy or of actual injury to the mother.

So far the instances in this country of an absence of special statutes. Where such exist, they may be variously classified. Reserving for a little all other considerations, we find them at once falling into four great divisions:

I. Those acknowledging the crime only after quickening has occurred:

Connecticut,	Minnesota,
Mississippi,	Oregon.
Arkansas,	

II. Those acknowledging the crime throughout pregnancy, but supposing its guilt to vary with the period to which this has advanced:

Maine,	Ohio,
New Hampshire,	Michigan.
New York,	

III. Those acknowledging the crime throughout pregnancy, unmitigated, but still requiring proof of the existence of this state:

Vermont,	Missouri,
Massachusetts,	Alabama,
Illinois,	Louisiana,
Wisconsin,	Texas,
Virginia,	California.

If our previous assumption of the actual character of criminal abortion be granted, and we believe they have been proved to a demonstration, it must follow from the subsequent remarks, that the common law, both in theory and in practice, is insufficient to control the crime; that in many States of the Union the statutory laws do not recognise its true nature; that they draw unwarrantable distinctions of its guilt; that they are not sufficiently comprehensive, directly allowing many criminals to escape, permitting unconsummated attempts, and improperly discriminating between the measures employed; that they require proofs often unnecessary or impossible to afford; that they neglect to establish a standard of justification, and thereby sanction many clear instances of the crime; that by a system of punishments wholly incommensurate with those inflicted for all other offences whatever, they thus encourage instead of preventing the increase; and that in many respects they are at variance, not merely with equity and abstract justice, but with the fundamental principles of law itself.—*North American Med. Chir. Review*, Sept., 1859.

Production of Abortion in India.—Mr. Carter, surgeon to the coroner, Bombay, reports that although the fact of producing abortion in Bombay by the introduction of instruments into the uterus is well known, yet it is only now and then that cases are found in such situations and under such circumstances as to lead to their discovery; hence it seems desirable that when such cases do occur, the medical testimony respecting them should be recorded, and therefore it is hoped that the following depositions, which are given as they were taken down by the coroner, will be found acceptable.

"Case I.—Dec. 11th, 1857. I examined the body of a deceased male infant before the jury; it appeared to have arrived at the full period of seven months' gestation, and was fourteen inches and a half long. The placenta and umbilical cord were attached, and the latter entire. There were no marks of violence externally about the infant, and it appeared to have been born within the last twenty-four hours. I examined the body internally, and found the organs of the abdomen and chest healthy and natural; part of the meconium appeared to have been discharged, and the stomach contained a little glairy fluid; there were signs present in the lungs of the child having breathed, but not freely. I also examined the head, and found the left frontal bone broken in two, with the brain in a pulpy state, and partly extravasated under the scalp. I was unable to say if the injuries occurred during lifetime; but if they did, they were in all probability the cause of the child's death, unless the child died of its tender age. I therefore was unable to state what was the cause of death.

"I also examined Cassee, the reputed mother of the infant, and found that she bore all the signs of having been recently confined. On examining the vagina, I found a stick in it six inches long and one-eighth of an inch broad, which projected into the womb; it consisted of a piece of bamboo, and was covered at one end with a dossil of cotton. I cannot conceive the object of this stick being in such a position, unless it got there by accident during an attempt to procure abortion. The stick might have been inserted by Cassee herself without any other person's aid; but, being so high up, it could not be withdrawn by her alone. I had no conversation with her about it.

"In this case the piece of bamboo had fallen in such a transfixed position, that it was impossible for a person unacquainted with the relation of the parts to remove it without considerable violence, which I suppose, with the difficulty of reaching it, was the reason why it was left there.

"CASE II.—Jan. 17th, 1858. I examined the body of a deceased female infant before the jury. It had arrived at the seventh month of utero-gestation. The placenta and umbilical cord were attached, and the latter entire. The child had been born within the last forty-eight hours. There were no marks of violence about the body; there were signs present of the child having breathed; the stomach was empty; there was no mark of a ligature round the neck, neither was there any foreign substance in the mouth or throat. I was unable to assign the cause of death.

"I was not called upon to examine the mother of this infant, as she had been previously admitted into the Midwifery Hospital under Dr. Mead, in whose presence the nurse removed from the vagina a stick about four inches long, bound round throughout with cotton and lead, and coated at one end with red-lead. This stick I examined when the case was brought before the magistrate, and then also saw a bundle containing three or four other sticks of the same kind similarly bound round with cotton, &c., which had been found in the woman's house.

"CASE III.—In a third instance I had to give evidence where the instrument produced with which the abortion was said to have been effected consisted of a straight piece of a small branch of oleander, about the size of the piece of bamboo mentioned in the first case, but without any arming of lint, and with one end (on which there were stains of blood) just as it had been broken off from the continuation of the branch."—*Transactions of the Medical and Physical Society of Bombay*, 1859.

III. MISCELLANEA.

Studies on Strangulation.—Dr. Tardieu, in a paper on the above subject, endeavours to give an exact and practical definition to the term strangulation, as distinguished from suffocation and hanging, so as to constitute, if the term may be used, a medico-legal species. Dr. Tardieu thus defines strangulation: "An act of violence, consisting in the direct application of constriction, either around or in front of the neck, having the effect, by preventing the passage of air, of abruptly suspending respiration and life."

Our author teaches that strangulation, as a general rule, is an act of the non-suicidal kind; it is connected either with the marks of blows or wounds of the head, showing that consciousness has been destroyed before the constriction has been applied; or it is connected with suffocation, previously brought about by occasion of the mouth and nose; or again, it is accompanied with signs of injury on other parts of the body. It is generally committed on women or new-born children, and it may, indeed, be regarded as an act only perpetrated on persons either physically feeble or paralysed by some antecedent injury. After pointing out the difference of signs in cases where the strangulation has been made by a cord, and by the hand, and in cases where the resistance has been little and great, Dr. Tardieu notices that the face in strangulation is tumefied, violet coloured, and as if marbled.

There is also a peculiar and constant sign—viz., ecchymoses, very numerous and of extremely small dimensions, upon the face, under the conjunctiva, and in the front of the throat, and in the chest. This sign is not absolute, as it may occur in suffocation, but it is very striking. The mark of the constriction by the cord differs from the mark produced by hanging, in that it is not deep; it need not be continuous around the neck; the edge of the furrow, instead of looking like parchment, is often pale, and approaches in colour the violet tint of neighbouring parts; it does not present any change of texture or consistency, no thinning, and no condensation. In cases where a stick or tourniquet has been used in the tightening process, a mark of the stick will be found impressed on the parts against which it rested; while, in instances where the hand has been used, the mark of the fingers is often so fairly delineated, that the hand which the murderer used, and his position to his victim, may be determined. The appearances of the internal parts of the body are deep ecchymosis of the soft parts of the neck. This is especially brought out in instances where the hand has been the compressing body. The larynx and the trachea are rarely the seat of great mischief; fracture of the cartilages and fracture or luxation of the hyoid bone are quite exceptional. The internal surface is most frequently congested and of an uniform red colour; sometimes violet mucus is found in greater or lesser quantity; the mucus is sometimes sanguinolent, and, in fact, Tardieu has seen it in some cases replaced by an exudation which, having coagulated, lined the larynx.

Luxation of the spine in true strangulation is doubtful. The lungs are not often obstructed, or but little; they are of moderately uniform rose-colour, sometimes strongly congested, sometimes quite normal. There is almost always rupture of the superficial air-vessels, giving rise to more or less extensive emphysema. The ruptures are multiple sometimes, isolated more frequently; united in groups, they give the lung the appearance of being scattered over with pseudo-membranous patches, of small thickness, very white and of various sizes; but which, on being carefully examined, are found to contain small bubbles of air, and may be caused to disappear by simple puncture. The subpleural punctuated ecchymoses scattered over the surface of the lung, which are the essential signs of death by suffocation, are not found as a consequence of strangulation, but there may be apoplectic clots in the thickness of the lung tissue, together with extravasation or infiltration of blood, varying in size, but always longer and more extensive than in suffocation.

The state of the heart presents nothing essential; there is no extravasation of blood, either under the pericardium or endocardium. The heart is sometimes empty, but there is generally found a little black fluid blood.

I have only once, if ever, found some semi-coagulated blood. The brain does not present any constant or precise characters in strangulation: it is found free from change as often as congested. The state of this organ differs remarkably from what is observed after hanging, where obstruction of the vessels of the brain is as frequent as it is rare in strangulation.

The signs of incomplete or attempted strangulation are thus described: the face is swollen, violet coloured, marbled, spotted with red, and livid mucus escapes from the nostrils and mouth; the eyes are bloodshot, and there is ecchymosis under the conjunctiva; the neck is swollen and painful, the voice is broken; deglutition is very painful; the swelling extends to the entire cervical region, and to the lower part of the jaw; it is diffuse and accompanied by an ecchymotic colouring of the skin. Sometimes the swelling of the face and of the neck is considerable, and these parts are of a bluish colour nearly throughout. In some cases the mark of the fingers is visible; more so than in complete strangulation, for the simple reason that, while life continues, ecchymoses have time to appear, and to render the mark more and more apparent.

Incomplete strangulation may produce a loss of consciousness for some hours, but in all cases there remains for some time an obstruction in the throat, great difficulty in speaking and swallowing, and various nervous disturbances. Thus violent compression of the neck and contusions of the cellular tissue and muscles may produce inflammation and even abscess.—*Ann. d'Hygiène publique*, tome xi. p. 106.

Supposed Death from a Blow on the Abdomen.—Dr. Seward, Assistant-Surgeon of the Native Veteran Battalion, Bombay, reports, in reply to the following letter of inquiry as to the cause of death of Turff Palwan:—The Police Carbaree of Moujay Seersodee, Turff Palwan, was sleeping in his own house on the 10th of June, 1857. His two sons were lying by him. On a sudden he cried out loudly; he felt that two blows had been struck on his abdomen. From that time his abdomen swelled and became painful. He could no longer defæcate. He died on the next morning. The people believed his death to have arisen from a natural cause; this, however, being very doubtful, the body was sent for inspection.

It was further gathered from the attendants, that a light was burning in the room; that the door was closed; that the deceased was strongly impressed with the idea that a man had inflicted these two blows; that he was greatly alarmed; that he afterwards vomited water, then blood. The main incident in this history seemed to point clearly to a rupture of some abdominal viscus: the autopsy was undertaken in full anticipation of lighting upon some "solution of continuity." The body was brought for examination on the fifth morning after death—vocant with flies—swollen and bloated with gases of advanced decomposition. On cutting the abdominal parieties, the intestines protruding with great force, altered the position of the viscera. Maggots swarmed in the peritoneal sac. The liver appeared to be unusually small, spleen-like in colour, soft, and easily lacerable. The spleen was flattened and expanded. The stages of decomposition going on hand in hand with great compression, to which these viscera were subjected by the accumulating gases, might have had much to do with the apparent smallness of the liver. Fæces, semifluid, were found in the pelvic cavity inside the sac of the peritonæum. No blood was evident. The stomach was greatly dilated. Its inner coat mammilated, or thrown up here and there in nipple-shaped eminences. These eminences were deemed results of decomposition, their contents, gas, ripping the mucous coat from its attached surface. On cutting them open they were found to be empty, nor did they collapse. The areolar submucous tissue in one case appeared to be thickened, existing as a glistening circular band or bundle. If these were so, post-mortem change would fail to account for this stage. About eight inches from the duodenum was the jejunum completely rent asunder. Carefully removing that portion of the bowel contiguous with the torn parts, the following description was at once recorded:—

1. The peritoneal coat of the portions of bowel, to the extent of about five inches, is covered with patches of lymph, easily separable in flakes. No

lymph is observed on the mesentery. The general surface is pale, mottled, or striped with dark red in the course of the veins (which are gorged with black blood); red, of a lighter hue, colouring the mesenteric border. At the point of disunion the mesentery is torn to the extent of a quarter of an inch.

2. This portion has evidently been the seat of acute disease, as shown by its deep, blackish red aspect. The rent was possibly the result of peristaltic motion prior to death, or more probably was effected by the rough handling of the corpse during its carriage, or during forcible protrusion of bowel-contents (by compressed gases) on opening the abdomen.

3. On one side of the rent, redness extended an inch from the line of lesion, and merged suddenly into a deep blackish spot. The serous membrane was irregularly torn and retracted from the edge of the rent. The muscular tissue was dark and thickened for one-third of an inch across the bowel at this edge of the tear, and thence a red line extended around the gut at extreme limit of ruptured serous coat. The serous coat of the second portion was also irregularly torn and retracted. Corresponding to the black spot already noticed was a small, round, shallow concave depression (an ulcer apparently?), and seemingly situate in the subserous tissue. In a line with this, on the circumference, and towards the free edge of the tube, was a hole in base of a small triangular flap of serous coat, which would be applied to the side of a tongue of the same coat on other half. Next again to this, was a thickened dark spot in middle coat, and to this the submucous tissue, if not the mucous coat of a valvula conniventis, was adherent. Its size was that of a dhall grain. The mucous membrane appeared to be unaffected. The valvulæ conniventes of the duodenal portion were stained with yellow bile. The other detached portion of tube was perfectly free from stain, and of the usual colour. The contents of the upper part had escaped into the abdomen, the remainder consisting of soft fæcal matter of a deep brown colour, with altered particles of dhall and ooreed, the lower containing grey chyle.

4. The retraction of the serous membrane, the thickened everted edges of the rent, exclusive of the thickening, the result of disease, would indicate that the rupture happened during life, or certainly before the tissues had lost their contractility.

Medical opinion.—On June 15th, the fifth morning after death, Dr. Seward examined the body reported to be that of Police Carbaree Ramjee Morey, of Moujay Seersodee, Turff Palwan, and found that the intestinal tube near the stomach was divided and broken asunder. At the place of rupture disease had existed and did exist at the time when deceased was taken ill, sufficient to permit the tearing asunder of the bowel by its own action. He (Dr. Seward) was of opinion that death was brought about by a natural cause—namely, the breaking asunder of the bowel weakened by disease.—*Transactions of the Medical and Physical Society of Bombay*, 1857-8.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By Edward H. Sieveking, M.D.,
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I. Diseases of the Thoracic Organs.

I. *On Thrombosis of the Ductus Arteriosus.* By C. Rauchfuss, Physician to the Foundling Hospital, St. Petersburg. (Archiv für Pathologische Anatomie, Band xiii. Heft 5 and 6.)

This paper is written to show that Virchow's doctrine of thrombosis serves to explain a condition which Billard has termed aneurism of the ductus arteriosus.

The author relates seven cases which he observed in the St. Petersburg Foundling Hospital, and in several of which portions of the plug that had formed in the ductus arteriosus were carried into distant vessels, so as to constitute what Virchow has termed embolia. The following is an abridgment of one of the cases :—A female infant, ill-developed and ill-fed, attacked with diarrhœa, was seized with severe dyspnœa and cyanosis, lasting above twenty-four hours, and died when nineteen days old. On removing the anterior thoracic parietes, the right lung appeared much collapsed, especially its upper lobe. Two-thirds of the right pleural cavity contained air, and at its base was an accumulation of about three drachms of liquid, dark, viscid blood. On the surface of the right upper lobe was a laceration of the size of a bean, surrounded by ragged margins; it led into a hemorrhagic spot occupying one half of this lobe, in the centre of which was a large cavity surrounded by ragged walls, which were infiltrated with blood; the cavity was partly filled with blood and broken-down tissue. The remainder of the lobe was congested, but without apoplectic spots. At the apex of the right lower lobe, near the pulmonary hilus, was a hemorrhagic spot of the size of an almond. The congestion of this lung depended mainly upon repletion of the larger pulmonary veins. In the left lung only the larger pulmonary veins contained much blood. The heart was normal, its coronary vessels gorged with blood. The ductus arteriosus, near the aorta, was considerably dilated, and filled throughout with a firm brown coagulum or thrombus; this was drawn out to a thread, and, passing through the pulmonary opening of the ductus, was continued in the form of a large, firm brown thrombus into the right pulmonary artery, which it entirely occluded. It also extended into the two chief divisions of the artery; the part occupying the upper division presenting a jagged, fibrillating appearance, owing to portions having been broken off and carried into the smaller branches of the upper lobe; no such emboli could be detected in the middle or lower lobes. At the aortic opening of the ductus arteriosus the thrombus extended into the aorta, so as to occupy about two-thirds of the calibre of the artery; it was tolerably adherent to the artery, firm, and of a brown colour; its left end presented a rough, excavated, fractured surface. Liver and spleen normal. The upper half of the right kidney, which showed ecchymoses on the surface, was turgid, and of a darker colour; the pyramids were partly hyperæmic, partly almost black from hemorrhage. The cortical substance was similarly, but less affected. In the subdivisions of the right renal artery connected with these parts four small fibrinous plugs were found, loosely impacted. No other emboli were found.

The second case was one of erysipelas of a new-born child, which died on the tenth day. There was thrombosis of the ductus arteriosus, with embolia of the pulmonary artery in the upper lobe, and corresponding hemorrhage into the pulmonary parenchyma; formation of plugs in the umbilical and portal veins; pericarditis and perisplenitis; catarrh of the intestine; and multiple synovitis.

The third case is termed one of pyæmia in a new-born child, which died at the age of one month. The umbilical vein contained a puriform yellow liquid; near the vena portæ it contained yellow, cheesy, friable masses, which also filled both branches of the vena portæ, and extended into their subdivisions. Nothing is said in this case about the ductus arteriosus.

For the remaining cases and the author's arguments, we must refer to the original.

II. *Lecture on the Causes and Treatment of Pulmonary Tuberculosis.* By M. Gueneau de Mussy. (L'Union Méd., No. 138, 1859.)

In this lecture the Professor considers the question of the contagiousness of phthisis. He repudiates the doctrine of Morgagni, who believed in the direct contagiousness of the disease to such an extent as scarcely ever to have made an autopsy of a phthisical subject himself, and to object to their being made, on account of the danger accruing to the operator. Still, Dr. De Mussy holds that phthisis is transmissible by cohabitation, by great and protracted intimacy during all the stages of the disease. He has long seen grounds for adopting this view; having met with robust subjects whose breadth of chest attested the vigour of their respiratory organs, while they presented no hereditary taint; but who, having lived a life of great intimacy with phthisical subjects, became themselves tuberculous. The author quotes Andral and Morton in support of his views. Parenthetically he observes that angina glandulosa* frequently accompanies phthisis, and often precedes it; he has often met with it in tuberculous families apart from all symptoms of pulmonary affection, and in individuals who did not become tuberculous; and he has also often met with it in persons who lived with phthisical subjects. He infers that the catarrhal element of phthisis may be transmitted by contagion, independently of its specific cause, "as one admits that chance may produce by contagion a simple catarrhal affection, without virulence or specific character." "If," he subsequently remarks, "angina glandulosa may really result from cohabitation with a phthisical individual, one can explain how it should induce a morbid irritation in the respiratory passages, which in certain cases would greatly favour the development of the diathetic element."

Dr. De Mussy's experience leads him to believe that the transmission of phthisis more frequently takes place from the husband to the wife than from the wife to the husband. Assuming this to be the case, he explains it in this way: the female who conceives by a phthisical husband contains in herself an embryo predisposed to tubercle; she carries, as it were, the diathesis in her womb; and, moreover, the devotion of the woman exposes her more and more continuously to contagion than the male sex. Dr. De Mussy thinks that the experiments that have been made with reference to the inoculation of tubercular matter have not led to any definite result; but he regards it as a curious coincidence that Laennec, who affirmed tubercle not to be inoculable, from failing to produce tubercle in himself by inoculation, should have evidently died of phthisis; as Alibert and Biett, who maintained exactly the same doctrine with regard to carcinoma, and performed similar experiments upon themselves, should have died of carcinomatous affections.

This is not the place to enter into a controversy on the questions suggested by Dr. De Mussy, but we cannot refrain from expressing an opinion that the laws which govern the development of disease, physical and moral, would suffice to explain satisfactorily all the apparent cases of tubercular contagion, without reference to this doctrine. We have, however, thought it right to place Dr. De Mussy's views before our readers, because emanating from an eminent French physician, and because the subject has not been mooted of late.

III. *On the Comparative Results of the Treatment of Croup by Tracheotomy and by Medication during the years* 1854–58. By Dr. Barthez. (Gazette Hebdomadaire, Dec. 2nd, 1859.)

In a letter addressed to Dr. Rilliet, Dr. Barthez inquires into the causes which may account for the varying results obtained by the medical and surgical treatment of croup. He seeks to determine the conditions which in each case influenced these results, so as to arrive at a safe basis upon which to decide upon the value of tracheotomy. During the first year after the Hôpital St. Eugénie was opened, 13 cases were submitted to tracheotomy. The first died during the operation, and successively eleven others died after operation; it

* See a review on Dr. De Mussy's work on this subject in the British and Foreign Medico-Chirurgical Review, vol. xx. p. 143.

was not till the thirteenth that a cure was obtained. On the other hand, four patients who were not operated upon recovered. The fatality of the operative proceedings now induced Dr. Barthez to be more sparing of the knife; but still the ensuing year brought a great fatality—of 18 patients only four recovered. Two had been tracheotomized. But at this period French physicians began to distinguish between simple and infectious croup or diphtheria, and Dr. Barthez arrived at the conclusion that the disease (croup and diphtheria being employed synonymously) was the result of an intoxication giving rise to two forms of morbid action—the one local, pseudo-membranous; the other, general or infectious. The author now considered the operation inadmissible in the form that was primarily malignant, on account of its rapid progress; while he held that it should be done where the disease was slow in its progress, and, although severe, only induced asphyxia slowly. The asphyxia ought to be combated by operation, whatever the previous health of the child. At this time he laid down the following rules: 1. To try internal remedies, which had been most successful; 2. To perform tracheotomy at an advanced period, when the former method had evidently failed; 3. To operate also, however unfavourable the age and prior health of the patient, if there were evidence of impending asphyxia.

This method was not followed by great success during 1856, for among 18 cases there were only four recoveries, of which three had been tracheotomized. During 1857, however, the author's views appeared justified by his results, for among 33 cases (*croups*) there were nine cures, seven of which were without operation. Two of these were fortunate enough to have been brought out for operation but to have been sent back for a further trial of internal treatment. Of 23 who were operated upon but two recovered; however, the author expresses himself well satisfied with this result (*je le trouvai très beau*), for the two patients were snatched from certain death by the operation. The general results obtained during the course of 1858 were as follows: Total number operated upon, 124, with a mortality of 106, or 1 recovery in 6·9; total number not operated upon, 62, with a mortality of 26, or 1 recovery in 2·4. With regard to the character of the disease during this year, the author remarks that it presented successively all the forms of diphtheria; the recoveries accumulated at certain periods, while at others the number of deaths was terrible. From the 1st of January to the 3rd of June the non-infectious form, accompanied by slow and feeble intoxication, prevailed. The false membranes descended to the small bronchi; tracheotomy, which was almost invariably performed, scarcely yielded one recovery in 6 or 7 cases; and the recoveries nearly all took place in February. During June the epidemic almost ceased, to recover its virulence toward the end of July. During August it was so severe that of 12 children none recovered by the operation. In September and October there were four recoveries among 14 patients; then, after a respite of a few days, the epidemic appears more severe than ever, and with great efforts but one cure is achieved among 14 patients.

The author concludes from the preceding facts that it is not the treatment which determines the results, but that we must seek for the causes of the variations observed regarding them in the varying forms of the epidemic.

Although Dr. Barthez regards the two varieties of diphtheria as "the expression of the same affection," which often renders it "difficult to establish a distinction at the bedside," he considers it necessary to make the distinction, because the results of the treatment are so different, according as the diphtheria is local or general. In a subsequent part of the paper, though admitting the doubtfulness of the statistics, he states that of 55 patients in whom the disease put on the general type, only 7 recovered, or about 1 in 8; while of 64 patients where the disease was of the local character, 27 recovered, or 1 in 2·3.

The following are the characters which Dr. Barthez regards as distinctive of the two forms of the disease:

1. When the false membranes extend in a continuous layer over the palate, uvula, tonsils, so as to spread into the respiratory passages, he considers generalization of the disease certain; and still more so if the nasal fossæ are attacked. On the other hand, the absence of coryza, the limitation of the false membranes—i.e., their exclusive formation in the larynx, or their slight extension to the tonsils only, are the features which appear to indicate the absence of general intoxication.

2. The grey, greyish black colour, the gangrenous appearance of the false membranes visible on direct inspection, the discharge of a blackish liquid and ichorous blood on the slightest touch of the throat, are certain signs of intoxication; while the grey-yellow, and above all the white tint, of the false membrane indicates local disease.

Diphtheritic intoxication is further characterized by marked tumefaction of the cervical ganglia of a painful character, especially if accompanied by swelling of the adjoining cellular tissue; by the production of false membranes on the skin when deprived of its epidermis, and on wounds; by gangrene of the mucous membranes of the skin or of wounds; by abundant albuminuria, which is independent of all other causes which may produce it (upon this symptom the author was "imperfectly informed" up to the end of 1858); by the leaden, and not purple, hue; the smallness and feebleness of the pulse; the exhaustion apart from the asphyxia, or out of proportion to the apparent asphyxia. Finally, diphtheritic intoxication is characterized by consecutive paralysis, either limited to the pharynx or general.

II. Diseases of the Abdominal Organs.

1. *Acute Yellow Atrophy of the Liver in an Infant.* (Quoted from Jahrb. für Kinderheilkunde ii., 1, p. 42, in Schmidt's Jahrb., Band cv. p. 71.)

A girl, aged twenty-one months, was admitted into the Children's Hospital at Vienna in a moribund condition, having for eight days been affected with icterus. The skin was intensely yellow, the cheeks and lips cyanotic, the abdomen distended with gas; there were convulsions in the upper extremities and of the maxillary muscles, breathing stertorous, coma. Death ensued three hours after admission. Autopsy: The body was well developed and plump; skin and conjunctivæ intensely yellow; both pupils dilated; spumous, serous fluid in the oral cavity; the abdomen tympanitic. The cranium was compact; the dura mater firmly adherent and yellow; the sinuses full of dirty red blood. The inner meninges were also congested; the cerebral tissue very soft, œdematous; icteric. The lateral ventricles, of normal size, containing a small quantity of clear serum. The thymus was above 2" long by $1\frac{1}{2}$", bi-lobar, and dense. The bronchial glands were normal; the lungs healthy, except that there were numerous emphysematous patches under the pleura pulmonalis. The bronchi contained spumous, and occasionally sanguineous, serum. The heart and pericardium were normal. The liver occupied the cavity of the diaphragm, being covered in front by the transverse colon, and being reduced to about half its normal size, only weighing 4 oz. 6 dr. The capsule was delicately corrugated; the free margin translucent, flabby; the parenchyma tough; the portal vessels dilated. On section, numerous smaller and larger dense spots of uniform yellow tint were noticed, in which small vessels were visible. Professor Wedl, who instituted the microscopic examination, pronounced the hepatic cells to be everywhere destroyed and replaced by molecular detritus. The gall-bladder was small, containing mucus and thin bile; the ductus choledochus and hepaticus patulous. The spleen was not softened, but swollen. The stomach was empty; the small intestines contained food, gases, and a lumbricus; the large intestines were full of firm, clayey, whitish fæces. The kidneys, with exception of a considerable dilatation of the pelvis and calices on the right side, presented no abnormity.

It is observed that, as there was no important complication, the suddenness of death could only be explained by the interruption to the functions of the nervous system by the diseased state of the blood; it is also remarked that this case disproves what has been stated by Buhl, that the affection of the liver is the result of general atrophy.

2. *On the Differential Diagnosis of Abdominal Tumours and Ovarian Cysts.* By Boinet, Member of the Society of Surgery. (Gazette Hebdomadaire, Jan. 6, 1860.)

We refer to this paper because, incidentally, the author mentions a case in which he, by mistake, injected the iodized fluid intended for the radical cure of ovarian cysts into the peritoneum, with the effect of producing a radical cure of aseites, which, he states, had been confounded with ovarian dropsy.

A young lady, of about thirty years of age, came to M. Boinet to be cured of an ovarian cyst, because she had heard of his iodine injections. She objected to an examination, because she had been well assured of the fact of her disease being ovarian by two eminent hospital physicians, and an appointment was accordingly made for the operation. Entirely trusting to the diagnosis of his learned *confrères*, the author, assisted by Dr. Delarue, and without even attempting a verification of the previous diagnosis, made a puncture, and evacuated above twenty litres (forty-two pints) of serum. He then made the injection; but at the first introduction of the iodine the patient uttered so piercing a cry, and felt such intense pain, that M. Boinet at once felt assured that he had injected the peritoneal cavity. Universal purulent peritonitis resulted, placing the patient in imminent danger. Still, after much anxiety, and repeated punctures made for the purpose of evacuating the pus from the peritoneum, a radical cure followed. The patient is now (six years after the operation) enjoying excellent health.

3. *A Case of Pancreatitis.* By Dr. C. Haller, with remarks by Dr. Klob. (Zeitschrift der Gesellsschaft. der Aerzte, No. 37, 1859.)

T. G., aged sixty-three, had always enjoyed good health until sixteen days before admission into the Vienna Hospital, when he was seized with difficulty of digestion, inability to bear food, and vomiting. When admitted he looked cachectic and pale, but showed no perceptible emaciation. Intellect clear, no headache; tongue with a yellow fur, rather dry; loss of appetite, no great thirst, the neck thin, the cervical glands not infiltrated; respiration unembarrassed, no cough, thorax well formed, percussion normal, breathing here and there coarse; the heart normal; the epigastrium was somewhat distended, and tympanitic and tender; the liver and spleen not enlarged; the abdomen moderately distended; œdema of both lower extremities; vomited matter yellow, thin, and bitter; pulse 90. The pain gradually increased, radiating from the epigastrium over the whole abdomen; the vomited matters streaked with blood; the vomiting gave no relief: sudden collapse, and death took place the day after admission, the vomiting having been arrested for some time previously. The cranial and thoracic cavities presented no abnormity of importance; the liver was of normal size and anæmic; the gall-bladder and ducts healthy; the spleen small, and its capsule furrowed. The stomach was collapsed, and contained a thin, turbid, brownish fluid. The mucous coat was somewhat tumefied; translucent posteriorly, where the superficial veins shone through as dark-brown streaks. The posterior wall presented three circular openings of the size of lentils, forming funnel-shaped perforations from without inwards through all the ventricular coats. The loss on the peritoneal surface was the largest; the margins of these openings were extremely friable and discoloured. Behind the stomach lay a large ichorous abscess, extending to the spinal column posteriorly, to the spleen on the left, to the pylorus on the right; within it lay the pancreas, which was exposed on all sides, and appeared as a greyish, discoloured, thin, extremely friable, sanious cord. The splenic artery and vein ran along its upper margin. The former was moderately full of fluid blood; the latter was blocked up by a recent, darkened fibrinous plug, which adhered to the coats of the vein, and extended as far as the junction, with the vena portæ on the one side, and on the other to the hilus of the spleen. The retro-peritoneal glands were scarcely swollen, pale red, and moist. The intestines, kidneys, and supra-renal capsules presented nothing remarkable. Under the microscope the pancreas appeared in a state of disorganization; where the cells were recognisable they were tumefied and turbid; the acini that remained were straggling, partly collapsed, and with fine granular molecules, and large oil-globules intervening. There were also the elements of the exudation, pus-corpuscles, and nuclei, partly preserved, partly in a state of decomposition.

Dr. Klob has no doubt that the case is one of primary inflammation of the pancreas, and that the morbid products gave rise to perforation of the stomach. He observes that inflammation of the pancreas, like the inflammatory affections of other salivary glands, commonly begins in the interstial areolar tissue, and thus induces small scattered abscesses; but that, as in the instance detailed, the inflammation may primarily affect the acini, and thus induce general suppuration of the organ. He states that this is also the view of Rokitansky on the ease in question.

4. *A Case of Argyria, with Deposit of Silver in the Intestines, Liver, Kidneys, and Spleen.* By Dr. C. Frommann. (Archiv für Patholog. Anat., Band xvii. Hefte 1 and 2.)

The subject of this paper, W. Jordan, aged sixty, was attacked in March, 1856, with epilepsy, the fits occurring three to four times daily during the first month, when they lasted an hour at a time; they subsequently became less frequent, and at the end of the year they occurred once a fortnight, and only lasted about a quarter of an hour. Almost from the commencement of the disease, nitrate of silver was exhibited, and for nine months he took a daily pill containing six grains, so that altogether he swallowed about 3½ ounces. Towards the end of July the skin began to be discoloured, gastritic symptoms supervened, but still the remedy was persevered with. In the beginning of 1857, there was hæmatemesis and other undoubted symptoms of gastric ulceration, and scarce any food could be borne. He recovered so far as to undertake a voyage to England, but the fatigue proved too much for him, and on his arrival he was compelled to seek aid at the German Hospital. On his discharge he was able to take food well, but his circumstances being very bad, he had a relapse, and was again admitted into the German Hospital on the 6th November, 1858, in a wretched condition, severe cough and hemoptysis having supervened. The whole surface exhibited a steel-grey colour, which was particularly marked in the face. There was in addition to the gastritis, advanced tuberculosis, bronchitis, and pneumonia. The patient died two days after admission. The following is an abridged account of the autopsy:—The parts in the face which had exhibited a great intensity of discoloration, owing to their containing more blood, now presented a tint uniform with the rest. In the brain the choroid plexuses presented an uniform greyish-blue tint. The state of the lungs corresponded with what had been observed during life; the left ventricle of the heart was much hypertrophied. The stomach contained a large quantity of acid brown liquid, streaked with blood; the mucous membrane was covered with a considerable layer of dirty red, viscid mucus, inclosing streaks of black coagulated blood. The vessels were much injected, and there were numerous small extravasations. At the upper part of the posterior wall, half way between the pylorus and cardiac orifice, was a large ulcer, seven centimetres by five (2·75 × 1·96 inches), at the basis of which there was an orifice of the size of a crown-piece, which was blocked up by the pancreas, to which adhesions had formed. The pylorus formed an annular stricture, only large enough to permit the passage of a common lead pencil. The mucous membrane of the duodenum and jejunum was dotted over with many small black granules, most closely aggregated along the folds. In the ilium these spots become more and more scanty; examined by the microscope, the villi in these black spots presented, especially in their globular end, groups of black aggregated particles, varying much in form and size, and without a crystalline character; cyanide of potassium rapidly dissolved these deposits here as well as in the other organs in which they were found. The spleen was small; its veins had an ashen hue, which was due to a fine granular precipitate upon their coats. The liver was small, congested, and fatty; the small branches of the vena portæ and of the hepatic veins presented the same precipitate of silver throughout, but the capillaries were free from it. Fine sections of the hepatic tissue showed numerous black dots, each of which occupied the centre of an acinus, corresponding to the point of exit of a central vein, and the colour was produced by a black margin surrounding the calibre of the artery. The dark colour of the branches of the vena portæ was also very characteristic throughout The largest argentean deposit was in the kidneys, where the bundles of vessels in the Malpighian corpuscles and the intertubular capillaries seemed to be its primary seat. The pyramids all exhibited a dark grey colour, which was deepest and all but black near the papillæ. The tubules in these parts were entirely invested with a dense precipitate; so that on a transverse section each tubule appeared surrounded by a black ring. Parts of the skin taken from the temporal, axillary, and digital regions were examined. Transverse sections showed a pale, purplish streak immediately underneath the rete Malpighi, following the undulations of the cutis. At the roots of the hairs it accompanied the external sheath towards the bulb, but nowhere except in the sudoriparous glands was a granular deposit to be found; in them it presented an appearance similar to that seen in the renal tubules. The glandular epithelium uniformly presented fatty degeneration.

We may mention that concentrated sulphuric acid, as well as cyanide of potassium, dissolved the argentean deposit; though the latter did so with the greatest rapidity. Portions of the liver and kidneys analysed by Dr. Versmann afforded the following result: 217 grains of dried liver yielded 0·009 grammes of chloride of silver, or 0·0068 grammes of metallic silver, or 0·047 per cent. of metallic silver; 133 grains of dried kidney yielded 0·007 grammes of chloride of silver, or 0·0053 grammes of silver, or 0·061 per cent. of the latter.

5. *Contributions to the Pathology of Diabetes Mellitus.* By RUD. LEUBUSCHER. (Archiv für Pathol. Anat., Band xviii., Hefte 1 and 2.)

The following are the results obtained by Professor Leubuscher and Dr. Passauer from a series of experiments on the influence of various articles of diet in a case of diabetes mellitus upon the secretion of sugar, urea, and chloride of sodium, and upon other circumstances:

1. The temperature of the skin was throughout below the average; it was generally only 35° C., and even under the influence of an acute affection, which

ultimately caused the patient's death, it did not exceed 36° C. Diet appeared to influence the variations less than the temperature of the room.

2. The quantity of urine did not correspond to the quantity of liquid drunk, but exceeded it materially. On one day when six ounces of red wine, three ounces of rectified spirits, and 1000 cubic centimetres of water were taken, the quantity of urine amounted to 3300 cubic centimetres, or less than usual. The tendency of the patients to deceive in these matters renders the fact less trustworthy.

3. With a mixed diet and preponderance of starchy diet with much water, the quantity of chloride of sodium and urea secreted exceeded the average materially.

4. A preponderance of animal food increases the quantity of urea secreted, and diminishes the amount of sugar without material alteration of the chloride of sodium.

5. A free use of milk with mixed diet produces no change in the relative proportions of the sugar, chloride of sodium, and urea.

6. Alcoholic beverages, with chiefly proteinaceous diet, increases the secretion of sugar materially and diminishes the urea, the chloride of sodium remaining unaltered.

7. Iron given in the form of the lactate from the middle of November to the middle of December, in doses of four to six grains, left the sugar at the average quantity, produced no effect upon the urea or chloride of sodium, the patient feeling generally well.

8. Pepsin taken from the middle of December to the beginning of January, twice daily, in ten grain doses, diminished the quantity of urine; the specific gravity rose to 1·044; all the constituents, sugar as well as urea and chloride of sodium, were relatively as well as absolutely increased. The general health of the patient continued good.

9. Benzoin, in the form of benzoic acid, benzoate of soda, or ammonia, taken for a month daily in doses of six to eight grains, produced no material influence.

Acute symptoms of pulmonary disease, gastritis, and enteritis, resulting from a cold, supervened, during which albumen appeared in the urine, and the patient died comatose on the fourth day of the attack. From the post-mortem account we merely extract the remarks that the vessels of the meninges, and especially the longitudinal sinus, contained an emulsified liquid in which were red coagula. A close examination of the blood exhibited much free fat and white blood-corpuscles, with a considerable quantity of sugar.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *Account of the Results of Amputations observed at Constantinople during the Crimean War.* By M. Salleron. (Recueil de Mémoires de Médecine et de Chirurgie Militaires, deuxième série, tome xxii., pp. 262-420.)

This is another testimony to the enormous sufferings of the French army during the Crimean war, and a silent protest against the disparaging comparisons heretofore instituted between it and the British army. M. Salleron was in charge of the Dolma-Bagtche Hospital at Constantinople, which he represents as of faulty construction and defective in hygienic appliances. Any mischief which would have resulted from these circumstances alone was augmented by the unavoidable over-crowding of its wards with the wounded soldiers.

The immediate object of the author's paper is to give an account of the amputations performed and treated under these painful circumstances, and especially to point out the greater amount of mortality that attended secondary amputations. He selects the period from the 1st of May to the 1st of November, 1855, as being that during which the comparisons he desires to institute may be best made. During it, 2753 gunshot-wounds were admitted, of which more than one-half were of a very severe character. Of the 2753 patients, 2009 were either discharged, or more often transferred to France for ulterior treatment, and 744 died. After great engagements, the subjects of amputation were usually evacuated upon Constantinople as soon as possible, arriving there three or four days after the performance of the operations, with the stumps, as regards dressing, bandages, and cleanliness, in a most unsatisfactory condition. These operations and those performed in the trenches are entered in the hospital registers as immediate amputations, the secondary ones being those performed afterwards at the hospital itself. The bulk of these latter were also performed from five to ten days after the accident. The total number of amputations was 639—i.e., 490 amputations in continuity and 149 disarticulations. Of the 639, 419 were primary operations, furnishing 221 recoveries and 198 deaths; and 220 were secondary operations, furnishing 73 recoveries and 147 deaths. Thus, among the 639 cases there were 294 recoveries and 345 deaths, the primary operations yielding more than a half of cures, and the secondary operations yielding but a third.

M. Salleron next examines into the immediate causes of this great mortality after amputation—a result so opposite to that which he and the other French surgeons had been accustomed to in Algeria, where amputations succeed very well. Omitting causes which only operated on a few cases, we find that of the 345 deaths, 65 resulted from gangrene with emphysema, 45 from hospital gangrene, and 228 from purulent infection.

Gangrene with emphysema.—The author met with gangrene under two forms—the *œdematous* or mild form, and what he terms the *emphysematous or instantaneous* form. No case of the former proved fatal, but rapid death occurred in 65 instances of the latter; 46 of these had been amputations in continuity, and 19 disarticulations. Among 220 amputations performed in the hospital, 36 cases of this form of gangrene occurred, while among 419 performed in the Crimea, only 29 cases occurred. Those about to be attacked seldom properly rallied after the operation, and were the subjects of great nervous irritation. The attack itself was quite sudden, the limb became rapidly and immensely distended, and soon after blackened, the general symptoms undergoing frightful aggravation. It was not, indeed, peculiar to persons who had been operated upon, as it proved in some of those suffering from wounds rapidly fatal. The progress of the disease was always rapid and continuous, no kind of temporary suspension of its course ever being observed, and its mean duration in the 65 cases was from twenty-five to thirty hours. The chief feature was an enormous emphysematous distension, which induced compression of the deep-seated veins. The superficial veins were distended with gaseous fluid, which also separated the fibres of the muscles from each other. These last were pale, but not disorganized. The patient always died, a state of indifference or stupor coming on, and all remedies proving useless. Perhaps the affection should rather be called *emphysema of the stump* than gangrene, for there was not the disorganization of tissues met with in ordinary gangrene—on the contrary, they remained distinct and recognisable, and preserved their consistency, relations, and organization.

Hospital gangrene.—Besides the well-known ulcerative and pultaceous forms of the disease, the author met with a small number of examples of another form, hitherto unknown to him, and which he designates as *caseous*, which attacked stumps nearly healed. The lower angle of the stump became violaceous and engorged, and a small excavation formed, which soon filled with matter of a sebaceous consistency and of a greyish colour. This constantly increased in quantity as the excavation, which was lined by a soft membrane, rapidly augmented in size. The progress of the affection was at once arrested, while it yet seemed local in its operation, by the actual cautery or nitric acid. Hospital gangrene in the two other forms affected many patients besides those who had undergone amputation; and the author regards it as a manifestation of a general pathological rather than a local condition, the air-passages being the ordinary vehicle of its transmission. He found local treatment of little or no avail, unless the overcrowding could be diminished and ventilation secured, which under the circumstances was rarely possible. Of the great number of local applications tried, the *perchloride of iron* succeeded best. In 30 cases, where all other treatment seemed unavailing, amputation was performed, 14 of the patients dying, and 16 recovering. In none of the 30 cases was there relapse of the gangrene, nor did one of them die of the immediate effects of the operation.

Purulent infection.—This prevailed in the Constantinople hospitals from the period of the battle of the Alma to the end of the campaign, and proved the principal cause of death after wounds and operations. It especially manifested itself in the case of osseous lesions, however slight these might be. Fractures of the shafts of the long bones were always rapidly followed by pyæmia, rendering any subsequent operation useless and mischievous, inasmuch as this but accelerated the progress of the general affection, as of 490 amputations performed in continuity, 192 terminated fatally, while but 32 of 49 disarticulations exhibited a like issue. The author has found no description of treatment useful, and recommends only that symptoms should be combated as they manifest themselves.

II. *On Chronic Enlargement of the Lymphatic Glands as a Symptom of Secondary Syphilis.* By PROFESSOR SIGMUND, Vienna. (Wien. Medizin. Wochenschrift, 1859, Nos. 22, 23, 25.)

Swelling of the glands placed nearest to the primary ulcers may be observed even within the first week after the infection, and by six weeks even the most distant glands may have become affected, although they may not be sufficiently indurated to be plainly felt until after the eighth week. In only very few instances is there any considerable pain, and in the great majority of cases the occurrence passes unperceived either by patient or practitioner. The size the glands may reach varies in different persons, and bears no relation to the condition of their health, the inguinal and cervical regions being those in which the largest are met with. The hardness of the gland, slight at first, goes on increasing, until sometimes it becomes almost cartilaginoid. The intervening lymphatic vessels do not become much swollen, except in the inguinal region, and even there the cord-like enlargements afterwards diminish, or nearly disappear. When the course of the syphilis is undisturbed by treatment, the swelling and hardness of the glands progressively increase, but eventually the swelling abates, so that the gland even undergoes at last contraction, but retains all the morbid induration, which indeed may persist for the rest of life. The glands continue moveable, as does the skin covering them. It is very rare for suppuration to take place, and when it does occur, the pus is not inoculable.

The author passes in review the various affections of the glandular system which may be mistaken for this one, and admits that in scrofula and tuberculosis, a condition much resembling the syphilitic glands may arise, so that it may be impossible to pronounce an opinion at once from mere objective examination; while the fact of scrofulous or tuberculous subjects becoming syphilitic may also give rise to the true nature of the glandular affection being overlooked. The peculiarity of the syphilitic affection is that it commences nearest the primary sore, and spreads hence gradually and constantly to the more distant glands, pursuing its course without exciting inflammatory action, but inducing an induration which may become permanent. This mode of its production, joined to the general prevalence of the affection, is usually quite sufficient to establish the diagnosis.

Enlargement of the glands precedes the secondary affections of the skin and mucous membranes, persists during the appearance of these and all subsequent symptoms, and still remains when all other subjective and objective symptoms of syphilis have ceased to appear. It is, too, the earliest sign of hereditary syphilis. This chronic enlargement of the glands is observable at all periods of life; a new-born child accidentally poisoned during its passage into the world will exhibit it, as well as the aged subject of syphilis. Neither sex, mode of life, nor race exerts any influence on the manifestation. In subjects who have become affected without having had primary symptoms, as nurses or sucklings, the glandular affection is still gradually developed, and precedes other symptoms of secondary syphilis. So, too, the new-born infant who has contracted the syphilis in utero exhibits the same enlargement, although this is not so easily detected as at a later age. Autopsies, however, completely prove the exactitude of the above statement. The bodily constitution of individuals exerts no marked effect on the fact of the production of the enlargement, although it may influence its amount.

With respect to the treatment of syphilis, this may be purely local, as long as the development of the general affection of the glandular system has not commenced; while, after this period, the author prefers the employment of mercurial ointment to all other means. Although the indurated glands rarely return to their normal state, this may happen in strong and otherwise healthy subjects; while in all, their diseased condition diminishes in proportion to the success of the treatment.

III. *A rare Form of Fracture of the Jaw treated by a Novel Method.* By Dr. Fountain. (New York Journal of Medicine, Jan., 1860, p. 140.)

W. G., aged forty-two, fell from a height, striking his chin against a piece of timber, and fracturing his jaw through the body on each side, and through the neck of the condyle on the left side. The jaw was displaced backwards, and laterally on the left side—a displacement which was temporarily rectified as long as traction was made at the symphysis, which the connexions of the middle fragment with the membranous and muscular tissues permitted. As soon as this traction was removed, the lateral deformity was reproduced, and every contrivance resorted to failed to maintain a permanent reduction of the fracture of the neck. After trying all these methods during a week, the author contrived the following means of establishing permanent traction forwards, which should keep the parts in apposition. Holes were drilled through a front incisor of each jaw, and a double strand of fine annealed jeweller's iron wire was passed through and twisted so as to keep the parts in exact apposition, the central fracture, which gave no trouble, being supported by a pasteboard splint. In ten days the wires gave way, and a cord was inserted composed of four of the same wires; and in this way the jaw was held securely and immovably until all the fractures were united—viz., four weeks, during which time the patient was nourished by liquids, which were easily drawn into the mouth through the teeth. Perfect union, without a particle of deformity, took place, and now, nearly four years after, no one would be able to tell that any fracture had ever taken place.

IV. *On a Modification in the Treatment of Hydrocele.* By M. VOILLEMIER. (L'Union Médicale, 1859. Nos. 125–128.)

M. Voillemier recommends the following supplementary procedure in the treatment of hydrocele by iodine injection, having found it prevent the delay and relapse not infrequently met with in voluminous hydrocele. The object is to diminish the amount of secondary secretion, not by applying compression, but by affording firm support to the scrotum.

After the injection has been evacuated, he passes three or four strips of diachylon, each two centimetres in breadth, beneath the scrotum, their length being sufficient to admit of their ends being crossed above the pubis. The crossing must not take place too near the base of the penis, or œdema will be produced there. The base of the scrotum is circularly surrounded by other strips, to prevent the testes rising towards the rings as much as possible. The strapping thus far applied forms a framework to give support to other shorter strips which are extended from the perineum to the base of the penis, and complete the covering of the scrotum. Triple or quadruple layers of such strips should be applied, and then, by means of a warm hand, converted into a homogeneous covering, or thick carapace. A suspensory bandage is then put on, and, if the patient is not still in pain from the injection, he may be allowed to walk about. On the second or third day the strapping is removed, and generally a swelling is observed at the bottom of the scrotum, which, at first, is supposed to be due to the presence of the testis. It is, however, only œdema, doubtless due to fluid secreted in the tunica vaginalis, and which, unable to distend this, owing to the resistance offered, has passed through the wound made by the trocar, and become infiltrated into the cellular tissue. Generally, very little fluid, or even none at all, is found in the serous cavity, the tissues presenting a doughy feeling, as if containing a soft, plastic mass. The testis on the affected side has mounted up towards the ring in spite of the diachylon, and is usually found somewhat large and tender, but much less so than after the ordinary operation. The diachylon should be renewed every forty-eight hours, and at the end of eight or ten days the patient is cured; although, as a matter of precaution, he must continue to wear a suspensory bandage.

The chief advantage of this mode of treatment is, that its duration is one-half less than that of the ordinary iodine treatment; while the patient is at once enabled to get up, and in some instances even to walk about. M. Voillemier employs as material for injection the undiluted tincture of iodine, to which a little iodide of potassium is added; and he allows the injection to remain in three or four minutes. Where there is reason to anticipate little susceptibility to inflammation he leaves a little of it in, as under the influence of the passive compression exerted by the plaster, an insufficient rather than too great an amount of inflammation is to be feared.

V. *On Galvanism as a Remedy for the Photophobia of Strumous Ophthalmia.* By Dr. A. HEWSON. (American Journal of Medical Science, Jan., 1860, pp. 114–119.)

Dr. Hewson has found galvanism localized to the supra-orbital branch of the fifth pair the most efficient means of treatment he has yet employed in this troublesome affection. It is the direct current of galvanism or chemical electricity which he employs, the induced current of the magneto-electrical apparatus not having been attended with the same satisfactory results, while its application gives rise to great pain. The direct current, when properly localized, only gives rise to a faint quivering or flashing of light, more amusing to the child than otherwise.

"The form of apparatus employed by me was Pulvermacher's chain-battery of sixty links, with vinegar as the chemical agent. The electricity from this I applied by moist conductors, which consisted of brass rods six inches long, slightly curved, and surmounted at one end by a wooden handle, and at the other by a small brass cup filled with wet sponge. These conductors were hooked to the ends of the chain which constitute the poles of the battery, and I applied the sponge of the one attached to the negative pole to the skin over the supra-orbital foramen, while I made frequent contacts with the sponge of the other to the skin of the forehead at various points. These applications were generally made at intervals of three or four days, and only for a minute or two each time: and I would caution all who may resort to galvanism for these purposes, not to attempt its too frequent or protracted use, or to use it in too great strength, for there is danger of permanent injury to the retina from such uses of it, as has been pointed out by M. Duchenne. It may, however, be used with safety more frequently than above stated. I have myself used it in private practice every day. My reasons for employing Pulvermacher's battery were that (notwithstanding its well-known want of constancy of action), the facility of its application, and its comparative cleanliness, made it far preferable for my clinical purposes to any other apparatus known to me."

VI. *On the Forced Taxis in Strangulated Hernia.* By M. GOSSELIN. (Gazette Hebdomadaire, Nos. 44 and 46, 1859.)

M. Gosselin is of opinion that forced taxis is too sparingly resorted to. During the last fifteen years 85 cases of strangulated hernia have come under his care, and of this number 29 have been treated exclusively by the taxis, this having been in the great majority of the cases forcible and prolonged from thirty to sixty minutes. Two of these patients died (one from persistence of strangulation and refusal of operation, and the other from the return of a perforated intestine), and 27 were relieved, while, had they been operated upon, some of their number would certainly have died. In none of the 27 cases was the strangulation of old date, having existed for three days at farthest in 17 cases of inguinal, for thirty-six hours in 7 of femoral, and twenty-four hours in 3 cases of umbilical hernia. It is true that cases operated upon were also first submitted to the taxis, which failed; but in ignorance of the prior history of many of these cases, or the long duration of the strangulation in others of them, the forced taxis was not resorted to in such. It is to be observed, too, that this is longer applicable in inguinal than in femoral hernia, as gangrene and perforation are of more rapid occurrence and of more frequent production in the latter.

As to the procedure, commencing with gentle pressure, if at the end of four or five minutes this is not found sufficient, it is gradually increased, until at last the whole strength of the operator's two hands is exerted on the tumour, the manœuvre being so directed as to return first the parts which have last escaped. Generally the operator adds a portion of the weight of his body to the pressure made, and when he is fatigued he causes an assistant to apply his hands over his own and press also, or one or two fresh assistants replace him. M. Gosselin does not resort to preparatory measures, such as baths, cataplasms, &c., not having much faith in their efficacy, and believing that loss of time in employing the taxis should before all things be avoided. The forced taxis, performed under the influence of chloroform, is infinitely more successful than when this agent is not resorted to, one chief reason being the great amount of pressure which then can be borne. Without the aid of chloroform few surgeons would have the courage to inflict the suffering induced by forced taxis.

One important circumstance as a result of forced taxis must be borne in mind—viz., apparent reduction, the tumour being so reduced in size as to give rise to the belief that the intestine has been returned, some omentum alone remaining. The previous history of the case, as regards the prior reducibility of the tumour and the amount of persistent tension, must be our chief guides as to whether strangulation still persists, requiring the operation; and in doubtful cases a purgative may assist the diagnosis. On the other hand, a thickened state of the surrounding parts, the consequence of inflammation or fatty deposition, may lead the young practitioner to believe that reduction which is real has not taken place. The diminution in size and the absence of tension are the chief diagnostic signs. As to the objection which has been raised to the forced taxis, that in the event of its not succeeding and the operation becoming necessary, the patient is exposed to more danger in consequence of the contusion the parts have undergone, it is not confirmed by M. Gosselin's observation. In the cases of 6 patients upon whom it had been practised from thirty to forty-five minutes, and who were subsequently operated upon, 1 death and 5 recoveries took place. It is true that, as is the author's practice, the operation was performed immediately after the taxis had failed, so as not to give time for inflammation to become developed as a consequence of the contusion. A more temporizing procedure would probably be attended with less favourable results.

VII. *On In-growing Toe-nail.* By Drs. PORTER and LORINSER.

Dr. Porter (American Journal of Med. Science, Jan., p. 124) describes the treatment of this affection as it occurred in his own person. He dwells upon the following three points: 1. The importance of removing all pressure from the affected part, which in his own case was accomplished by the following procedure. "After a loose stocking had been drawn on the foot, a friend was requested—having pulled its extreme end forward—to stitch a seam partly across it in front of all the toes except the large one, thereby hanging or resting the stocking on the second and third toes, leaving the affected phalanx in a sulcus." A circular piece of leather from a partly worn boot was also removed directly over the affected nail. 2. The nail is not incurvated, as it is sometimes said to be, the disease being really in the soft parts and induced by pressure. If the corner of the nail can be skilfully removed, the suffering ceases, when the inflammation is but slight, or even when suppuration has occurred, provided that all future pressure be prevented —the affection, in the author's opinion, being indeed originally produced by the pressure of too short a boot. 3. When granulations conceal the edge of the nail which is irritating the swollen integuments, fomentations and poultices must be applied to allay the inflammation, in order to admit of the removal of a portion of the nail. When this end is not attained, the knife or caustic must be resorted to, and in the author's case both had to be employed, for he had let the malady gain a considerable height before he yielded to it. The cure was accomplished in the end by taking off all pressure through the contrivance in the stocking above described, and pencilling the granulations with saturated solution of alum, which was applied as hot as it could be borne, this increasing its efficacy. Small pledgets of lint wetted with it were left on for twenty-four hours, covered with oiled silk. Dry lint was next substituted, wet and dry applications alternating. Under this treatment, which was not painful, the granulations flaked off or were absorbed, and at the end of three days the macerated corner of the nail was easily removed. The hot saturated solution of alum is an admirable application for granulations or ulcers about the nails, the salt being deposited on cooling in a state of impalpable powder.

Dr. Lorinser (Boston Journal, Dec. 15th, from the Oesterr. Zeitschrift für

Heilkunde) takes a different view of this affection to that usually held. Examining it at an early stage, pressing the granulations to one side with a flat probe, the edge of the nail is found to be undermined for a short distance, and is hollow, and the granulations which furnish the pus exist also beneath it, and on the lateral border of its matrix. "Since the ulcerated portion of the matrix is covered by the nail, so that only that portion of the pus next to the edge can be pressed out, while the rest remains collected underneath, the ulceration assumes all the characters of a *fistula*, and if left undisturbed can only with difficulty, or not at all, be cured, because the conditions under which the healing of a fistula is usually accomplished are wanting. Then, again, the sharp edge of the nail is pressed by treading against the granulations which surround it acting as a foreign body and increasing the growth of these. But if this pressure of the nail existed alone, without the fistulous condition below just alluded to, prolonged rest would be sufficient to produce a cure, which is not the case. Moreover, at first the nail has generally undergone no change in shape, and the affection may exist with a completely normal one, while persons having very distorted nails may never suffer from it. Again, the agency of the shoe is doubtful in many cases. After describing the further progress of the affection, Dr. Lorinser concludes—1. That the affection originates in an inflammation and ulceration of a part of the matrix, and that paronychia maligna is nothing more than such ulceration of greater extent and more rapidly destructive; 2. That the names onyxis, in-growing nail, onychia, are wholly inappropriate and confusing, since the nail does not grow into the skin, but the granulations springing from the skin and from the matrix cover over and embrace the edge of the nail, without the shape of the latter being necessarily abnormal.

In accordance with these views, the treatment should not be directed so much to the nail as to the fistulous ulceration beneath the matrix, the indication being to expose this, so as to convert it into an open wound, prevent the accumulation of pus, and promote cicatrization.

1. At an early stage of the affection the exposure of the ulceration may be effected by inserting a small pledget of lint between the edge of the nail and the granulations, and then destroying these with nitrate of silver. The pledget should at first consist only of a few threads of lint, not longer than the edge of the nail, pushed carefully and deeply in with a flat probe. It should be changed at least twice a day, its thickness being gradually increased. If there is much tenderness, cold compresses should be applied over the lint, and when the suppuration is abundant, a cold foot-bath should be employed before the dressing. After a few days, the edge of the nail having become free, the pledget can be pushed under the undermined portion, so as to absorb the pus as secreted, prevent the springing up of granulations, and hinder the contact of the nail with surrounding parts. In this way, in the milder forms, healthy granulations form at the bottom of the ulceration, and as the pus escapes freely, healing soon follows. When, however, the granulations have formed into a high ridge, partly covered with skin, and the edge of the nail is soft and deeply undermined, the fungous growth must be seized with a forceps, and enough of it removed by a crescent-shaped incision to expose the edge of the nail, the soft and ragged portion of which is to be cut away as far as possible without injury to the matrix, thus laying open the ulceration of the latter. Lint may now be pushed under any overhanging portion of the nail as before.

2. When the edge of the nail is still undermined, and there is a disposition to the production of flabby granulations, sheet lead may be advantageously applied.

QUARTERLY REPORT ON MIDWIFERY.

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I. The Unimpregnated State.

The Uterine Dilator. By Horatio R. Storer, M.D. (Amer. Journ. of Med. Science, July, 1859.)

Dr. Storer advocates fluid pressure as the best means of artificially dilating the cervix uteri; and describes an apparatus he has contrived for the purpose. The instrument consists of three portions: a distensible sac, made in preference of goldbeater's skin, the dilating medium; a hollow staff for support, and as a channel of communication to and from the sac; and an external source of supply, for which Higginson's elastic pump is best adapted. As the membranous sac becomes distended with water, it assumes first a fusiform, then a globular shape. An advantage insisted upon by Dr. Storer is, that the pressure operates, *more Naturæ*, from above downwards. He relates a case in which the cervix was by this means rapidly dilated; reflex action was remarkably excited, especially by suddenly evacuating the sac. He also makes the incidental observation that, upon presenting the central mass of the elastic pump to the stethoscope, the continuity of the column of water remaining unbroken by the stopcock, the sounds of the fœtal heart were rendered more distinctly audible. He remarks that this is a further development of the principle of fœtal auscultation described by Dr. Keiller, of Edinburgh. [It may be said, in like manner, that the plan of dilating the cervix by fluid pressure is a further development of the colpeurynter of Braun.—Rep.]

II. Labour.

1. *Cases of Obstructed Labour:—1. From extensive Hard Cicatrix of the Vulva. 2. From Malformation of the Pelvis.* By L. J. Martin. (Australian Med. Journ., Oct., 1859.)
2. *On the Use of the Forceps in Face Presentations.* By Dr. Von Helly, Teacher of Obstetrics at the University of Prague. (Vierteljahrschr., 1859.)
3. *Experience in Face Presentations.* By Spaeth. (Œst. Zeitschr. f. Prakt. Heilkunde, Nos. 26, 27, 1859.)
4. *Observations on External Turning.* By Dr. Carl Esterle. (Schmidt's Jahrb., No. 10, 1859.)
5. *Placental Adhesion as a Cause of Hour-glass Contraction and Protracted Labours.* By John Martin, M.D., New York. (Amer. Med. Monthly, Oct., 1859.)

1. Mr. Martin, of Melbourne, describes a case of obstructed labour from occlusion of the vagina, which had occurred after destruction of the labia, clitoris, &c., by a burn when the patient was a child. The vulva was occupied by a very dense and extensive cicatrix, the only opening through which was a small ring-like aperture, admitting the tip of the little finger, and situated in the posterior part of the cicatrix close to the anus. Division of the cicatrix was performed, and closure counteracted by bougies. She became pregnant. There was great obstruction when the head came to the vulva; incisions were made and delivery effected.

Case 2.—We refer to the second case on account of the recovery of the child after injury during birth. The antero-posterior diameter was not more than three inches. The head presented in first position. Under chloroform the long forceps was applied. Great and protracted efforts were made to extract. The child, a female, when born, showed scarcely any sign of life; the head exhibited an extensive radiated and depressed fracture of the left parietal bone, near the anterior fontanelle, which had evidently been caused by the pressure of the projecting bone as the head was drawn through the brim. By the "ready method" and otherwise the child was restored, and a month afterwards was alive and well. The fractured surface appeared to be gradually rising.

[This case is especially interesting, as illustrating the extent of compression and even of violent injury from which a child may recover. It is an encouragement to resort to the long forceps or turning in preference to craniotomy in cases of contracted pelvis. Contrary to the opinion of the author, we believe that turning in this case would have been attended with less injury to the skull.—REP.]

2. Dr. Von Helly presents a valuable analysis of the mechanism and treatment of face presentations. Starting from the familiar fact, that these are more tedious than labours in which the vertex presents, he says the reason lies in the circumference with which the head enters the pelvis, and in the unusual relations which the peculiar position of the fœtus induces. The head of a fœtus born by the vertex, is lengthened in the longest or diagonal diameter—i.e., from chin to vertex; the vertex is the highest point, towards which the roof of the skull forms a gradually inclined plane from the forehead. The diagonal diameter surpasses the straight one, from forehead to vertex, by an inch, so that the two diameters form two lines which, when the head is looked at in profile, form an irregular triangle. The occiput of a head born by face-presentation appears drawn out or lengthened in the direction of the straight diameter; the roof is but slightly arched, is flat, and ends in a sharper angle at the forehead. The difference between the straight and diagonal diameters disappears, so that the two lines, one drawn from forehead to vertex, the other from chin to vertex, form a nearly isosceles triangle. Measurements have been made in reference to this point in 32 cases; these give:—

The straight	diameter was	longer than	the diagonal in	2	cases.
"	"	equal to	"	12	"
"	"	¼" shorter	"	13	"
"	"	½" "	"	3	"
"	"	1" "	"	2	"

The head finds, from the arching of the roof and occiput towards the opposing side of the pelvis, an obstruction to its descent, whence, through protracted uterine contractions, the neck is more stretched, the occiput approaches the back, and the forehead from having been the lowest part is drawn back. When the skull is flattened, and the head has in this manner lost in height, its vertical diameter decreases in length, and so finds room in the pelvic brim, the chin sinking backwards to be on same level as the forehead. When the face approaches the outlet, the chin immediately leaves the side of the pelvis, draws forward near the symphysis, and the neck places itself against the posterior surface of the anterior wall of the pelvis. Most frequently this change from the diagonal to the antero-posterior diameter is effected at the floor of the pelvis. The skull thus enters the cavity of the sacrum; the chin is gradually driven forward under the symphysis pubis, and the face becomes visible between the labia pudendi. Forehead, roof, and occiput roll over the perinæum, whilst the head, by revolving on its horizontal axis, is brought nearer to the breast.

Dr. Von Helly cites the well-known experience of L. J. Böer, as proving the efficacy of nature in bringing these cases to an end; and says, that in 58 cases which have occurred in the last few years in the Prague Lying-in Hospital, perforation was performed twice under urgent circumstances, the child being dead, and in two instances the forceps was used.

Dr. Von Helly deprecates attempts to alter the presentation by changing the face for the occiput, or by turning. In the 58 cases of the Prague Hospital there was a proportion of 18-19 per cent. of dead-born children, calculated in this wise: 2 were delivered after perforation, 1 was born putrid; these three being subtracted, there remained 55 births. Of these 10 gave dead children. The cause of this unfavourable result to the child lies in the compression which the skull and brain undergo; in the obstruction to the circulation of the brain, caused by the diminution of the calibre of the vessels of the neck under the great stretching produced; and, above all, by the long continuance of these dangerous conditions occasioned by the unusual protraction of the labour. Injury of the spinal marrow he looks upon as theoretical, and says he has found few opportunities of observing in the autopsies cerebral apoplexy, although there may be congestion of the brain and membranes.

Before the dilatation of the os uteri, the author deprecates interference. In cases where the necessity for aid arises, and the os is open, the question, he says, is in what relation the forceps is to be applied to the face-presentation, and how it is to be applied so as to entail no bad result for mother or child. The long forceps ought not to be applied when the head is still high; at this stage the circumference and resistance of the head are still great; the operation is very difficult, the prospect of the child's life very small; whilst danger is incurred by the mother from the liability of the instrument to slip. Above the brim the double-curved forceps must be applied in the transverse diameter; one blade will lay on the forehead and crown, but the other can get no secure hold on the face and neck without so compressing the latter part as to destroy it. If urgent circumstances call for delivery when the child is undoubtedly dead, perforation is to be resorted to.

When auscultation declares that the child is alive, nothing but accidents threatening the mother can justify tentative applications of the forceps; and as soon as conviction is obtained that further force is dangerous for the mother, perforation is indicated. The author agrees with Mittermaier and the greater number of obstetric practitioners in deciding in favour of perforation even when the child may still be alive, rather than with those who would wait until the lives of both mother and child are imperilled. But when the face has descended into the lower part of the pelvic cavity, the relations are so changed as to be more favourable for the forceps: one blade can be laid in opposition to the sacro-iliac synchondrosis, the other to the foramen ovale. If the chin be at the symphysis, the application of the forceps is of course still easier.

The following two cases are important:

Case I.—A woman who had borne eight children was in labour on the 9th of September at term; the liquor amnii had escaped. Pains first came on next day, weak, and rare. Accustomed to quick labours, and getting anxious, she pressed the midwife to apply the forceps. This was done on the 11th, and abandoned after fruitless attempts. Another and a third attempt was made on the following day by several physicians, which were equally fruitless; and the patient was brought to hospital. The countenance was blanched, the features sunk, extremities cold, pulse scarcely felt, abdomen painful and meteoric, uterus unevenly distended, the lips of the os uteri swollen, hanging flaccid in the vagina. The head was in the brim, face presenting in the transverse diameter, the forehead to the right and lower down. The presenting eye was hanging out of its socket; the epidermis came off the face in shreds. The patient was a little revived from her state of exhaustion by hot wine and musk. The trepan-perforator was applied, and a large putrid child extracted by forceps. On removing the placenta, the previously diagnosed rent in the uterus was felt. Death took place in the evening. Section revealed a rent in the fore and left side of the cervix, and a conjugate diameter of 3″ 11‴.

CASE. II.—A woman who had borne four living children naturally was in labour at term. A surgeon called in, applied forceps an hour after escape of waters, and as this slipped, tried to turn. Flooding appeared, and the patient was brought to hospital. The pulse was small, quick; abdomen distended with gas; uterus contracted on its contents; genitals swollen. In the vagina was the right foot and right arm. Higher up was felt the face on the brim. The head was prevented from descending by the lower extremities being dragged behind the head, whilst the descended arm was hemmed in between the head and the left side of the pelvis. The line of the face lay in transverse diameter, forehead to the right. The presenting arm was replaced with some difficulty, and so much room was gained that the forehead could be perforated by Kiwisch's instrument. The cephalotribe slipped off. By pulling at the foot extraction was at length effected. The fœtus, apparently not long dead, weighed without brain 6¾ pounds Vienna civil weight. The mother collapsed after the operation, and died after four days under symptoms of peritonitis. Section revealed purulent exudation in great quantity, covering the peritoneum, and here and there between the lamellæ blood-effusion. The iliac part of the peritoneum was torn through; gangrenous endo-metritis; left half of cervical canal torn through, the rent gaping, and opening into abdominal cavity.

[This practical essay has been analysed in some detail, because the prevalent doctrine—although in the main correct—that no aid is required in face-presentations, is apt to conceal the great difficulties that occasionally arise. Our own experience agrees with the author's as to the inapplicability of the forceps when the face has not yet entered the pelvic cavity; but we see no valid reason why turning should not be resorted to where the child is alive, and delivery is indicated.—REP.]

3. Dr. Spaeth's experience in face-presentations may be usefully given in illustration of the foregoing. He found it occur seven times in 14,424 cases. At first the head is mostly in transverse direction, the greater fontanelle being to right or left. The further mechanism of labour always proceeded in like manner—namely, the head turned in the pelvic cavity with the face forwards, and at the outlet the upper lip was fixed against the symphysis; the occiput then rolled over the perineum, when at last the mouth and chin emerged from under the symphysis. The configuration of the head was always the same, the forehead strongly projected forwards; the sides compressed unsymmetrically, so that the side which was turned to the symphysis during the passage through the brim appeared concave in the direction from above downwards. Of seven children, three only were saved; in two cases perforation was necessary—one was dead-born, one died quickly after birth. One mother died of metrophlebitis, which set in during labour. The rest recovered; but three suffered from gangrenous ulceration, the result of the pressure during labour; two of these suffered in addition from endo-metritis.

The case should be left as long as possible to nature. If forceps become necessary whilst head is still high, it must be applied in transverse direction. When the case is complicated with moderate conjugate contraction, 3¼ to 3½″, Spaeth advises delivery by turning.

4. Professor Esterle gives an elaborate exposition of the subject of external turning. In some of the applications of the operation his views and experience are extremely interesting. He commences by citing the opinions of Wigand, Matteo, and Stoltz; and then considers the conditions which favour spon-

taneous evolution, and the manner of operating. The patient is to lay so as to relax the abdominal muscles as much as possible. The method pursued by nature in spontaneous version is to be closely followed. The partial peripheral contractions of the uterus which diminish the transverse diameter are to be replaced by lateral compressions, which must gradually merge into a compressing stroke on one side near the fundus, on the other near the os uteri. This is much aided by gentle blows applied alternately to the apices of the ovum. These blows may be repeated more quickly at a later period by acting on the one side upon the head, whilst with the other hand a fixed point is maintained upon the opposite extremity. This will not always succeed in removing the head at the first attempt. When effected, the longitudinal position is to be maintained. This is to be done by the recumbent posture and bandages. The author's observations were made upon 500 pregnant women, who were mostly examined in the seventh and eighth months. Amongst these were detected 22 complete transverse presentations. Nine were rectified spontaneously; in 10 external turning was carried out; in 2, turning by the head was effected by combined internal and external manipulations; and in 1 case internal turning brought down the breech. In the 10 cases of external turning, placenta prævia was present once; pelvic contraction of the first degree twice; excessive obliquity of the uterus three times. In some instances the cause of the cross-presentations was probably strong compression of the abdomen for the purpose of concealing pregnancy; and perhaps also the weight of the clothes, which, as countrywomen wear them, presses almost entirely on the fundus uteri.

We give three of his cases as examples of the practice:

Case 1.—A woman, pregnant thirty-four weeks; head high to left; back forwards, breech towards right ilium, liquor amnii copious. It was sought to bring the head down, which was so far successful that it was brought within reach; but as soon as the pressure was removed it receded. After two unsuccessful attempts to retain the head, the breech was brought down. This was effected without much difficulty, and was maintained. On repeated examinations, the longitudinal direction of the fœtus, with the breech presenting, was constantly verified. Twelve days after the last examination labour began; the occiput was presenting in first position, and the labour proceeded normally.

Case 2.—Pregnancy near termination. Twins diagnosed; one fœtus in first head-position, and one in cross-presentation, head to the left, back backwards. During labour the first fœtus had to be delivered by forceps. External turning was resorted to to rectify position of the second. The head was easily brought down, and whilst assistants by external pressure maintained it *in situ*, the membranes were ruptured, and the head was born in the first position.

Case 3.—A woman, pregnant thirty-two weeks. Head high to the right; back backwards. External turning proved very difficult and tedious; but by persevering especially with the methodical strokes, it succeeded in placing the head in the second position. Upon this the customary bandage was applied, and recumbency on the right side prescribed. After a short time, however, the woman was unable to bear the bandage, or to lay in the position ordered. When examination was made four days after, the head had moved, but not quite back to its original place; it had passed over to the opposite side, over the left crista iliaca. The causes of this side movement were the great quantity of liquor amnii, the great flaccidity of the uterine walls, and above all, a marked inclination of the uterus to the right, in consequence of which the breech, which had been brought to the fundus, continued to move in the direction of this inclination, and was supported by the laying on the right side, as ordered. External turning was performed a second time successfully.

[The chief field for the useful application of this proceeding appears to consist in the rectification of cross-presentations during the latter weeks of gestation. It is, of course, a substitute for the more severe and hazardous operation of internal turning. It deserves more attention than it appears to have received in this country.—REP.]

5. Dr. Martin, of New York, discusses minutely the influence of placental adhesion as a cause of hour-glass contraction and protracted labour; and offers some conclusions of interest as connected with the diagnosis of placental adhesion. He rests his views upon a pathological basis, but does not adduce any clinical observations precisely illustrating his hypothesis. He refers to the opinions of Dr. Barnes, "that in true placental adhesion there is organic change of structure, or abnormal deposit in the decidual portion of the placenta; while in more frequent cases the adhesion is merely the result of ineffectual contractions of the uterus;" and of Dr. Simpson, who ascribes adhesion to inflammation producing effusion of lymph. He says: "Whatever may be the pathological condition of the adherent placenta, it is evident that the connexion must be between its surface and the uterine muscles. For there can be no morbid condition of the circulation on the part of the uterus; no local error of secretion in the uterine membrane; no abnormal accumulations about the fœtal tufts. Because the firmest adhesion is found in the healthiest females, carrying to the full period of gestation the largest and best developed infants. [These statements are certainly not in accordance with our own precise experience.—REP.] It is also well known that in such cases the muscles in connexion with the adhering placenta readily contract when the separation is made. It follows that, during the entire process of parturition, the muscles of a portion of the organ are firmly held by a completely adherent after-birth, in the same expanded state in which they were when labour commenced."

Dr. Martin thus sums up his conclusions:

First. Placental adhesion can be diagnosticated by auscultation, and by attention to the form of the uterine tumour, and to the action of the circular fundal muscles. If after repeated trials no change can be perceived in the distinctness and loudness of the placental souffle; if the muscles of the fundus do not act at the beginning of each pain, and contract regularly throughout its continuance; if during pain the uterine tumour does not present a globular form, but is more prominent at the place of placental attachment; if the pains be irregular, it will be safe to conclude that the placenta is adhering to the walls of the uterus.

III. THE OVARY.

1. *On Ovarian Gestation.* By Professor ARTHUR WILLIGK. (Vierteljahrsch. f. d. Prakt. Heilkunde, 1859.)
2. *On Abnormities of the Corpus Luteum.* By ROKITANSKY. (Allg. Wiener Zeitung, Nos. 34, 35, 1859.)

1. Should the views supported by Professor Willigk be confirmed, the opinions current concerning ovarian gestation must undergo considerable modification. In any case they are valuable as suggesting the necessity for more minute investigation and description of cases. He adopts the views enunciated by M. Mayer in 1845, who called in question the existence of ovarian gestation. Professor Willigk criticises a recent alleged case of ovarian gestation, published by M. Alquié, who describes an instance in which "ten embryo-sacs, with fat, skin, hairs, cartilages, and bones, were found," and concludes that there was here a tenfold impregnation of the ovary and intra-ovarian gestation. The improbability of the impregnation of ten ova at once is urged against the admission of this case. In many others he urges that the microscopic determination of the parts has been neglected, leaving it a matter

of doubt whether what was described as chorion or fœtus was done so correctly. Thus he examined a preparation, labelled ovarian gestation, in the Olmütz museum. He found in the right ovary a cavity the size of a hen's egg, to the flocculent lining of which a short thin stalk was attached, holding a body that had been taken for a fœtus of seven weeks. Microscopic examination showed that what had been taken for chorion-villi was nothing but a delicate growth of the connective tissue of the cyst-wall, and the seeming fœtus was a solid mass of connective tissue. So, in another preparation in the same museum, it was found that during the autopsy a separation had been made of adhesions binding the left Fallopian tube with the ovary; a cavity was revealed the size of a walnut: since the inner surface of the cyst was covered with villous excrescences and clots, it was concluded to be the embryo-sac of a fœtus formed in the abdomen. Microscopic examination proved that the villi consisted of the ordinary connective-tissue; and on the other hand, true chorion-villi were discovered in the cavity of the Fallopian tube near its fimbriated extremity, which, with other appearances, fixed this as a case of Fallopian-gestation.

The Professor demands, as a condition for admitting a case as one of ovarian gestation, that the fœtus itself, or undoubted remains of it, the membranes, or the placenta, shall be found within the fibrous capsule of the ovary; and says that, tried in this manner, there is no case that as yet seems to guarantee the existence of this form of extra-uterine gestation. He then analyses several cases of alleged ovarian gestation vouched by various authors, and disposes of one related by Kiwisch by comparing it to the following case examined and figured by himself. It is a preparation in the Olmütz museum. The right Fallopian tube is closed at its free end, and bears traces of adhesions; the right ovary has a scarred surface, and contains several Graafian follicles and corpora lutea; the two leaves of the broad ligament of the left side enclose a round sac, the anterior wall of which shows a wide irregular rent. This sac contains, besides clots, an embryo partly surrounded by the amnion; the inner surface is nearly covered with fine chorion-villi, which at one spot are developed into a placenta. When the posterior fold of the broad ligament is traced backwards, its direct course over the left ovary is seen. This ovary is inseparably united with the abnormal embryo-sac, its surface being marked here and there with threads of connective-tissue. This case is one of gestation between the folds of the broad ligament, and he suggests that Kiwisch's is of the same kind.

2. Rokitansky refers to the two species of corpus luteum, the one occurring in menstruation without conception, the other with conception and pregnancy. The proper characters of the latter consist in long persistence, extending over the period of gestation, in the great extent of the yellow stratum—that is, in its thickness and the depth of its convolutions, in the lesser saturation of the colouring matter, and in the speedy retrogression of the yellow colour of this stratum, whilst it takes on a yellowish-red or reddish colour; in the speedy discoloration of the nuclear mass formed of extravasation, and in the transformation of this into cellular tissue. The yellow layer is an outgrowth of the inner vascular layer of the fibrous coat of the tunica propria of the Graafian follicle. It consists in cells of cellular tissue, of which a certain portion enclose fat granules, and even pass into fatty metamorphosis. In the menstrual corpus luteum this outgrowth is much smaller. The tunica propria growing to the yellow stratum is in the true corpus luteum very highly vascularized during its development and growth; considerable vessels run from it into the sinuses of the yellow stratum. The cause of the folding of the yellow stratum lies most probably in the original unequal outgrowth of the follicle in the form of a papillary and ridge-shaped swelling, upon which the yellow stratum

appears to be drawn in elevations and depressions. The extravasation which forms the nucleus of the corpus luteum, by the above-mentioned speedy absorption of the colouring matter of the blood, grows to be soft, gelatinous, or to a fibrous cellular mass. In the menstrual corpus luteum there commonly remains an indented pigment-mass adhering to a gelatinous substance. Of the true corpus luteum there finally remains a crumpled, white, callous body, filling the retracted follicle, which, when it possesses a cavity, contains in it a gelatinous mass; these are the so-called corpora albida. The menstrual corpus luteum degenerates into a thin-walled capsule, which encloses an orange-yellow, nut-brown, black nucleus, and is soon confounded with the ovarial stroma.

BOOKS RECEIVED FOR REVIEW.

Medico-Chirurgical Transactions. Vol. xiii. London, 1859. pp. 461.

The Mineral Springs of Vichy. By the Author of the Spas of Germany. London, 1859. pp. 243.

Lectures on the Development of the Gravid Uterus. By W. O. Priestley, M.D. London, 1859. pp. 110.

Descriptive Catalogue of the Preparations in the Museum of St. Thomas's Hospital. Printed by order of the Governors. Vol. ii., Part I., pp. 351; Vol. iii., Part II., pp. 350. London, 1859.

Transactions of the Pathological Society of London. Vol. x. 1859. pp. 365.

Kent Lunatic Asylum, Barmingheath, Maidstone. Annual Medical Report. Maidstone, 1859. pp. 21.

Eight Cases of Ovariotomy. By T. Spencer Wells, F.R.C.S. Dublin, 1859. pp. 82. (Reprint.)

Essays on Medical Science. By J. Capper, M.D. Edinburgh, 1859. pp. 103.

On Certain Popular Fallacies concerning the Production of Epidemic Diseases. By D. Noble, M.D. Manchester, 1859. (Reprint from Trans. Manch. Statist. Soc.) pp. 27.

Du Froid Thermomètrique et de ses Relations avec le Froid Physiologique dans les Plaines et sur les Montagnes. Par C. Martins. Montpellier, 1859. pp. 52. (Extrait des Mémoires de l'Academie des Sciences de Montpellier.)

The Physician's, Surgeon's, and General Practitioner's Visiting List for 1860.

The Indian Lancet. Nos. 6, 7.

Religious Revivals, in Relation to Nervous and Mental Diseases. By J. S. Bushnan, M.D. London, 1860. pp. 96.

Sixteenth Annual Report of the Managers of the New York State Lunatic Asylum. Utica, 1859. pp. 47.

The American Journal of Insanity. April, 1859.

Further Researches on the Grey Substance of the Spinal Cord. By J. L. Clarke, Esq., F.R.S. (Phil. Trans. Part I. 1859.)

The North American Medico-Chirurgical Review. November, 1859.

The Journal of Mental Science. Jan., 1860.

On the Injurious Effects of Mercury in the Treatment of Disease. By S. O. Habershon, M.D. London, 1859. pp. 86.

On Jugular Venesection in Asphyxia. By Dr. John Struthers. (Reprint from Edin. Med. Journ.)

The Half-yearly Abstract of the Medical Sciences. Edited by W. H. Ranking, M.D., and C. B. Radcliffe. Vol. xxx.

The Journal of Psychological Medicine. Jan. 1860.

The Retrospect of Medicine. Edited by W. Braithwaite. Vol. xl. July—Dec., 1859.

Edinburgh Veterinary Review. Jan., 1860.

A Manual of Anæsthetics. By C. Kidd, M.D. New Edition. London, 1859. pp. 249.

On Excision of the Knee-Joint. By Oliver Pemberton. (Reprint from Brit. Med. Journ. 1859.)

On the Coagulation of the Blood in the Venous System during Life. By G. M. Humphry, M.D. (From Brit. Med. Journ., with Additions.) Cambridge, 1859.

On Chronic Alcoholic Intoxication. By W. Marcet, M.D. London, 1859.

Observations on the Causes of the Great Mortality in Grenock. By James Wallace, M.D. Grenock, 1860.

The Action and Sounds of the Heart. By G. B. Halford, M.D. London, 1860.

The Anatomy of the Human Lung. By A. T. H. Waters, M.R.C.P. London, 1860.

Stricture of the Urethra, its Complications and Effects. By Robert Wade, F.R.C.S. Fourth Edition. London, 1860. pp. 352.

Clinical Surgery. The Injuries and Diseases of the Nervous System. By Thomas Bryant, F.R.C.S. Part II. London, 1860. pp. 75.

Histoire Littéraire des Fous. Par Octave Delepierre. London, 1860. pp. 184.

On the Intimate Structure and the Distribution of the Bloodvessels in the Human Lung. By A. T. H. Waters, M.R.C.P. (Proc. Roy. Soc. May 26, 1859.)

On the Development of the Female Pelvis. By T. Matthews Duncan, M.D. Edinburgh, 1859.

Cancer Cures and Cancer Curers. By J. Spencer Wells, F.R.C.S. London, 1860. pp. 93.

Brighton Gazette. Thursday, Jan. 19, 1860.

Gustaf von Düben's Treatise on Microscopical Diagnosis. With 71 engravings. Translated by Professor J. Bauer, M.D. New York, 1859. pp. 82.

Notes on Nursing. By Florence Nightingale. London. pp. 79.

Notes on Hospitals. By Florence Nightingale. London, 1859. pp. 108.

The New Sydenham Society. Vol. v. Selected Monographs. 1859. pp. 380.

On the Employment of Trained Nurses among the Labouring Poor. By a Physician. London, 1860. pp. 32.

Foundation for a New Theory and Practice of Medicine. By Thomas Inman, M.D. London, 1860. pp. 374.

Deaconesses for the Church of England; also a paper on the Supervision and Training of Workhouse Girls. By Louisa Twining. London, 1860.

Journal of the Workhouse Visiting Society. No. V. Feb., 1860.

Clinical Lectures on certain Acute Diseases. By R. B. Todd, M.D. London, 1860. pp. 487.

Exposition of a Method of Preserving Vaccine Lymph Fluid and Active. By W. Husband, M.D. London, 1860. pp. 45.

Handbuch der Lehre von den Knochenkrankheiten. Von Dr. E. Gurlt. Erste Lieferung. Frankfurt, 1860. pp. 256.

Grundriss der Physiologie des Menschen. Von Dr. Karl Vierordt. Erste Lieferung. Frankfurt, 1860. pp. 240.

Della Nature e dell' Arte nella Cura del Morbo. Di Giovanni Forbes. Traduzione dall' Inglese sulla seconda Edizione, di Antonio Consentino. In Napoli, 1859.

Introductory Lecture to the Class of the Female Medical College of Pennsylvania. By Anne Preston, M.D. Philadelphia, 1859.

Aspirations from the Inner Spiritual Life. By Henry M'Cormac, M.D. London, 1860. pp. 370.

A Practical Treatise on Fractures and Dislocations. By Frank Hastings Hamilton, M.D. Philadelphia, 1860. pp. 757.

The Transactions of the American Medical Association. Vol. xii. Philadelphia, 1859. pp. 722.

Therapeutics and Materia Medica. By Alfred Stillé, M.D. Vol. i., pp. 813. Vol. ii., pp. 975. Philadelphia, 1860.

The Care of the Sick. By Richard Barwell, F.R.C.S. Third Edition. London, 1860. pp. 126.

On the Organs of the Senses and the Cerebral Faculties connected with them. By W. E. C. Nourse, F.R.C.S. London, 1860. pp. 19.

Traité des Maladies Mentales. Par le Docteur B. A. Morel. Paris, 1860. pp. 866.

La Psychologie Morbide. Par le Docteur J. Moreau (de Tours). Paris, 1859. pp. 576.

On Diseases of the Throat, Epiglottis, and Windpipe. By G. D. Gibb, M.D. London, 1860. pp. 182.

Advice to a Mother on the Management of her Offspring. By Pye Henry Chavasse, F.R.C.S. Fifth Edition. London, 1860. pp. 255.

De l'Hématocèle Rétro-utérine. Par le Docteur Auguste Voisin. Avec une Planche. Paris, 1860. pp. 359.

The Diseases of the Ear; their Nature, Diagnosis, and Treatment. By Joseph Toynbee, F.R.S. London, 1860. pp. 442.

The Acclimation of European Troops for Service in India. By James Bird, M.D. (Reprint from Journal of United Service Institution.)

The Present State of the Medical Profession in Great Britain and Ireland. By W. Dale, M.R.C.S. London, 1860. pp. 70.

Kleinere Mittheilungen von der Medicinischen Abtheilung des Allgemeinen Krankenhauses in Hamburg. Von C. Tüngel, Dr. Hamburg, 1860. pp. 139.

North American Medico-Chirurgical Review. Vol. iv. No. I.

Contributions to the Hygienic Treatment of Paralysis. By M. Roth, M.D. London, 1860. pp. 78.

On the Medical History and Treatment of Diseases of the Teeth and the Adjacent Structures. By B. W. Richardson, M.D. London, pp. 274.

The Causes and Treatment of Imperfect Digestion. By Arthur Leared, M.D. London, 1860. pp. 224.

On Consumption; its True Nature and Successful Treatment. By Godwin Timms, M.D. London, 1860. pp. 412.

The Boston Medical and Surgical Journal. Feb. 2, Feb. 9, Feb. 16, 1860.

Cure of the Sick. By John Spurgin, M.D. London, 1860. pp. 91.

Hemorrhoids and Prolapsus of the Rectum. By Henry Smith, F.R.C.S. Second Edition.

On the Diseases, Injuries, and Malformations of the Rectum and Anus. By T. J. Ashton. Third Edition. London, 1860. pp. 420.

Practical Observations on the Prevention of Consumption. By John Hogg, M.D. London, 1860. pp. 226.

Recherches sur les Kystes Muqueux du Sinus Maxillaire. Par J. A. C. Giraldès. Paris, 1800. pp. 16.

ERRATUM.—In the Jan. No. of this Review, p. 177, *for* Mr. Liston, *read* Mr. Lister.

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